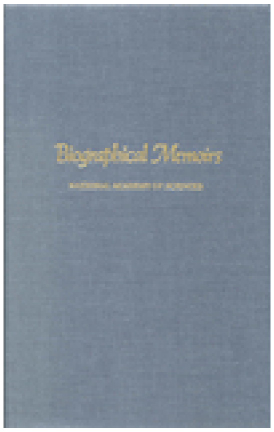

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Biographical Memoirs

VOLUME 76

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PREFACE

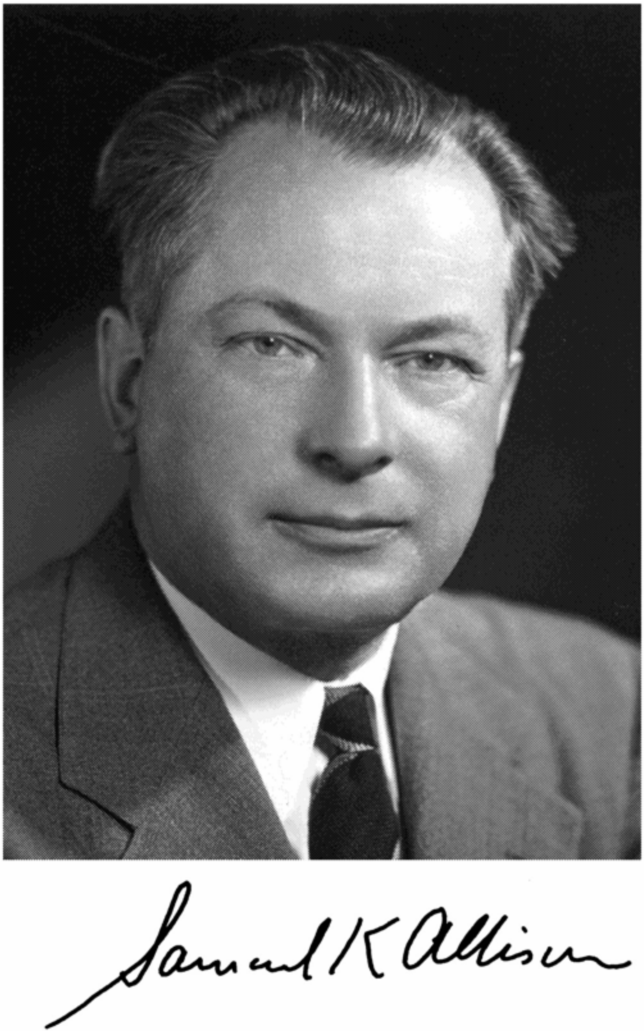
On March 3, 1863, Abraham Lincoln signed the Act of Incorporation that brought the National Academy of Sciences into being. In accordance with that original charter, the Academy is a private, honorary organization of scientists, elected for outstanding contributions to knowledge, who can be called upon 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 76



Courtesy of the Los Alamos National Laboratory

SAMUEL KING ALLISON

November 13, 1900–September 15, 1965

BY ROGER H.HILDEBRAND

SAMUEL K.ALLISON began his professional life at a time of intense interest in the properties and interactions of X rays. His contributions to the field were immediately recognized by the scientific community and especially by A.H. Compton, who was responsible for bringing him back to his alma mater, the University of Chicago. It was also near the time when Cockroft-Walton accelerators and then Van de Graaff machines began producing beams of protons and deuterons. His contributions to nuclear and atomic physics, using these accelerators, were well recognized during his lifetime, but they have grown in significance with the emergence of new fields, especially nuclear astrophysics.

FAMILY AND EARLY YEARS

Allison always regarded himself as a product of the University of Chicago and its surrounding community, Hyde Park. He attended the John Fiske Grammar School and Hyde Park High School. His father Samuel Buell Allison was the principal of an elementary school in the Chicago Public School System. The family owned one of the first automobiles in the neighborhood. When school was out they would drive with their friends to the family summer home near Three Lakes, Wisconsin. There young Sam de veloped a love of the North Woods, which continued throughout his life and led in his adult years to strenuous canoe trips into the Canadian wilderness with friends, including his distinguished colleagues William H.Zachariasen and John H.Williams.

Allison enrolled in the University of Chicago in 1917. As he later reminisced for the benefit of his younger colleagues, it was a time when attendance at chapel was compulsory. He competed on the varsity swimming and water basketball teams while doing honors work in chemistry and mathematics. He was introduced to quantum theory by R.A.Millikan, one of the university's first great teachers, and graduated in 1921. Two years later he received his Ph.D. in chemistry under W.D.Harkins. His dissertation was on "Atomic Stability III, the Effects of Electrical Discharge and High Temperatures."

His performance in Harkin's laboratory earned him an appointment as a National Research fellow at Harvard (1923– 25). From there he went to a fellowship at the Carnegie Institution in Washington (1925–26) and then to a faculty appointment at the University of California, Berkeley, where he advanced from an instructorship to an associate professorship (1926–30). While at Berkeley he married Helen Campbell. Their children Samuel and Catherine were born in Chicago after the family moved permanently to Hyde Park.

X RAYS

Except for a brief introduction to nuclear physics at the Cavendish Laboratory (to be discussed later), Allison's principal research from the time of his graduation until he returned to Chicago in 1935 at the invitation of A.H. Compton was in the properties and interactions of X rays by means of precision spectroscopy. It was a time when X rays were the primary means of studying the atom.

Allison later said that he was hired at Chicago because "the university needed a chemist, I was available, and the records showed that I usually operated well within my breakage allowance." A review by Robert S.Shankland gives a different perspective of Compton's invitation to Allison:

In Professor Wm. Duane's laboratory at Harvard, [Allison] became involved in the famous controversy between Duane and Arthur H.Compton on the validity of the X-ray scattering experiments that were basic for the "Compton effect." Compton's now classic experiments conducted at Washington University in St. Louis had been challenged by several X-ray physicists, including C.G.Barkla and Bergen Davis, but especially by Duane, for they were in conflict with the accepted classical theory of X-ray scattering of Professor Thomson. Duane had interpreted the experiments carried on in collaboration with students in his laboratory as being adequately explained as "tertiary radiation" produced from carbon and oxygen in the box enclosing the X-ray tube by impact of photoelectrons ejected by the primary X rays. Compton, however, had explained his results by the quantum theory— by no means accepted at that time.

When Allison joined Duane's group at Harvard, the experiments were repeated with greater care and precision, and the earlier results were shown to be due to secondary X rays produced by scattering of the primary beam by the walls of the box [1925]. When these definitive results were [obtained], Professor Duane strongly supported Compton's work at the next meeting of the American Physical Society. The close lifelong association of Allison and Arthur Compton began at this time.

The best-known result of the collaboration between Compton and Allison was their book *X Rays in Theory and Experiment* (1935), which served as an authoritative reference for many years. Much of Allison's major work in X rays was facilitated by his design and construction of a high-resolution double-crystal spectrometer. He chose John H. Williams, one of his first students at Berkeley, to be his collaborator in that project. Allison applied the instrument to measurements of unprecedented accuracy of the widths and intensities of X-ray lines. Among the results was his confirmation of the dynamical theory of X-ray diffraction by C.G.Darwin and P.P.Ewing. He provided the crucial measurements and pointed out fundamental errors in earlier theories. He also rendered a physical interpretation to relate the rather complex mathematical treatment to the experimental results.

WARTIME ACTIVITIES

During the war years Allison took on a series of responsibilities. He was a consultant to the National Defense Research Council (October 1940 to January 1941) and then was a member of the Uranium Committee of the Office of Scientific Research and Development (January 1941 to January 1942). In January 1942 he became director of the Chemistry Division of the Metallurgical Laboratory, then chairman of the Project Council, and finally director of the laboratory (June 1943 to November 1944). This was the laboratory that first achieved the controlled release of nuclear energy (December 2, 1942).

Alvin Weinberg, once a student in Allison's class in electricity and magnetism and later director of Oak Ridge National Laboratory, was among the scientists in the Metallurgical Laboratory. At a memorial service for Allison in 1965 he described Allison's work in the laboratory in these words:

Sam Allison's contribution to the controlled release of nuclear energy went much beyond holding people's hands and submerging his own technical aspirations to the interest of his country and of mankind. He did the earliest experiments on the multiplication of neutrons in a beryllium-moderated chain reactor here at Chicago even before the Metallurgical Laboratory was begun. [His relatively small exponential pile came closer to the critical value, $k=1$, than was achieved by the Fermi group, then at Columbia.] This work has remained of fundamental interest, and serves now as the basis for certain major lines of nuclear reactor development both in the United States and abroad. His was the first experimental group at the newly formed Met Lab, and indeed was the nucleus of the wartime lab [around which grew] the final 3,000-man institution.

Weinberg described Allison's administrative burdens in the laboratory as follows:

The laboratory had its giants—Enrico Fermi and Arthur Compton, and Leo Szilard, and Eugene Wigner; it had its pessimists and bureaucrats; and it had a lot of somewhat bewildered young people undertaking their first scientific jobs. It was Sam Allison who, with his extraordinary patience and insight, kept this disparate crew focused on the main job, which was to achieve success ahead of the Nazi competitors.

If the project was faced with a technical crisis, as when the multiplication factor appeared too small to sustain a chain reaction, or when the canning of the uranium slugs seemed to be impossible; or if the project was confronted with a personnel crisis as when the most senior and desperately needed physicist handed in his resignation, it was always Sam Allison upon whom much of the burden fell, and it was he, with his gentle and appropriate humor and technical knowledge who saved the day.

By the end of 1944 the center of activity moved to Los Alamos and Allison was called on to go there as chairman of the Technical and Scheduling Committee (November 1944–January 1946). When the first atomic device was exploded in the desert at Alamogordo, New Mexico, in July 1945, it was Sam Allison's voice that was heard counting down the last seconds before the explosion. That countdown received a great deal of attention in descriptions of the event, and Allison joked that he became famous for his ability to count backwards. In a ceremony at the University of Chicago on January 12, 1946, he was awarded the Medal of Merit by Major General Leslie R.Groves. President Harry S.Truman signed the citation.

POSTWAR SCIENTIFIC LEADERSHIP

The Medal of Merit ceremony marked the end of his official duties at the Metallurgical Laboratory and the beginning of a new phase of public service, administrative accomplishment, and scientific success. He was an eloquent and effective spokesman in the drive for civilian control of atomic energy and a staunch defender of individuals under attack during the “Red scare” led by Senator McCarthy.

Allison became the first director of the Institute for Nuclear Studies (now the Enrico Fermi Institute), a peacetime successor to the Metallurgical Laboratory and among the first interdisciplinary institutes. The Institute for Nuclear Studies was formed on the conviction—inspired by the wartime example—that physicists, chemists, and astrophysicists could benefit by working together. Among the senior members were Enrico Fermi, Willard Libby, Joseph and Maria Mayer, Leo Szilard, Edward Teller, Harold Urey, and later S. Chandesekhar and Gregor Wentzel. The younger faculty included Richard Garwin, Marvin Goldberger, Murray Gell-Mann, Yoichiro Nambu, Eugene Parker, John Simpson, Nathan Sugarman, Anthony Turkevich, and Valentine Telegdi. The students of that era included James Cronin, Jerome Friedman, T.D.Lee, Jack Steinberger, and C.-N.Yang. It was an array of talent seldom, if ever, matched by any laboratory in any decade.

At a luncheon in the Shoreland Hotel announcing the creation of the institute, Allison fired the opening gun in the struggle against continuation of military censorship, when he said, “We are determined to return to free research as before the war. If secrecy is imposed on scientific research in physics, we will find all first-rate scientists working on subjects as innocuous as the colors of butterfly wings.” This speech, delivered at the founding of a prominent institute, caught the attention of a wide audience and was credited with hastening the re-establishment of open scientific inquiry.

NUCLEAR AND ATOMIC PHYSICS

Allison's contributions to nuclear physics began in the mid-1930s while he was visiting the Cavendish Laboratory as a Guggenheim fellow. In a paper presenting the results of his "Experiments on the Efficiencies of Production and the Half-Lives of Radio-Carbon and Radio-Nitrogen," he thanked "Dr. J.D.Cockroft for instruction in the use of the high-voltage apparatus at the Cavendish Laboratory [and] Lord Rutherford for permission to work in the laboratory."

When he returned to Chicago he built his own Cockroft-Walton accelerator in Eckhart Laboratory, home of the Physics Department. He soon had some five students measuring the energies of particles produced in lithium targets bombarded with protons and deuterons. Just as this work was achieving its initial success it was interrupted by war.

When he was free to return to the field, he reconstructed the accelerator in the new Research Institutes Building, which had just been built to house the Institute for Nuclear Studies. He called his accelerator the "kevatron" to emphasize its modest peak energy (400 KeV) at a time when his associates were building machines in the million- and then billion-volt range with names like "cosmotron" and "bevatron." The kevatron stood on the basement floor of the building, extended through a very large hole in the first floor, and reached almost to the level of the second floor. Access to the ion source was by way of a plank thrown across the gaping hole some 10 feet above the basement floor. His students tell of hair-raising adventures in coping with that feature of the laboratory. The high-voltage apparatus was operated from an adjacent room with a haywire but smoothly efficient rig of mirrors, pulleys, and strings culminating in an array of broomsticks—you turned the brooms that pulled the strings that worked the levers that made the beams.

The research had two objectives:the study of low energy nuclear reactions induced by light projectiles (protons, deuterons, helium ions, lithium ions) and the elucidation of the phenomena associated with the interaction of atomic and ionic beams with matter, in particular the energy loss and the capture and loss of electrons by the beam particles. A by-product of the research effort was the development of sophisticated apparatus for the production of monoenergetic beams of particles and for the precise measurement of their energy.

Allison's postwar studies of low-energy nuclear reactions in light nuclei were concerned at first with the energy release as determined by measurement of the kinetic energy of the reaction products. These studies included measurements of the energy levels of unstable reaction products, such as ^{7}Be, ^{13}B, ^{15}C, and ^{17}N. These light nuclei and the reactions leading to their formation later proved to be of great cosmological significance because of their role in the production of stellar energy and in nucleosynthetic processes.

In the kevatron, Allison's projectiles were protons or deuterons; the targets were lithium, beryllium, and boron. The reaction products were studied with his electrostatic or magnetic analyzers. Later, Allison acquired a 2-MeV Van de Graaff accelerator, which he equipped to accelerate lithium ions to energies sufficient to cause nuclear reactions in light nuclei. With his modest apparatus, first the kevatron and then the Van de Graaff, he was an early pioneer in a field of research that would later be known as “heavy ion physics.” His projectiles were too light to qualify as heavy ions by modern standards, but they were heavier than could be found in other laboratories of that era.

Edwin Norbeck, then one of Allison's students, described the venture into lithium projectiles as follows:

By 1953 it was difficult to come up with good nuclear physics experiments that could be done with a low-energy accelerator. I remember a brainstorming session he had arranged to uncover promising projects. The conclusion of the meeting was that any new experiment would be difficult, either because it required high precision, had a low cross-section, or used exotic beams or targets. After this meeting Prof. Allison and I met in his office to discuss the situation. He recalled seeing an article, published many years earlier in *Review of Scientific Instruments,* that described a method for making a beam of lithium ions:

The authors, J.P.Blewett and E.J.Jones, had produced lithium ions by heating the lithium aluminum silicates, spodumene and beta-eucryptite, on a filament of platinum gauze. Eucryptite gave twice as much lithium current as spodumene. Allison contacted friends who were geologists and soon we had some spodumene, a semiprecious jewel, and then some alpha-eucryptite. These natural minerals gave good ion currents, but soon we were making our own beta-eucryptite using separated isotopes.

We put the source in a Van de Graaff accelerator and brought out a 1.2-MeV ^{7}Li beam. This was more difficult than it sounds, but Allison had a good solution to every problem that arose. When the big day came to bring out the beam, we had a variety of detectors. If there were any nuclear reactions at such a low energy we wanted to be sure that we would not miss them. We had a gamma ray detector and a neutron survey meter. We used a thick target of LiF in a chamber with a thin window on one side. Outside the thin window we had a phototube coated on the end with a ZnS phosphor and covered with a thin aluminum foil.

When the beam hit the target I was pleased to see lots of gamma rays and neutrons, but what caught Prof. Allison's attention were the charged particles. He put a sheet of paper in front of the ZnS and found only a slight reduction in the counting rate. He commented that such a large number of high-energy protons could only come from the reaction ^{7}Li(^{7}Li,p)^{13}B. He then noted that the only trouble with that explanation was that the nucleus ^{13}B [was not supposed] to exist.

The discovery of this nucleus was only the beginning. It was soon followed by further studies of lithium-induced nuclear reactions. The study of reactions with lithium beams was a new branch of nuclear physics. Even with a maximum beam energy of only 2 MeV, the Van de Graaff accelerator could be used to study reactions of ^{6}Li and ^{7}Li with all of the stable isotopes of Li, Be, B, C, N, and O. The lithium ions produced nuclei far from stability, of which ^{13}B was the first example. Reactions observed at energies near or below the Coulomb barrier included "fusion-like" processes such as $^{7}Li(^{7}Li,p)^{13}B$ and $^{9}Be(^{7}Li,p)^{15}C$ and "stripping or transfer" processes such as $^{9}Be(^{7}Li,^{8}Li)^{8}Be$. Measurements of the products of various reactions made it possible to determine the masses of the ground and low-lying excited states of ^{12}B, ^{13}B, ^{15}C, and ^{17}N. The last of his nuclear studies involved elucidation of the mechanisms of complex reactions such as $^{6}Li+^{6}Li$ yielding three alpha particles, and investigation of the role of intermediate nuclei (e.g., ^{8}Be) in these reactions.

Using data on $^{9}Be(^{7}Li,^{8}Li)^{8}Be$ from an experiment by Norbeck et al. at the University of Minnesota, Allison calculated the neutron density out to 40 fm. The words "halo nuclei," now in common use, did not appear until much later.

Allison introduced the precision techniques he had developed for nuclear reaction spectroscopy to study the interaction of particles with matter. He commented that everyone wanted quantitative information about the passage of beams through matter, but no one wanted to make the measurements. Using the apparatus developed for precise determination of the energies and products of nuclear reactions he and his associates were able to measure the changes in energy, the "stopping power," and the charge-changing cross-sections as a function of energy, ionic species, and stopping material. The early work on the energy loss of slow protons, deuterons, alpha particles, and Li^{6} nuclei passing through thin aluminum and gold films was pioneering and established Allison and his collaborators as the leaders in this field. The work was extended to gaseous targets. The results of the measurements of cross-sections for electron capture and loss in hydrogen and air were outstanding. This work was followed by extensive studies of helium ions in gasses where neutral atoms and both the singly and doubly charged ions coexist. The work was then extended to 2-MeV lithium.

In this atomic beam work Allison was without peer. The review article “Passage of Heavy Particles Through Matter” by Allison and Warshaw (1956) was the definitive work on stopping powers for at least a decade. The measurements of atomic capture cross-sections became important in applications, such as neutral injection into plasma machines and production of H^- ions in tandem Van de Graaff machines.

In the experiments on light nuclei it was often necessary to subtract a background due to a contamination of the targets by decomposed pump oil. Allison identified the unwelcome scattering nuclei by measuring the difference in energy between the incident and recoiling projectiles. That experience led him to suggest to his colleague Anthony Turkevich that this technique could be used to analyze surface materials where conventional chemical analysis was not feasible.

Turkevich and his colleague Anthony Tuzzolino built an instrument on this principle using the recently developed silicon detectors. Their scattering analysis instrument was carried to the moon on the last three *Surveyor* missions and made the first chemical analyses of the lunar surface. More recently, a successor to that instrument built by Tom Economu has analyzed the surface of Mars.

STUDENTS

Among Allison's major interests was the training of Ph.D. candidates in the techniques of research. Today many of his students pursue distinguished careers, in some cases working in fields far removed from their thesis problems. They recall his gift for making hard things clear and his emphasis on putting effort where it counts, a point he drove home with a turn of phrase: “If it's not worth doing, it's not worth doing well." His numerous overseas contacts resulted in a flow of foreign students and postdocs. George Morrison, a postdoc who played a leading role in the work with lithium at the Van de Graaff, relates, "In Looking back, I have to say that my period at Chicago was the most rewarding and enjoyable research time of my life… Lithium beams, even at 2 MeV were opening up new physics and there was Sam himself—encouraging, ebullient, luminous, and larger than life."

James Cronin began working in Allison's laboratory when he was still uncertain about what sort of physics to do, and Sam Allison's personality played a dominant role in his decision to do a thesis on nuclear physics. He says, "Sam was easy to work with, but [he] had his subtle ways of pushing his students. One Christmas, while I was away visiting my family, Sam built a proportional counter detector for my thesis experiment. It was done complete with a flowing gas system and a preamplifier. This showed his impatience with my slowness (and even reticence) to build this particular piece of equipment."

On Memorial Day weekends Allison brought his students and staff to his cabin in the North Woods. Everyone was expected to help clear brush and windfall accumulated over the winter, and Leo Herzenberg was among those who learned on those occasions to paddle a canoe, catch a fish, and wield an ax. Recalling an incident that was typical of Sam Allison's style, Herzenberg recounts, "One of the graduate students was attempting to cut down a small tree. He kept swinging the ax with much energy but hardly scratching the bark with each stroke. After a while he just stood there, covered with sweat, with a look of extreme frustration. Allison came over, took the ax, and with a single seemingly effortless swing cut right through the tree. The student stood there, mouth wide open, and asked, “How did you do that?” Allison, replied, “Fifty-seven years of experience!”

LAST YEARS

Allison went to Culham, England, near Oxford, in 1965 as the U.S. delegate to the Plasma Physics and Controlled Nuclear Fusion Research Conference sponsored by the International Atomic Energy Agency. He died there of complications following an aortic aneurism on September 15, 1965. In a memorial service at Chicago, William H. Zachariasen commented on Allison's last years and on the character of his life in words that provide a fitting conclusion to this memoir.

Despite heavy demands on his time by other duties in postwar years, Sam continued as an active scientist and teacher. But the combination of administrative duties and personal research taxed his strength in increasing measure as he grew older. When he resigned as director of the Fermi Institute in 1957, he felt relieved and looked forward with anticipation to many years of fruitful scientific inquiry under less stressful conditions. However, two years [before his death] his colleagues in the Fermi Institute appealed so strongly to Sam's sense of duty that he reluctantly agreed to serve yet another term. Surely… a younger man should have been found to do the job so that Sam, who had already given so much unselfish service, could have been spared this burden.

Sam had a good life. He was at peace with himself and with the world, and he had much happiness at home and in his work. He had a simple approach to his research. The only motivation was the job and excitement of satisfying intellectual curiosity. He had no thought of other rewards. However,…Sam was pleased and somewhat surprised that fellow scientists had such high opinions of his work. While he tended to belittle his own accomplishments, he was most liberal in praising those of other workers in the same field…[He was] a great and noble man.

I AM GRATEFUL TO many of Allison's friends, family members, students, and colleagues who have contributed material to and commented on drafts of this memoir. Among these are James Cronin,

Carol Herzenberg (Caroline Littlejohn)), Leo Herzenberg, Tanera Marshall, George Morrison, Paul Murphy, Edwin Norbeck, Gilbert Perlow, John Schiffer, John Simpson, and Anthony Turkevich. I have used copies of the tributes by H.L.Anderson, R.S.Shankland, A.Weinberg, J.H.Williams, and W.H.Zachariasen, and excerpts from anonymous notes, possibly by N.Sugarman, found in the files of the Enrico Fermi Institute. I have also used material from a booklet “Samuel K.Allison: The Frank P.Hixon Distinguished Service Professorship,” edited by C.Daly (University of Chicago Development Office). I have given all of the documents used in preparing this memoir to the Special Collections Department of the University of Chicago's Joseph Regenstein Library, which was an additional source.

SELECTED BIBLIOGRAPHY

1925 With W.Duane. On scattered radiation due to X rays from molybdenum and tungsten targets. *Proc. Natl. Acad. Sci. U.S.A.* 11:25–27.

1927 The reflection of X rays by crystals as a problem in the reflection of radiation by parallel planes. *Phys. Rev.* 29:375–79.

1929 With J.H.Williams. Design of a double X-ray spectrometer. *J. Opt. Soc. Am.* 18:473–78.

1935 With A.H.Compton. *X Rays in Theory and Experiment*. New York: D. Van Nostrand Company.

Experiments on the efficiencies of production and the half-lives of radio-carbon and radio-nitrogen. *Camb. Phil. Soc. Proc.* 32:179–82.

1939 The masses of Li^6, Li^7, Be^8, Be^9, B^{10}, and B^{11}. *Phys. Rev.* 55:624–27.

1956 With S.D.Warshaw. Passage of heavy particles through matter. *Rev. Mod. Phys.* 25:779–817.

With P.G.Murphy and E.Norbeck, Jr. Mass of B^{13} from the nuclear reaction $Li^7(Li^7,p)B^{13}$. *Phys. Rev.* 102:1182–83.

With C.S.Littlejohn. Stopping power of various gasses for lithium ions of 100–450 KeV kinetic energy. *Phys. Rev.* 104:959–61.

1958 Experimental results on charge-changing collisions of hydrogen and helium ions at kinetic energies above 0.2 KeV. *Rev. Mod. Phys.* 30:1137–68.

1960 Classical analysis of the reaction $Be^9(Li^7,Li^8)Be^8$. *Phys. Rev.* 119:1975–81.

With J.Cuevas and M.Garcia-Munoz. Experiments on charge-changing collisions of lithium ionic and atomic beams. *Phys. Rev.* 120:1266–78.

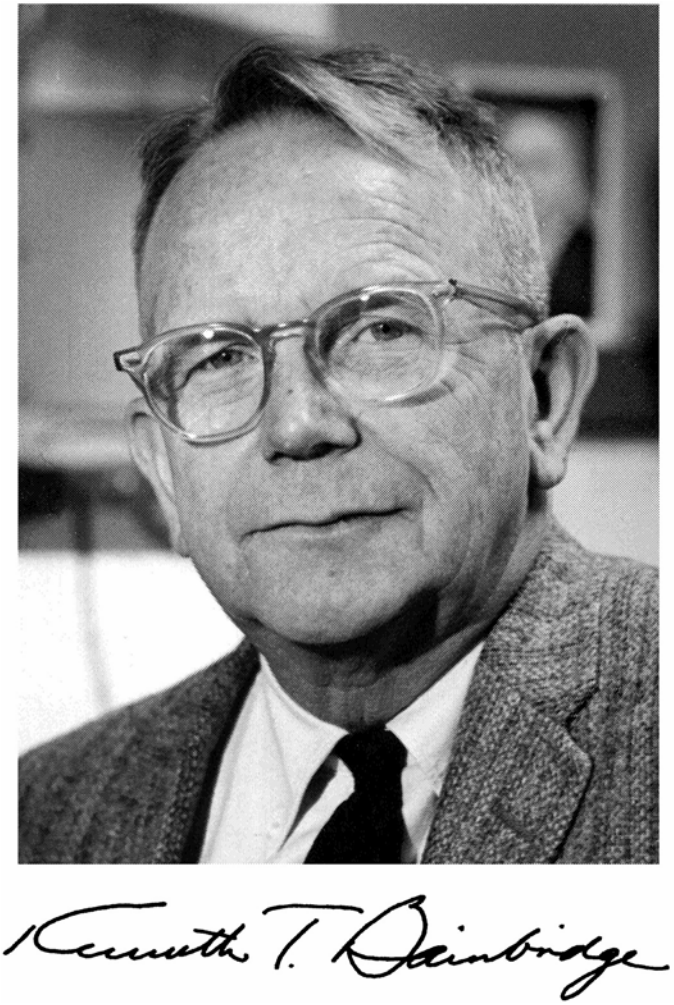


Photo by Armand Dionne, Harvard University

KENNETH TOMPKINS BAINBRIDGE

July 27, 1904–July 14, 1996

BY ROBERT V.POUND AND NORMAN F.RAMSEY

KENNETH TOMPKINS BAINBRIDGE was recognized early in his scientific career for his design and applications of mass spectrographs as research tools for nuclear mass measurements. His precise measurements of mass differences between nuclear isotopes, when compared to the energies of decay radiations, allowed him to confirm the mass-energy equivalency of A.Einstein. In collaboration with the late J. Curry Street[1] he designed and built the cyclotron at Harvard University that was sent to Los Alamos, New Mexico, during World War II. Bainbridge participated in the formation of the wartime Radiation Laboratory at MIT, where he spent more than two and one-half years developing microwave radar, particularly high-powered systems. In the spring of 1943 he transferred to the nuclear weapons project at Los Alamos, where he oversaw the test explosion of the first nuclear bomb at Alamogordo. Returning to Harvard after the war, he renewed his work with mass spectrographs, began the construction of a new cyclotron, and was able to measure changes in the decay rates of some radioactive nuclei resulting from differing molecular bonding and from physical compression.

FAMILY BACKGROUND

Kenneth Bainbridge was born on July 27, 1904, in Cooperstown, New York, the second of three brothers. He grew up in New York City, attending the Horace Mann School and the Horace Mann High School. He attributed his early interest in technology to the influence of two uncles who were engineers. His Uncle George worked on switching and safety devices for the New York subway and conceived a form of safety braking, but he was beaten out by Westinghouse, which had developed a better system. The Bainbridge family lived on Riverside Drive near 158th Street and the Hudson River, where just after World War I returning naval vessels docked. As a high schooler, Ken became interested in radio; on the family's rooftop he put an antenna that came to the attention of ship radio operators, who would knock on his door to investigate. These contacts enabled him to acquire rare 5-watt vacuum tubes from his callers for a couple of dollars. With those tubes he was able to set up a radiotelephone, obtained a radio amateur license, and operated a “ham” station with call letters 2WN (this was before the national prefix letter “W” was used.)

COLLEGE YEARS

Ken gave up his activities in radio and schoolboy chemistry, when in 1921 he entered MIT to study electrical engineering in a cooperative program with the General Electric Company. In that five-year program he was able to receive both an S.B. and an S.M. degree and to work summers at one of the General Electric facilities. In Ken's case this was at first in Lynn, Massachusetts, and then mostly at the Research Laboratories in Schenectady, New York. After completion, a natural consequence of his participation in the cooperative program with GE would have been for him to continue as an engineer at General Electric. As an outgrowth of his work there, Ken obtained patents on photo-cathode materials for photocells[2] and on amplification of photocurrents by secondary emission cathodes[3]. His work at the GE laboratories had, however, brought him to realize that his strong interest was in physics, and his colleagues there advised him to look to Princeton University for graduate work. Among those advisors was Karl T.Compton, who served as a consultant to GE and was then head of the physics department at Princeton.

With Tom Killian, his friend from his years at MIT, Ken applied to Princeton, and they were admitted in 1926. He described the two young men's interview with Dean West soon after their arrival, and credited West with saying, “You're nice boys, but it's too bad you never went to college.” After repeating that story, Ken usually indicated that, with his immersion into the collegiate Princeton atmosphere, he had soon somewhat made up for that lack in his background. As a graduate student in physics at Princeton, he became interested primarily in the developing study of nuclei, which had not yet become well covered in formal course work.

Ken's initial attraction to mass spectroscopy was excited by his desire to search for the then undetected element 87 of the periodic table, an element that should behave chemically as a heavy alkali, and which he therefore would call eka-cesium. He searched for it mainly in materials extracted from ores that were rich in the lighter alkalis—lithium, sodium, potassium, rubidium, and cesium—without success. Element-87 turns out to exist naturally only as short-lived isotopes resulting from the decay of actinium, the longest-lived having a half-life of 22 minutes. It was finally found in 1939 by Marguerite Perey at the Curie Laboratory of the Radium Institute of Paris, and hence has become known as “francium.”

THE YEARS BEFORE WORLD WAR II

After completing his Ph.D. program at Princeton, Ken Bainbridge spent four years, first as a National Research Council fellow and then as a Bartol Research Foundation fellow, at the Franklin Institute's Bartol Research Foundation. The “Bartol” was then located on the campus of Swarthmore College in Pennsylvania, and was directed by the eccentric Englishman, W.F.G.Swann, who was especially interested in research on cosmic rays and nuclear physics. (I use the term “eccentric” in recalling his frequent participation in the meetings of the American Physical Society with comments from the front row seats. He often carried his cello along and his long white hair, perhaps even then less remarkable for a musician, was quite unconventional at that time for a physicist.) It was there that Ken continued to develop his mass spectrographs and to undertake precise nuclear mass measurements, which he used to confirm the mass-energy equivalence, $E=Mc^2$.

In September 1931 Ken and Margaret (“Peg”) Pitkin, then a member of the Swarthmore teaching faculty, were married. In the summer of 1933 they traveled to Cambridge, England, where, as a John Simon Guggenheim fellow, Ken joined Lord Ernest Rutherford's Cavendish Laboratory, then a world leader in experimental nuclear physics. Ken described (1975) his first encounter there with the idea of a nuclear chain reaction when Rutherford stopped him in passing in a corridor to ridicule as obviously impractical a suggestion just made to him by a visitor, Leo Szilard, for such a process based on protons. Szilard went on to envisage a much more practical process involving neutrons, which, of course, only became reality after neutron-induced uranium fission was discovered. At Cambridge Ken continued to pursue mass spectroscopy and began a continuing close friendship with John D.Cockroft (later to become Sir John). Martin K.Bainbridge, the first child of Ken and Peg was born in Cambridge, England, in 1933.

In September of 1934 Ken returned to the United States and began his long association with the physics department at Harvard University. He built and employed the improved mass spectrograph he had designed during his sojourn at the Cavendish Laboratory. With the collaboration of J.Curry Street he also undertook to build a cyclotron. He was grateful to E.O.Lawrence of the University of California at Berkeley for assisting them in the design by sending details of his new 37-inch cyclotron. It was a lifelong characteristic of Ken's style that he thoroughly documented all of his projects and, to emphasize that point, he said (1975), "In the event the cyclotron was ever mislaid, stolen, or borrowed, I knew I could identify it—and later did at Los Alamos." The operational cyclotron was requisitioned in 1943 by the U.S. Army, dismantled, and rebuilt at the weapons laboratory. It remained there after the war, never to return to Harvard.

Bainbridge's interest in mass spectroscopy of nuclei led him to modify the naturally occurring abundances of nuclear isotopes, and he proposed a method using gaseous counterflow in a Holweck molecular vacuum pump. With the discovery of uranium fission he recognized the importance of enrichment of ^{235}U and enlisted colleagues from the Harvard chemistry department George B.Kistiakowsky and E.Bright Wilson in pursuing such a project. A trial experiment with argon gas confirmed their expectations, but when they sought to gain the interest of officials in Washington in 1940, they were told to forget it, that classified work was going on, and "the situation [was] well in hand."

THE WAR YEARS

As Europe became embroiled in World War II and with the resulting recognition of a need for increased military preparations in the United States, an exchange of military technical secrets with the beleaguered British was undertaken. This was the subject of the Tizard Mission from Britain in September of 1940. A major element of the exchange turned out to be a demonstration by the British of the newly developed pulsed-cavity magnetron, which produced many kilowatts of peak power at a microwave frequency near the 10-cm wavelength. The members of the microwave subcommittee, chaired by Alfred Loomis of the National Defense Research Committee, were so excited by the demonstrated performance that they undertook almost overnight to establish a special laboratory to develop microwave "radar" around it. Kenneth Bainbridge was the first scientist not already involved with the committee to be recruited (by E.O.Lawrence) to the laboratory, which became the Radiation Laboratory at the Massachusetts Institute of Technology. On a leave of absence from Harvard, he spent more than two and one-half years on that project, during a part of which, in the spring of 1941, he participated in a mission to Britain.

In wartime activities that involved close collaboration with parallel projects in Britain, Ken's friendship with British physicists, especially with Cockroft, who had been a scientist member of the Tizard Mission, was an asset. On his visit he gained information about not only the radar program but also learned of British progress toward releasing nuclear energy while attending a meeting of the Maud Committee, which was overseeing that effort in Britain. Ken's particular project at the Radiation Laboratory was the push toward higher-powered radars, especially for the Navy. He found the Navy at that time the most technically oriented of the

U.S. military services and the least handicapped by protocols related to military rank. This experience was reflected in his concern about the organization of Los Alamos, where he was recruited in May of 1943, which operated under the Manhattan District of the U.S. Army and General Leslie R. Groves. The Bainbridge's two daughters Joan (Bainbridge) Safford and Margaret Tomkins (Bainbridge) Robinson were born in Cambridge, Massachusetts, well before the family moved to Los Alamos.

At Los Alamos early in 1944, at the request of George Kistiakowsky and Director J.Robert Oppenheimer, Ken undertook the oversight of the design of high explosive assemblies and preparations for a full-scale test of a nuclear bomb. In articles in the *Bulletin of the Atomic Scientists* (1975), he lucidly described the search for an appropriate site, the preparations, and the successful carrying out of the test early in the morning of July 16, 1945. He titled the second of those stories “A Foul and Awesome Display.” His remark to J.Robert Oppenheimer immediately after the event— “Now we are all sons of bitches”— marked the beginning of his dedication to ending the testing of nuclear weapons and to efforts to maintain civilian control of future developments in that field.

RETURN TO ACADEMIC LIFE

In the fall of 1945 Ken was at last free to return to academic science at Harvard. He undertook then to build a large mass spectrograph, designed for high resolution of masses and to replace the prewar cyclotron with a much more powerful one utilizing the then newly invented concept of synchronous acceleration. The relativistic increase of effective mass as the protons gained energy was compensated by sweeping the radio frequency down appropriately during an acceleration cycle. The latter project was handed over to Robert R.Wilson, who had joined the physics department at Harvard after the end of the war. Wilson, however, was recruited to head the large nuclear physics group at Cornell after residence at Harvard for just one semester. Norman F.Ramsey was then recruited to the Harvard department and assumed responsibility for managing the construction of the new cyclotron. Its design had been completed before discovery of the pi meson as the particle mediating nuclear forces. The energy of the new synchrocyclotron turned out to be just less than required for pion production. For about a dozen years the new Harvard cyclotron was employed for many scattering experiments and other studies of nucleon-nucleon forces and of nuclear structure. The operating life of the cyclotron was greatly extended when it became a facility for research on the use and clinical applications of the highly focused proton beam in collaborative projects with staff members from the Massachusetts General Hospital. It will be shut down finally in the late 1990s, when its role will be taken over by an even more powerful dedicated machine at the hospital, enabling further expansion of the important clinical applications developed using the physics cyclotron.

Ken devoted much of his energy just after the war to designing for the Harvard physics department an advanced laboratory in nuclear physics intended as a course of study for graduate students. Because of the many new students underwritten by the GI Bill, the number of graduate students in physics was far greater than had been the norm before the war. Nuclear physics had gained new visibility and popularity from its contributions to winning the war. Students gained their first experience in activities preparing them for research in experimental physics in Ken's meticulously designed and documented laboratory. The experiments ranged from a replica of J.J.Thompson's positive ray apparatus (a precursor of mass spectrographs) through a bent crystal X-ray spectrograph, a 180° beta-ray spectrograph using the then new technique of NMR for field calibration, to analysis of tracks in photographic emulsions to identify muons. As a part of Ken's dislike of the development and testing of nuclear weapons, he set up a facility associated with his laboratory of nuclear physics to collect and measure radioactive fallout. In his own research he built balanced ionization chambers with which he was able to determine changes in lifetimes of several long-lived isomers, which decay by internal electron conversion when their atoms are differently bonded chemically or are subjected to physical compression. In addition to constructing his large mass spectrograph to make precise measurements of mass differences among pairs, he built an elegant double-focusing electron spectrograph. In the years before his retirement in 1975, Ken devoted much of his time to improving the graduate student advanced laboratory and to developing a similar version for advanced undergraduates. Among his other contributions to teaching were lecture courses on nuclear physics, mainly for graduate students.

From 1950 to 1954 Ken served as chairman of the physics department at Harvard. This was a time marked by the vicious attacks on certain members of academia, and especially at Harvard, by the House Un-American Activities Committee and a committee of the Senate dominated by Senator Joseph McCarthy. Ken gave generously of his time and energy overseeing the relationship between the university administration and one of our colleagues who became a prime target of these attacks.

Two legacies of Ken's years as chairman were a renovation of a part of the then seventy-year-old Jefferson Physical Laboratory and the establishment of the Morris Loeb Lectures in Physics. Both were enabled by use of a part of a newly available endowment fund, which had been held in trust for many years. Thus the Morris Loeb endowment was shared between the chemistry and physics departments. A characteristic of Ken's style of work in his developing of instruments, lecturing in courses, research, and administrative activities was a meticulous documentation and keeping of records, habits of enormous help to his successors in all those projects.

In the late 1950s Ken was one of the first members of the Harvard faculty to participate in a new academic exchange program with the Soviet Union. Harvard's sister university was designated to be the University of Leningrad. Almost concurrent with his arrival in Leningrad there occurred the incident of the crash of the RB72 reconnaissance plane somewhere off Murmansk, which threw a difficult shadow over his relationship with his Soviet hosts, but the tension relaxed during the course of his stay.

In June 1975 in his last year before his retirement, Bainbridge was enlisted to serve on a joint Iran-Harvard planning commission to design Reza Shah Kabir University for Iran. Ken and his Harvard colleagues made visits to Iran, however this project was short lived because of the political upheaval and expulsion of the Shah from Iran.

Ken's years in Los Alamos and Alamogordo, New Mexico, provided him an opportunity to indulge in his long established amateur interest in mineral crystallography, a source of great pleasure. He had always enjoyed outings in the mountains, even in New England, to collect specimens, and New Mexico brought a much expanded dimension to that interest.

In January 1967 Ken suffered a tragic loss when his wife Margaret (Pitkin) Bainbridge, the mother of his three children, died suddenly at their home in Watertown, Massachusetts, from a blood clot associated with a recently fractured wrist. Ken, Peg, and their children had formed a great attachment to the island of Martha's Vineyard, where they had spent many of their summers as tenants in a cottage in Chilmark overlooking Chilmark Pond just below Abel's Hill. They had finally been able to buy a piece of land there and had just completed their own summerhouse the year before Peg's sudden death. That beautifully situated house was designed and constructed with the same careful attention to detail that exemplified all of Ken's personal and professional activities. His daughters have described instructions he left for the continued upkeep of the house, especially its extensive deck, adding that a supply of the needed materials was stored in the basement. Peg, their son Martin, and finally Ken were all buried in a plot in the small historic cemetery on Abel's Hill overlooking their house below. I (R.V.P.) am grateful to Ken for also introducing my wife and me to the beauties of the island as long ago as 1950, where we, too, were able to spend several happy holidays.

In October 1969 Ken married Helen Brinkley King, an old friend then serving as an editor for the William Morrow publishing house in New York City. She, as well as his son Martin Keeler Bainbridge, predeceased him. He is survived by his two daughters Joan Bainbridge Safford of Evanston, Illinois, who is deputy United States attorney for the northern district of Illinois, and Margaret Bainbridge Robinson of Cleveland Heights, Ohio, who is dean of undergraduate studies at Case Western Reserve University, and five grandchildren.

CONCLUSION

Kenneth Bainbridge contributed extensively to the development of the field of nuclear physics during his many active years. Especially notable were his several designs of mass spectrographs and their many applications to the study of nuclear isotopic masses and energy-mass equivalence. Other important contributions to nuclear physics were his early construction of cyclotrons and his discovery of the effect of chemical states on nuclear decay rates. During World War II, as the first recruit to the MIT Radiation Laboratory, he made major contributions to the microwave radar program of the Allies and then moved on to Los Alamos, where he oversaw the preparations and carrying out of the first test nuclear explosion. He was a strong advocate of civilian control of nuclear developments and devoted time and energy to efforts to restrict any first use of nuclear weapons by the United States. Bainbridge, as a teacher, introduced many students to the science of physics, especially nuclear physics, through his lecture courses and his advanced laboratory. He was a careful designer and painstaking keeper of records. The work of his colleagues and his successors in the enterprises he had developed gained enormously from his pioneering contributions, detailed designs, and meticulous record keeping. He was a model of personal and scientific integrity and a personal friend who is much missed.

Kenneth Bainbridge was awarded the Levy Medal of the Franklin Institute in 1934. He was elected a fellow of the American Academy of Arts and Sciences in 1937 and a member of the National Academy of Sciences in 1946. He was the recipient of two letters of commendation from General Leslie R.Groves for his work on the Manhattan Project and the Presidential Certificate of Merit for his services as staff member of the MIT Radiation Laboratory,

ESPECIALLY HELPFUL sources for this memoir were an interview of K.T. Bainbridge by John Bryant published as *"Rad Lab: Oral Histories Documenting World War II Activities at the MIT Radiation Laboratory"* published by the IEEE in 1993 and the Bainbridge articles in the *Bulletin of the Atomic Scientists* cited in the bibliography (1975).

NOTES

1. K.T.Bainbridge and others. Jabez Curry Street 1906–1989. In *Biographical Memoirs,* vol. 71, pp. 346–55. Washington, D.C.: National Academy Press, 1997.

2. Photo-electric tubes, patent no. 1,901,577; method of preparing photo-electric tubes, patent no. 1,901,578 (British patent no. 303,476).

3. A method of amplifying photo-electric currents by means of secondary emission from an auxiliary cathode, patent no. 2,206,713.

SELECTED BIBLIOGRAPHY

1929 A search for element 87 by analysis of positive rays. *Phys. Rev.* 34:752– 62.
1930 Simple isotopic constitution of caesium. *Phys. Rev.* 36:1668.
1931 The isotopes of lithium, sodium, and potassium. *J. Franklin Inst.* 212:317–39.
1932 A mass spectrograph. *Phys. Rev.* 40:130A.
1933 Comparison of the masses of He and H^1 on a mass spectrograph. *Phys. Rev.* 43:103–105.
The equivalence of mass and energy. *Phys. Rev.* 44:123.
Atomic masses and structure of atomic nuclei. *J. Franklin Inst.* 215:509– 34.
1936 With E.B.Jordan. Mass spectrum analysis. 1. The mass spectrograph. 2. The existence of isobars of adjacent elements. *Phys. Rev.* 50:282– 96.
1940 The Harvard cyclotron. *Harv. Alumni Bull.* May 17.
1941 With R.Sherr and H.H.Anderson, Transmutation of mercury by fast neutrons. *Phys. Rev.* 60:473–79.
1951 With A.A.Bartlett. High resolution two-directional focussing beta-ray spectrometer. *Rev. Sci. Instrum.* 22:517–23.

1951 With M.Goldhaber and E.Wilson. Influence of the chemical state on the lifetime of an isomer. *Phys. Rev.* 84:1260–61.

1953 With M.Goldhaber and E.D.Wilson. Influence of the chemical state on the lifetime of a nuclear isomer, Tc^{99m}. *Phys. Rev.* 90:430– 39.

Charged particle dynamics and optics, relative isotopic abundances of the elements, atomic masses. In *Experimental Nuclear Physics,* vol. I, ed. E.Segre. New York: Wiley and Sons.

With J.J.Kraushaar and E.D.Wilson. Comparison of the values of the disintegration constant of Be^7 in Be, BeO, and BeF_2. *Phys. Rev.* 90:610–14.

1957 With T. L Collins. A large mass spectrograph. In *Proceedings of the Conference on Nuclear Masses and Their Determination, MAINZ, 1956.* Pergamon Press.

1960 With P.E.Moreland. The mass spectrometer at Harvard University. In *Proceedings of the International Conference on Nuclidic Masses.* Toronto: University of Toronto Press.

1966 With A.C.Malliaris. Alteration of the decay constant of Te^{125M} by chemical means. *Phys. Rev.* 149:958–64.

1967 With J.W.Dewdney. Use of a lock-in amplifier for mass doublet measurements by the coincidence method. In *Proceedings of the 3rd International Conference on Atomic Masses, Winnipeg, Manitoba, Canada, 28 Aug.–1 Sept. 1967,* pp. 758–76. Winnipeg: University of Manitoba Press.

1969 With A.Olin. Influence of superconductivity on the half-life of niobium-90^m. *Phys. Rev.* 179:450–52.

1971 With D.P.Kerr. The ^{235}U-^{207}Pb and ^{238}U-^{208}pb mass differences. *Canad. J. Phys.* 49:756–60.
With D.P.Kerr. The ^{14}ND-^{15}NH mass differences. *Canad. J. Phys.* 49:1950–51.
1975 All in our time—Prelude to Trinity. *Bull. At. Sci.* 31(4):42–46.
All in our time—A foul and awesome display. *Bull. At. Sci.* 31(5):40– 46.

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VALENTINE BARGMANN

April 6, 1908–July 20, 1989

BY JOHN R.KLAUDER

"LET'S ASK BARGMANN!" With that phrase—addressed to me by my Princeton thesis advisor—I was led to my first real encounter with Valentine Bargmann. Our question pertained to a mathematical fine point dealing with quantum mechanical Hamiltonians expressed as differential operators, and we got a prompt, clear, and definitive answer. Valya—the common nickname for Valentine Bargmann—was already an established and justly renowned mathematical physicist in the best sense of the term, and his advice was widely sought by beginners and experts alike. It was, of course, a thorough preparation that brought Valya to his well-deserved reputation.

Valya Bargmann was born on April 6, 1908, in Berlin, and studied at the University of Berlin from 1925 to 1933. As National Socialism began to grow in Germany, he moved to Switzerland, where he received his Ph.D. in physics at the University of Zürich under the guidance of Gregor Wentzel. Soon thereafter he emigrated to the United States. It is noteworthy that his passport, which would have been revoked in Germany at that time, had but two days left to its validity when he was accepted for immigration into the United States. He soon joined the Institute for Advanced

Study in Princeton, and in time was accepted as an assistant to Albert Einstein.

Along with Peter Bergmann, Bargmann analyzed five-dimensional theories combining gravity and electromagnetism at a classical level. During World War II, Bargmann worked on shock wave studies with John von Neumann and on the inversion of matrices of large dimension with von Neumann and Deane Montgomery. Bargmann taught informally at Princeton beginning in 1941. He received a regular appointment as a lecturer in physics in 1946 and remained at Princeton essentially for the rest of his career.

Bargmann worked with Eugene Wigner on relativistic wave equations and together they developed the justly famous Bargmann-Wigner equations for elementary particles of arbitrary spin. In 1978 Bargmann and Wigner jointly received the first Wigner Medal, an award of the Group Theory and Fundamental Physics Foundation. Besides this honor, Bargmann was elected to the National Academy of Sciences in 1979 and won the Max Planck Medal of the German Physical Society in 1988.

Valya was a gentle and modest person—and he was a talented pianist. At social occasions it was not uncommon for Valya to perform solo or accompany other musicians.

His lectures were renowned for their clarity and polish. Among the prized series of lectures were those on his acknowledged specialties, such as group theory (e.g., the Lorentz group and its representations, and ray representations of Lie groups), as well as second quantization. In mathematics, his most influential work was on the irreducible representations of the Lorentz group. This work has served as a paradigm for representation theory ever since its appearance. Bargmann also made important contributions to several aspects of quantum theory. He was a stellar example of the European tradition in mathematical physics in the spirit of Hermann Weyl, von Neumann, and Wigner. A book in Bargmann's honor offers expert comments on a number of topics dear to the heart of Valya.[1]

Bargmann had his less serious side as well. No better example of that can be given than the story told by Gérard G.Emch, who arrived at Princeton in 1964 to begin a postdoctoral year with Valya. Emch also arrived with a newly minted “theorem,” which he proudly presented to the master. No sooner had the theorem been laid out than Bargmann was ready with a counterexample. Sorely disappointed, Emch retreated for home that day and continued to study the matter. At 3 a.m. Emch's phone range. The caller, Bargmann, heartily laughed when Emch quickly picked up the phone. He then said, “I thought you would still be up. Go to bed and get some sleep. I have found an error in my counterexample. We can discuss it tomorrow!”

Valya Bargmann published a modest number of papers by contemporary standards, but he nevertheless was instrumental in opening several distinct fields of investigation. His paper on establishing a limit on the number of bound states to which an attractive quantum mechanical potential may lead has spawned a minor industry in the research on such issues. His paper dealing with distinct potentials that exhibit identical scattering phase shifts redirected research in inverse scattering theory in which it had been previously assumed that the scattering phase shifts would uniquely characterize the potential. His study of the unitary irreducible representations of the noncompact group SL (2,R) have proved not only invaluable in their own right but have served as a model of how such representations are to be sought for more general noncompact Lie groups. Shortly after completing the work on SL(2,R) he also completed a manuscript on the related group SL(2,C). This work, however, was never published because independent work by Israel

Gel'fand and Mark Naimark covering the same ground reached the publisher ahead of Bargmann's planned submission.

To a large extent, Bargmann tended to write either short notes or long, extensive articles. When he felt he really had something to say it seems he would become didactic, thorough, and complete. Thus, his papers on SL(2,R) and the factor representation of groups were both long papers by Bargmann's standards. However, he saved his longest and most sustained study until the 1960s, when he dealt with one of the subjects for which he will long be remembered. It is to this set of papers and a brief sketch of some of their principal novelty that I would like to turn my attention at this point. I have chosen to outline two mathematical arguments, because on the one hand they are relatively simple and on the other hand they are universal and profound.

From 1961 onward, Valya published several papers dealing with the foundations and applications of Hilbert space representations by holomorphic functions now commonly known as Bargmann spaces (or sometimes as Segal-Bargmann spaces in view of an essentially parallel analysis of the main features by Irving Segal). We can outline a few of the principal ideas in such an analysis by first starting with the following background material. The basic kinematical operators in canonical quantum mechanics for a single degree of freedom may be taken as the two Hermitian operators Q and P, which obey the fundamental Heisenberg commutation relation

$$[Q,P] \equiv QP - PQ = i\hbar I, \quad i \equiv \sqrt{-1},$$

where $\hbar$ denotes Planck's constant $h/2\pi$, and where I denotes the unit operator. In the Schrödinger approach to quantum mechanics these operators are represented as

$Q \rightarrow x$ and $P \rightarrow -i\hbar\partial/\partial x$ acting on complex-valued functions $\psi(x)$ that belong to the Hilbert space $L^2(\mathrm{R},dx)$ composed of those functions for which

$$\int_{-\infty}^{\infty} \psi(x)^* \psi(x) dx < \infty.$$

Given any two such functions ϕ and ψ, which are sufficiently smooth and vanish at infinity, then the Hermitian character of x and $-i\hbar\partial/dx$ follow from the properties that

$$\int_{-\infty}^{\infty} [x\phi(x)]^* \psi(x) dx = \int_{-\infty}^{\infty} \phi(x)^* x\psi(x) dx,$$

and

$$\begin{aligned} &\int_{-\infty}^{\infty} [-i\hbar\partial\phi(x)/\partial x]^* \psi(x) dx \\ &= i\hbar\phi(x)^* \psi(x) \Big|_{-\infty}^{\infty} + \int_{-\infty}^{\infty} \phi(x)^* (-i\hbar)\partial\psi(x)/\partial x \, dx \\ &= \int_{-\infty}^{\infty} \phi(x)^* [-i\hbar\partial\psi/\partial x] dx. \end{aligned}$$

An elementary yet important alternative combination of the basic operators Q and P is given by

$$A \equiv (Q + iP)/\sqrt{2\hbar}, \quad A^\dagger \equiv (Q - iP)/\sqrt{2\hbar},$$

where $A^\dagger$ denotes the Hermitian adjoint of the operator A. The basic commutation relation between Q and P then leads to

$$[A, A^\dagger] = AA^\dagger - A^\dagger A = I,$$

which is often chosen as an alternative starting point. Indeed, Vladimir Fock in 1928 recognized that this form of the commutation relation may be represented by the ex pressions $A \rightarrow \partial/\partial z$ and $A^\dagger \rightarrow z$ acting on a space of analytic functions *f(z)* defined for a complex variable *z*. While it is true that this representation of the operators A and $A^\dagger$ satisfies the correct commutation relation, it is unclear how z and $\partial/\partial z$ can be considered adjoint operators to one another, especially in light of the fact demonstrated above that $x^\dagger = x$ and $(-i\hbar\partial/\partial x)^\dagger = (-i\hbar\partial/\partial x)$ hold in the Schrödinger representation.

Bargmann was among the first to show clearly how one can justify the relation $z^\dagger = \partial/\partial z$. To that end Bargmann defined a Hilbert space F of holomorphic functions *f(z)* restricted so that

$$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} f(z)^* f(z) e^{-|z|^2}\, dx\, dy < \infty$$

where $|z|^2 = z^* z$, $z = x + iy$, and the domain of integration is over the plane R^2. We observe that a general function of x and y can be viewed as a (related) general function of $z = x + iy$ and $z^* = x - iy$. A holomorphic function *g(z)* depends on only one such variable, *z*, or stated otherwise $\partial g(z)/\partial z^* \equiv 0$ along with the complex conjugate relation $\partial g(z)^*/\partial z \equiv 0$. Let f and g be two holomorphic functions and consider the following integral (with the limits implicit)

$$\begin{aligned}
&\int\int [zg(z)]^* f(z) e^{-|z|^2}\, dx\, dy \\
&= \int\int g(z)^* z^* f(z) e^{-|z|^2}\, dx\, dy \\
&= \int\int g(z)^* f(z) (-\partial/\partial z\, e^{-|z|^2})\, dx\, dy \\
&= \int\int e^{-|z|^2} (\partial/\partial z)[g(z)^* f(z)]\, dx\, dy \\
&= \int\int g(z)^* [\partial f(z)/\partial z] e^{e-|z|^2}\, dx\, dy,
\end{aligned}$$

which shows that $z^\dagger=\partial/\partial z$ as required. With the introduction of the given inner product for two holomorphic functions, Bargmann put the heuristic notion that z and $\partial/\partial z$ were adjoint operators onto a firm mathematical foundation.

All separable and infinite-dimensional Hilbert spaces are isomorphic. The relation between the space $L^2(\mathrm{R},dx)$ and the space F can be given in the following form. To each $\psi \in L^2$ associate the expansion

$$\psi(x) = \sum_{n=0}^{\infty} a_n h_n(x), \quad a_n = \int_{-\infty}^{\infty} h_n(x)\psi(x)dx$$

in terms of the complete, real, orthonormal set of Hermite functions $\{h_n(x)\}_{n=0}^{\infty}$ each element of which is implicitly defined by the fact that

$$\exp(-s^2 + 2sx - 1/2\,x^2) = p^{1/4}\sum_{n=0}^{\infty}(n!)^{-1/2}(s\sqrt{2})^n h_n(x).$$

Moreover,

$$\sum_{n=0}^{\infty}| a_n |^2 = \int_{-\infty}^{\infty} | \psi(x) |^2 \, dx < \infty.$$

Clearly, $\psi \in L^2$ uniquely determines the sequence $\{a_n\}_{n=0}^{\infty}$. To each such sequence we associate the holomorphic function

$$f(z) = \sum_{n=0}^{\infty} a_n z^n / \sqrt{n!},$$

which converges absolutely for all z. In a decided advance, Bargmann was able to put this association to an expanded use as follows.

The space of tempered test functions consists of those complex functions $u(x)$ such that $u \in C^\infty$ and $x^r\, d^s u(x)/dx^s$

$\to 0$ as $x\to\pm\infty$ for all nonnegative r and s. Each such function is realized by the expansion

$$u(x)=\sum_{n=0}^{\infty} b_n h_n(x),$$

where $n^r b_n \to 0$ as $n\to\infty$ for all r (i.e., b_n falls to zero faster than any inverse power). Dual to the space of tempered test functions is the space of tempered distributions, a special class of generalized functions. If $D(x)$ denotes such a generalized function, then D admits the formal expansion

$$D(x)=\sum_{n=0}^{\infty} d_n h_n(x),$$

where $\{d_n\}_{n=0}^{\infty}$ is a sequence of polynomial growth (i.e., $|d_n| \le R+S^n$ for suitable R and S). Generally, D is only a generalized function (e.g., $D(x)=\delta(x-y)$ when $d_n=h_n(y)$, etc.), not an ordinary function, and, although the left-hand side of the relation

$$\sum_{n=0}^{\infty} b_n^* d_n = \int_{-\infty}^{\infty} u(x)^* D(x)dx$$

is well defined, the right-hand side does not exist as a traditional integral.

Bargmann realized, however, that the action of tempered distributions on test functions did indeed possess a genuine integral representation in terms of holomorphic functions. For that purpose let

$$u(z) \equiv \sum_{n=0}^{\infty} b_n z^n / \sqrt{n!}$$

denote the image of $u(x)$ in F. Furthermore, we define

$$D(z) \equiv \sum_{n=0}^{\infty} d_n z^n / \sqrt{n!}$$

as the image of the generalized function $D(x)$. Since $|d_n| \le R$

$+S^n$ it follows that the series defining *D(z)* converges everywhere and thereby defines a holomorphic function. Moreover, it follows that

$$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} u(z) * D(z)e^{-|z|^2}\, dx\, dy \,/\, \pi = \sum_{n=0}^{\infty} b_n^* d_n \,.$$

Thus, the holomorphic function representation endowed with the Bargmann inner product provides an explicit integral representation for the action of an arbitrary tempered distribution, a feature entirely unavailable in the usual form of generalized functions and formal integrals.

The discussion just concluded regarding two topics dealing with holomorphic function spaces illustrates the penetrating simplicity of Bargmann's approach to mathematical physics. Would that we had more like him today; I occasionally miss the opportunity to "ask Bargmann."

THANKS ARE EXTENDED to Gérard G.Emch, Elliott H.Lieb, Barry Simon, and Arthur S.Wightman for their input and/or comments that have found their way into this article.

NOTE

1. E.H.Lieb, B.Simon, and A.S.Wightman, eds. *Studies in Mathematical Physics, Essays in Honor of Valentine Bargmann.* Princeton, N.J.: Princeton University Press, 1976.

SELECTED BIBLIOGRAPHY

1934 Über den Zusammenhang zwischen Semivektoren and Spinoren und die Reduktion der Diracgleichung für Semivektoren. *Helv. Phys. Acta* 7:57–82.

1936 Zur Theorie des Wasserstoffatoms. *Z. Phys.* 99:576–82.

1937 Über die durch Elektronenstrahlen in Kristallen angeregte Lichtemission. *Helv. Phys. Acta* 10:361–86.

1941 With A.Einstein and P.G.Bergmann. On the five-dimensional representation of gravitation and electricity. In *Theodore von Kármán Anniversary Volume,* pp. 212–25. Pasadena: California Institute of Technology.

1944 With A.Einstein. Bivector fields. *Ann. Math.* 45:1–14.

1945 On the glancing reflection of shock waves. Applied Mathematics Panel Report No. 108.

1946 With D.Montgomery and J.von Neumann. Solution of linear systems of high order. Report to the Bureau of Ordinance, U.S. Navy.

1947 Irreducible unitary representations of the Lorentz group. *Ann. Math.* 48:568–640.

1948 With E.P.Wigner. Group theoretical discussion of relativistic wave equations. *Proc. Natl. Acad. Sci. U.S.A.* 34:211–23.

1949 Remarks on the determination of a central field of force from the elastic scattering phase shifts. *Phys. Rev.* 75:301–303.

On the connection between phase shifts and scattering potential. *Rev. Mod. Phys.* 21:488–93.

1952 On the number of bound states in a central field of force. *Proc. Natl. Acad. Sci. U.S.A.* 38:961–66.

1954 On unitary ray representations of continuous groups. *Ann. Math.* 59:1–46.

1959 With L.Michel and V.Telegdi. Precession of the polarization of particles moving in a homogeneous electromagnetic field. *Phys. Rev. Lett.* 2:435–36.

1960 Relativity. In *Theoretical Physics in the Twentieth Century* (Pauli Memorial Volume), eds., M.Fierz and V.F.Weisskopf, pp. 187–98. New York: Interscience Publishers.

With M.Moshinsky. Group theory of harmonic oscillators. I. The collective modes. *Nucl. Phys.* 18:697–712.

1961 With M.Moshinsky. Group theory of harmonic oscillators. II. The integrals of motion for the quadrupole-quadrupole interaction. *Nucl. Phys.* 23:177–99.

On a Hilbert space of analytic functions and an associated integral transform. Part I. *Commun. Pure Appl. Math.* 14:187–214.

1962 On the representations of the rotation group. *Rev. Mod. Phys.* 34:829– 45.

1964 Note on Wigner's theorem on symmetry operations. *J. Math. Phys.* 5:862–68.

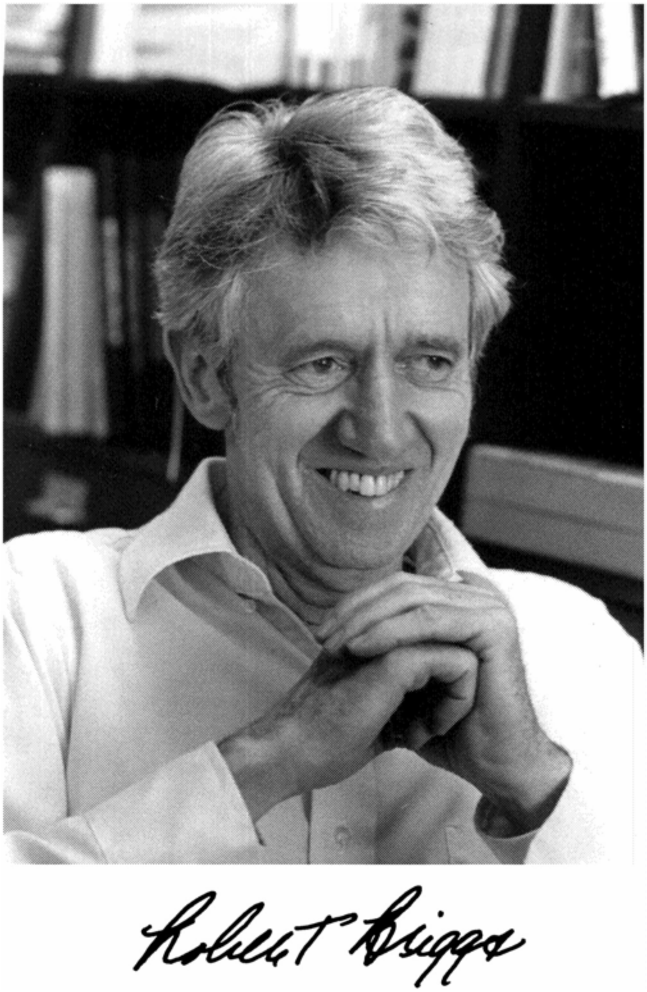
1967 On a Hilbert space of analytic functions and an associated integral transform. Part II. A family of related function spaces application to distribution theory. *Commun. Pure Appl. Math.* 20:1–101.

1971 With P.Butera, L.Girardello, and J.R.Klauder. On the completeness of the coherent states. *Rep. Math. Phys.* 2:221–28.

1972 Notes on some integral inequalities. *Helv. Phys. Acta* 45:249–57.

1977 With I.T.Todorov. Spaces of analytic functions on a complex cone as carriers for the symmetric tensor representations of SO*(n)*. *J. Math. Phys.* 18:1141–48.

1979 Erinnerungen eines Assistenten Einsteins. Vierteljahrsschrift der Naturforschenden Gesellschaft in Zürich, Jahrgang 124, Heft 1 , pp. 39–44. Zürich: Druck und Verlag Orell Fussli Graphische Betriebe AG.



ROBERT W.BRIGGS

December 10, 1911–March 4, 1983

BY MARIE A.DI BERARDINO

PROFESSOR ROBERT BRIGGS made pioneering research contributions in the developmental genetics of amphibia for over four decades. His chief embryological interest was to understand the genetic control of development. This focus led him to study, among other areas of research, two major problems: the developmental potential of nuclei during embryogenesis by means of nuclear transplantation into oocytes and the role of maternal gene products in the development of the embryo. He provided the basis for current research on cloning metazoan animals and the genetic control of pattern development.

Briggs developed with Thomas J.King a technique to transplant living frog nuclei from embryonic cells into an oocyte whose own nucleus had been removed. They found that many nuclei directed normal development of the oocytes from early embryonic stages, whereas only a few nuclei did so from advanced embryonic stages, indicating that most nuclei acquire restrictions concomitant with cell specialization. The results of these classic studies are, still today, consistent with the changing patterns of gene expression occurring during embryogenesis that are controlled by relatively stable alterations in the chromosomal proteins and DNA methylation. At least two additional results emanated from the nuclear transplantation studies: many advanced-stage nuclei undergo significant reprogramming of molecular function by the oocyte cytoplasm, and the nuclear transplantation procedure became the prototype for cloning metazoan animals.

To understand how genes control embryonic development, Briggs initiated a program on the effect of maternal gene products in the oocyte on the development of the embryo of the Mexican axolotl, a salamander. These studies were performed by analyzing the embryological, cellular, and molecular changes in embryos developing from oocytes whose mothers carried mutations. Thus, the abnormal gene products produced in the growing oocytes revealed how oocyte gene products control the initial stages of embryogenesis. This research in amphibia was one of the initial studies that revealed how maternal gene products control early pattern formation.

I should point out why I was asked to write this memoir of Bob Briggs. I knew Bob for thirty-five years, first joining his laboratory in 1948, just two years before he embarked on the nuclear transplantation experiments. By 1950 he had recruited Tom King, then a research fellow, to collaborate on the project, and in 1952 they had their first success. Later, in the 1950s and early 1960s, I had the pleasure of collaborating with Bob on some of the nuclear transfer studies and during his years at Indiana University (1956–83) I maintained contact with him. When he became research professor emeritus, he remarked that he felt like a postdoctoral fellow—he could now enjoy research with no other responsibilities. Unfortunately, he died approximately a year later.

Much of what I know of Bob stems from working with him, listening to his anecdotes at 4:00 p.m. tea breaks in Philadelphia, and the contact I had with him in later years when he was in Bloomington. Quotations that follow came from an interview conducted by Elizabeth Knight Patterson (no date) that were incorporated in her book.

EARLY PERSONAL HISTORY

Robert William ("Bob") Briggs was born in Watertown, Massachusetts, in 1911. When he was less than two years old, his mother and brother died of tuberculosis, and he was raised by his grandparents (1913–29) in Epping, New Hampshire, a small town of about 1,600 people located in the southeastern part of the state. He grew up with his uncles and aunts, one of whom was only ten years older than Bob. He had a happy childhood, as "there was an enviable stability and security in the social structure." Although his family "was rather poor like most of the other families," they did own a piano, and Bob took lessons from an aunt for several years. He recalled, "I drifted away from the piano, but the influence was a permanent one, and music has been a part of my life in one way or another ever since."

At fourteen, he began to work in the summer at the local shoe factory. In the winter, he earned money "as a banjo player in a small dance band that played two to three nights a week for dances in southeastern New Hampshire towns." Bob credits a teacher in high school for leading him into biology. "The teacher turned the students loose on projects of their own." Bob collected minnows, frogs, insects, worms, plants, etc., and studied them under magnifying glasses and a borrowed microscope. "The effect of merely looking at life at a different level was a lasting one. At the time it never occurred to me that I would become a biologist; I didn't even know that one could earn one's living this way."

After high school, Bob left home for Boston, where he "got a job working nights and attended classes by day at Boston University." Initially, he enrolled in the College of Business Administration to prepare himself to make a living. His lack of interest in those courses led him to take some science courses in the College of Liberal Arts. Still concerned about making a living, he also took courses in the School of Education. In 1934 he graduated with a B.S. and, firmly convinced that his future was in science, went to graduate school at Harvard University. Under the sponsorship of Leigh Hoadley, Bob "made a detailed analysis of changes in metabolic rate and density during the development of the frog." During graduate school he was an Austin teaching fellow in biology (1935–36), held an assistantship (1936–38), and continued his night job. In 1938 he received his Ph.D.

RESEARCH CONTRIBUTIONS

The contributions of Robert Briggs to developmental biology spanned over four decades and comprised four main periods of pioneering research in amphibian development, involving neoplasia, ploidy, nuclear transplantation, and maternal genes. After receiving his Ph.D. degree, he became a fellow in the Zoology Department at McGill University (1938–42). Here he initiated his first period, the characterization of tumor growth in the developing frog, for he recognized the importance of studying the behavior of tumors in the organization fields operative in developing systems. He was the first to induce tumors in a developing system, the larvae of *Rana pipiens,* and did so with a carcinogenic agent (1940). Also, he was the first to examine the effect of a developing organism on a malignant tumor. He transplanted fragments of the frog renal adenocarcinoma (Lucké tumor) to various sites of the larva and found that they grew well, but regressed prior to metamorphosis. He also found that good growths regressed even in tadpoles in which metamorphosis was prevented by removing the pituitary or thyroid gland (1943). He suggested that regression of this malignant tumor might be "an expression of the development of tissue specificity." Extensions of this research can be found today in studies of the development of immunocompetence, tumor immunosurveillance, and attempts to normalize cancer cells in embryonic systems.

In his second period he focused on the role of the nucleus in development. This occurred in 1942 after he joined the Lankenau Hospital Research Institute (later the Institute for Cancer Research and now the Fox Chase Cancer Center) in Philadelphia. First, he developed a method for producing anuran triploids with heat shock and analyzed the effect of ploidy on development. He found that the triploids developed normally (1947), except female gonads usually reversed to testes (1950). One practical outcome of this work was the availability of a triploid marker later to be used widely in frog embryos for various types of studies. The study on sex reversal in anuran triploids was done in collaboration with Rufus R.Humphrey and Gerhard Fankhauser. His association with Professor Humphrey later culminated in a research program in amphibian developmental genetics at Indiana University.

His investigation of the haploid syndrome showed that reduction of egg cytoplasm decreased the severity of the haploid syndrome, but it did not overcome the abnormalities (1949). This work showed that the nucleocytoplasmic ratio played a role in the haploid syndrome, but it suggested that deleterious genes were mainly responsible for the haploid abnormalities. Next, the production and analysis of embryos lacking functional chromosomes showed that anuran embryos lacking a functional nucleus but containing a normal cleavage center can develop into partially cleaved blastulae (1951). This study, predating the explosion of the molecular biology of embryos, indicated that gene products formed during amphibian oogenesis are sufficient to support cleavage, but post-blastula development requires new gene products. In addition, this study laid the foundation for the interpretation of nuclear transplantation experiments that occupied his third period of research.

In 1952 in collaboration with Thomas J.King, Briggs pioneered the development of the technique of amphibian nuclear transplantation in determining whether somatic nuclei remain equivalent to the zygote nucleus in developmental potential during embryogenesis, a question posed previously by H.Spemann and others. Briggs and King initially focused on cell nuclei from undetermined regions of the embryo and showed that, after transplantation singly into enucleated frog eggs *(R.pipiens),* many of these nuclei directed the eggs to develop into normal tadpoles (1952) and in a later study into normal metamorphosed frogs (1960). This was the first time successful nuclear transplantation had been accomplished in metazoans. Subsequently, they tested nuclei up to tailbud stages and found that simultaneously with cell differentiation there is a progressive decrease in the percentage of nuclei capable of supporting normal development (1977). The importance of this technique was immediately recognized, and Bob generously hosted in his laboratory numerous scientists to help them learn the procedure. Soon various laboratories around the world applied this technique to different amphibian species and confirmed the decreased developmental potential of most nuclei concurrently with advancing embryogenesis.

The conservative conclusion in the classic 1952 paper was that “although the method of nuclear transplantation should be valuable principally for the study of nuclear differentiation, it may also have other uses.” Some of its applications have been the analysis of haploidy, hybrid incompatibility, cancer, immunobiology, and cellular aging. It provided insight into the cytoplasmic control of nuclear and gene function, including reprogramming of nuclear and gene function. Most importantly, nuclear transfer became the prototype for cloning metazoan organisms and was extended to insects, fish, and mammals. In 1997 the first metazoan animal (a lamb, Dolly) was cloned from an adult cell, and this was followed in 1998 by the cloning of mice and calves from adult cells. During this period, the first transgenic lambs carrying the human gene (clotting factor IX) were cloned from fetal cells and the first transgenic calves were cloned also from fetal cells. The fundamental research begun in 1952 will now be translated into important biomedical and agricultural applications.

In 1956 Bob Briggs resigned from his post as head of the Embryology Department at the Institute for Cancer Research and became professor of zoology at Indiana University. He then embarked on his fourth and final period of research, the establishment of amphibian developmental genetics. He had been convinced for some time that the gap between embryology and genetics needed to be bridged in order to understand how the nucleus interacts with the cytoplasm in directing embryonic development. Recognizing the importance of the genetic lines of Mexican axolotl *(Ambystoma mexicanum)* that Professor Rufus Humphrey had developed, Bob recruited Humphrey to Indiana University soon after Humphrey retired from his post at the University of Buffalo Medical School (now the State University of New York at Buffalo). Humphrey became research scholar in the Department of Zoology, and together they built the research program in the developmental genetics of axolotl.

Briggs realized that the developmental genetics of early development would be revealed best by mutations showing a maternal effect (i.e., those that were expressed in the embryo regardless of the normal genes contributed by the sperm). Previous experimental embryologists had shown that the pattern of early development is controlled by morpho genetic substances produced during oogenesis and present in the egg cytoplasm at fertilization. Genes that exerted maternal effects through modifications of the egg cytoplasm were therefore of special interest, as they provided a means to study how the egg cytoplasm acts to control early embryonic development. Several such genes and others that act later were found by Humphrey. For example, four mutations cause early arrest. One in particular, the o^+ gene, produced a substance during oogenesis that is required for development beyond gastrulation. Injections of cytoplasm or nucleoplasm of germinal vesicles from normal oocytes into mutant eggs corrected the deficiency, resulting in normal development (1966). Eight other genes exerted specific effects on embryonic organs, whereas four caused alterations in pigment cells and four did so in nucleoli (1973).

The action of these mutant genes on development was elucidated by various methods (cytological, biochemical, embryological, molecular, and physiological) by Briggs, Humphrey, students, and others. In his 1973 review, Briggs credits especially the pre- and postdoctoral students, who in many cases published their findings independently. This was the policy of Bob, who gladly counseled students, but encouraged them to develop on their own. Various axolotl mutants and others to be discovered were supplied to other investigators for their research projects, and this continues today at the axolotl colony of Indiana University. The studies on maternal genes initiated in the 1950s provided a background and direction for the elegant molecular genetic experiments of others to follow in *Drosophila, Xenopus,* zebrafish, chordates, and invertebrates, in which many genes contributing to pattern formation have been identified and, in the best cases, several genes acting in a specific biochemical pathway have been recognized and elucidated.

Bob retired in 1982 and became research professor emeri tus at Indiana University. He continued his research at this time on a newly discovered temperature-sensitive mutant in the axolotl. One of the projects was completed before his death from kidney cancer on March 4, 1983, and was published posthumously (1984). He died in the Krannert Pavilion of the Indiana University School of Medicine in Indianapolis, and was survived by his second wife Francoise and two sons and a daughter: Evan of Bloomington, Indiana; Alexander of Hillsdale, New York; and Meredith Briggs Skeah of Green Village, New Jersey. His former wife, Janet Bloch Briggs of Hillsdale, and mother of his children also survived him.

HONORS AND OTHER CONTRIBUTIONS

Bob Briggs was the recipient of various honors, including election to the American Academy of Arts and Sciences (1960) and the National Academy of Sciences (1962). He was named research professor of zoology at Indiana University (1963) and fellow of the International Institute of Embryology. He was awarded honorary doctorate degrees by the Medical College of Pennsylvania (1971) and Indiana University (1983). In 1973 the French Academy of Sciences awarded him and Thomas J.King the Charles-Leopold Mayer Prize for their pioneering studies in amphibian nuclear transplantation. During his career he participated in many major symposia, served on editorial boards of important journals, and provided intellectual leadership as chair of zoology (1969–72) at Indiana University.

CODA

Bob Briggs will be remembered both as an outstanding scientist and a generous and cordial human being. He left a legacy not only of pioneering research but also a legacy of numerous problems for other investigators to pursue. He enjoyed life and had numerous hobbies, including listener and performer of classical music, as well as golfer, bowler, and sports car and motorcycle enthusiast. His earlier interest in playing classical music on the piano was followed by playing the recorder. As early as the 1950s, he owned an Austin-Healy, later a Corvette, and finally a top-of-the-line BMW motorcycle. He frequently shared these pastimes with students and colleagues, including his weekly Sunday morning golf.

REFERENCES

Etkin, L.D. 1998. Personal communication.
Justus, J. 1998. Personal communication.
Malacinski, G.M. 1998. Personal communication.
Patterson, E.K. No date. *Growth—The Early History of a Cancer Research Institute (1927–1957)*. Philadelphia: Talbot Research Library of the Fox Chase Cancer Center and American Philosophical Society.

SELECTED BIBLIOGRAPHY

1940 Tumour induction in *Rana pipiens* tadpoles. *Nature* 146:29.

1942 Transplantation of kidney carcinoma from adult frogs to tadpoles. *Cancer Res.* 2:309–23.

1943 With R.Grant. Growth and regression of frog kidney carcinoma transplanted into the tails of permanent and normal tadpoles. *Cancer Res.* 3:613–20.

1947 The experimental production and development of triploid frog embryos. *J. Exp. Zool.* 106:237–66.

1949 The influence of egg volume on the development of haploid and diploid embryos of the frog, *Rana pipiens. J. Exp. Zool.* 111:255–94.

1950 With R.R.Humphrey and G.Fankhauser. Sex differentiation in triploid *Rana pipiens* larvae and the subsequent reversal of females to males. *J. Exp. Zool.* 115:399–428.

1951 With E.U.Green and T.J.King. An investigation of the capacity for cleavage and differentiation in *Rana pipiens* eggs lacking “functional” chromosomes. *J. Exp. Zool.* 116:455–99.

1952 With T.J.King. Transplantation of living nuclei from blastula cells into enucleated frogs' eggs. *Proc. Natl. Acad. Sci. U.S.A.* 38:455-63.

1954 With T.J.King. Transplantation of living nuclei of late gastrulae into enucleated eggs of *Rana pipiens. J. Embryol. Exp. Morphol.* 2:73–80.

1955 With T.J.King. Changes in the nuclei of differentiating gastrula cells, as demonstrated by nuclear transplantation . *Proc. Natl. Acad. Sci. U.S.A.* 41:321–25.

With T.J.King. Specificity of nuclear function in embryonic development. In *Biological Specificity and Growth,* ed. E.G.Butler, pp. 207–28. Princeton, N.J.: Princeton University Press.

1956 With T.J.King. Serial transplantation of embryonic nuclei. *Cold Spring Harb. Symp. Quant. Biol.* 21:271–90.

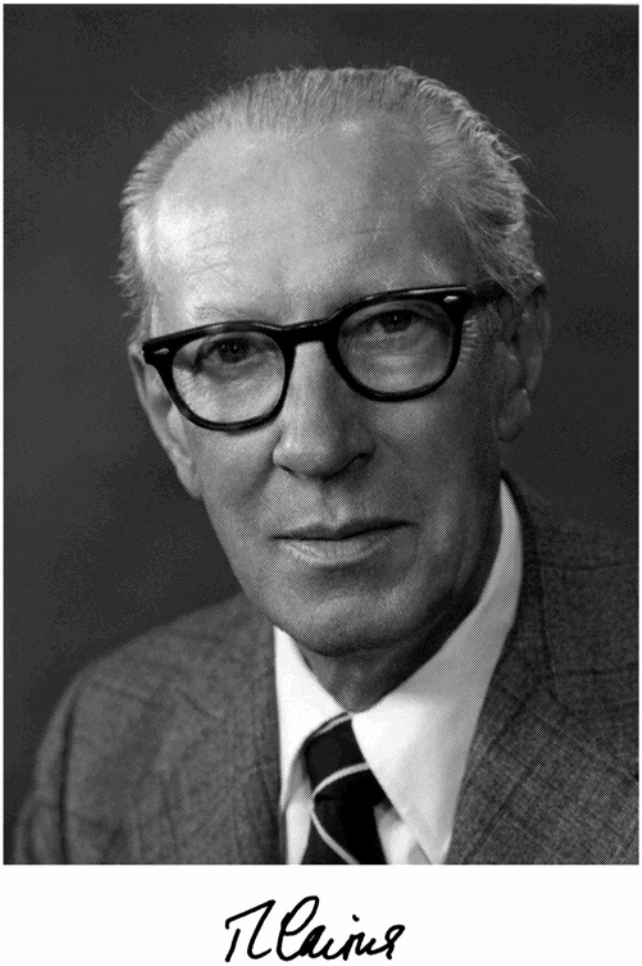
1957 With T.J.King. Changes in the nuclei of differentiating endoderm cells as revealed by nuclear transplantation. *J. Morphol.* 100:269– 312.

1959 With T.J.King. Nucleocytoplasmic interactions in eggs and embryos. In *The Cell,* vol. 1, eds. J.Brachet and A.E.Mirsky, pp. 537– 617. New York: Academic Press.

1960 With T.J.King. Nuclear transplantation studies on the early gastrula *(Rana pipiens)*. I. Nuclei of presumptive endoderm. *Dev. Biol.* 2:252–70.

1961 With T.J.King and M.A.Di Berardino. Development of nuclear-transplant embryos of known chromosome complement following parabiosis with normal embryos. In *Symposium on Germ Cells and Earliest Stages of Development,* ed. S.Ranzi, pp. 441–77. Milan: Fondazione A.Baselli. Istituto Lombardo.

1962 With J.Signoret and R.R.Humphrey. Nuclear transplantation in the axolotl. *Dev. Biol.* 134–64.
1966 With G.Cassens. Accumulation in the oocyte nucleus of a gene product essential for embryonic development beyond gastrulation. *Proc. Natl. Acad. Sci. U.S.A.* 55:1103–09.
1968 With J.T.Justus. Partial characterization of the component from normal eggs which corrects the maternal effect of gene *o* in the Mexican axolotl *(Ambystoma mexicanum). J. Exp. Zool.* 167:105–15.
1969 Genetic control of early embryonic development in the Mexican axolotl, *Ambystoma mexicanum. Ann. Embryol. Morphog.* 1(suppl.):105– 13.
1972 Further studies on the maternal effect of the *o* gene in the Mexican axolotl. *J. Exp. Zool.* 181:271–80.
1973 Developmental genetics of the axolotl. In *Genetic Mechanisms of Development,* ed. F.H.Ruddle, pp. 169–99. New York: Academic Press.
1975 With H.-M.Chung. Experimental studies on a lethal gene *(l)* in the Mexican axolotl, *Ambystoma mexicanum. J. Exp. Zool.* 191:33–47.
1977 Genetics of cell type determination. In *Cell Interactions in Differentiation,* eds. M.Saxen and L.Weiss, pp. 23–43. New York: Academic Press.
1984 With F.Briggs. Discovery and initial characterization of a new conditional (temperature-sensitive) maternal effect mutation in the axolotl. *Differentiation* 26:176–81.



Courtesy of E.I. du Pont de Nemours, Wilmington, Delaware

THEODORE L.CAIRNS

July 20, 1914–September 26, 1994

BY BLAINE C.MCKUSICK

THEODORE L.CAIRNS, commonly known as "Ted," was a DuPont Company research scientist who made important contributions to the science of chemistry, applications of chemistry, and U.S. scientific policy. He spent thirty-eight years in DuPont's Central Research Department, the last eight as its director.

Cairns was born in Canada in the city of Edmonton, Alberta. He attended Edmonton public schools and then entered the University of Alberta in 1932 as a chemistry major. He graduated with a B.S. in 1936. He showed an aptitude for research even as an undergraduate, co-publishing a paper on aminobiphenyls based on research done under the direction of Professor Reuben B.Sandin.

About a year before graduation, he met Margaret Jean McDonald, a fellow University of Alberta student majoring in home economics. The scene of their initial meeting—a smelly chemistry laboratory—was not especially romantic. His ownership of a rumble-seated car, which he had purchased for twenty-five dollars, perhaps impressed Margaret. They often dated during their senior year at the university.

Cairns had decided that opportunities for chemists were greater in the United States than in Canada and sought Sandin's help in gaining admittance to an American gradu ate school. Sandin recognized Cairns's potential as a chemist and recommended him to the renowned Professor Roger Adams of the University of Illinois Chemistry Department. Cairns was admitted to that department in the fall of 1936, and he promptly started to work with Professor Adams on the stereochemistry of substituted biphenyls. The research went well, and Cairns received his doctorate in 1939 after only three years, instead of the normal four.

Academia beckoned, and in the fall of 1939, after working that summer in the laboratories of the Eastman Kodak Co., Cairns joined the faculty of the Chemistry Department at the University of Rochester as an instructor.

Cairns and Margaret McDonald had not been able to see much of each other after their graduation from the University of Alberta (she was working in a Baltimore hospital). However, they corresponded regularly, and they married in Toronto in 1940. Their first child John was born in 1941; by that time Cairns had become a U.S. citizen, and Margaret followed suit a year later.

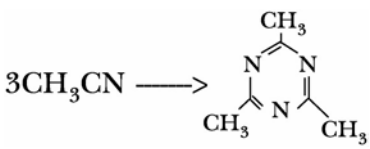
Life as a professor seemed less attractive close up than from a distance, and Ted's former professor, Roger Adams, long a valued consultant to the DuPont Company, painted a bright picture of research opportunities there. Indeed, opportunities were very good, for Wallace Carothers and DuPont colleagues had recently discovered the first practical synthetic fiber (nylon) and the first practical synthetic rubber (neoprene). Cairns visited the laboratories of the DuPont Experimental Station in Wilmington, Delaware, was favorably impressed by the chemists he met and the facilities he saw, and left the University of Rochester to join DuPont in 1941, a few months before the United States entered World War II.

At the time Cairns came to DuPont, the importance of nylon was well recognized there. It seemed that its chemi cal modification might open up new uses for it, and Cairns studied its modification by formaldehyde and other reactants. Some of the work was instigated by wartime needs for nylon with special properties. Interesting, patentable results were obtained.

With the coming of peace, expansion of research became possible, and the DuPont Experimental Station grew rapidly. With the expansion came a need for strong, capable research leaders, and Cairns soon found himself the leader of a group of eight or so Ph.D. chemists seeking useful applications of chemistry. His group looked for a new chemistry of cheap, reactive raw materials such as acetylene, ethylene, carbon monoxide, hydrogen sulfide, and hydrogen cyanide. Thus they turned up N-methylol polyamides by the reaction of formaldehyde with polyamides.

$$\mathrm{\text{-}(CO(CH_2)_4CONH(CH_2)_6NH)\text{-}_n} \xrightarrow{\mathrm{CH_20}} \mathrm{\text{-}(CO(CH_2)_4CO}\underset{\mathrm{CH_2OH}}{\mathrm{N}}\mathrm{H(CH_2)_6NH)\text{-}_n}$$

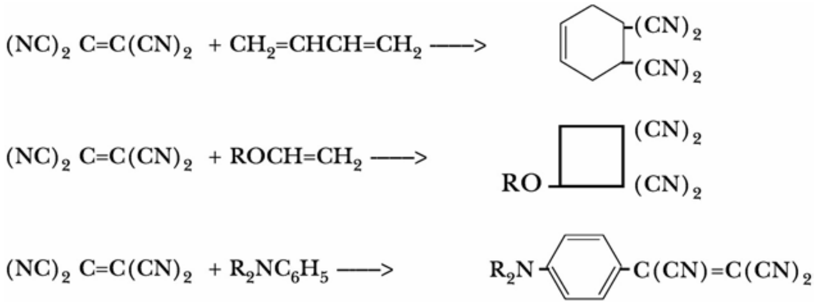
In examining the effect of very high pressure on chemicals, they found that a pressure of 8000 atmospheres converted nitriles to s-triazines.



Such pressures on mixtures of ketones and hydrogen sulfide provided gem-dithols, previously unknown.

$$\mathrm{R_2C{=}0 + 2H_2S \longrightarrow R_2C(SH)_2 + H_20}$$

Impressed by the properties of poly(tetrafluoroethylene), or Teflon, discovered elsewhere in DuPont, Cairns proposed the synthesis and polymerization of the as yet unknown tetracyanoethylene. Its initial synthesis was not easy, but once that hurdle was passed, it proved a very reactive, versatile chemical. Although it failed as a source of polymers, it formed six-membered ring adducts with 1,3-dienes, four-membered ring adducts with vinyl ethers, brilliantly colored tricyanovinyl dyes with aromatic amines, and many other classes of products.



Tetracyanoethylene readily forms an anion radical, for example, by reaction with potassium:

$$(NC)_2C{=}C(CN)_2 + K \longrightarrow K^+ \ (NC)_2\dot{C} - \ddot{C}(CN)_2$$

This anion radical had unanticipated stability, permitting isolation of various of its salts with interesting electronic, optical, and magnetic properties. These salts have been the subject of widespread studies for the past thirty years. The decamethylferrocenium salt was the first molecule-based ferromagnetic material ever characterized. Its critical temperature was only 4.8°K, but a salt prepared from dibenzene-vanadium is ferromagnetic above room temperature. An extraordinary variety of magnetic properties is available from the radical anions of TCNE and other cyanocarbons. This subject was reviewed in *Chemical and Engineering News* fairly recently.[1]

Cairns was gradually given greater responsibility in DuPont, becoming the laboratory director of the Central Research Department in 1952, its research director in 1966, and director of the entire department in 1971. When the Central Research Department merged with DuPont's Development Department in 1977, Cairns became director of the resultant Research and Development Department, an organization of hundreds of chemists and engineers devoted to discovering new chemistry and developing practical applications for it.

Cairns retired in 1979, with a multitude of his co-workers of the preceding thirty-eight years jamming the DuPont Country Club ballroom to demonstrate their friendship and admiration for him. He had been an inspiring leader who, as his long-time colleague Robert M.Joyce has pointed out, “was an inspiring leader with a sharp eye for spotting chemical talent and a great sense of putting the right person in the right job.”

Cairns participated in many professional activities, especially in the field of chemical publication. He was on the Editorial Board of *Organic Syntheses* (1949–56) and then served on its Board of Directors for several years. He subsequently worked similarly for its sister publication *Organic Reactions,* serving on its Editorial Board from 1960 to 1969. He played a truly vital role for *Organic Reactions* from 1967 to 1969. Its editor-in-chief Arthur Cope suddenly died in 1967. With great uncertainty as to the publishing plans and commitments that Cope had made, none of the other editors was willing to take Cope's place. Cairns, unwilling to see this useful publication die, became its unofficial editor-in-chief until William Dauben of the University of California, Berkeley, with urging from Roger Adams and Cairns, accepted the job in 1969. His acceptance was just in time to see that volume 17 was issued and that this important chemical publication got back on its feet. Cairns remained on the Advisory Board of *Organic Reactions* for several years, during which time he co-authored an important chapter on “Cyclopropanes from Unsaturated Compounds, Methylene Iodide, and Zinc-Copper Couple.”[2]

Cairns was on the Board of Editors of the *Journal of Organic Chemistry* from 1965 to 1970. He was active in the American Chemical Society both locally and nationally. He was on the Executive Committee of its Organic Division in 1955–56, its chairman in 1964–65, and represented it on the American Chemical Society Council during most of the period 1955–65.

He was elected to the National Academy of Sciences in 1966 after having served on one of its most important committees, the Committee for the Survey of Chemistry, in 1964– 65. This committee produced a definitive and influential assessment of basic research in chemistry in the United States.

His broad experience and knowledge in science and technology was put to use through membership on several important government committees:

The Delaware Governor's Council on Science and Technology, 1969–72

President Nixon's Science Policy Task Force, 1969

The President's Science Advisory Committee, 1970–73

The President's Committee on the National Medal of Science, 1974–75

The Polytechnic Institute of New York Advisory Council for Chemistry, 1976–78

For several years Cairns chaired the Division of Chemistry and Chemical Technology of the National Research Council. His accomplishments were recognized by several awards:

The City of Wilmington's Outstanding Citizen Award, 1963

The American Chemical Society Award for Creative Work in Synthetic Organic Chemistry, 1968

SOCMA (Society of Chemical Manufacturers Association) Medal for Creative Research in Synthetic Organic Chemistry, 1968

Honorary Doctor of Laws degree, University of Alberta, 1970

Perkin Medal, American section of the Society of Chemical Industry, 1973

Cresson Medal, The Franklin Institute, 1974

Cairns would use the occasion of an award to express views on the progress of technology and the future of research, thereby influencing both. For example, on receiving the Perkin Medal of the Society of Chemical Industry in New York in 1973, the topic of his address was "The Environment for Industrial Research." He noted the importance of investigation to improve product lines and processes and to find alternative raw materials to improve quality or lower mill costs. However, he stressed the value of searching for new products and new ventures to be at the heart of business ten to fifteen years in the future. He concluded by saying that "the world offers no end of difficult problems to be solved and will be glad to try whatever solutions we can provide at a reasonable price."

Ted and Margaret Cairns had four children: John A., a Minneapolis lawyer; Margaret Etter, a professor of organic chemistry, crystallography, and solid state interactions at the University of Minnesota, who died in 1992; Elizabeth Reveal, a Washington, D.C., financial adviser to local governments; and James R., a manager of trust accounts for a Philadelphia bank. The family was always close knit. As the children were growing up, the family did many things together, such as tennis, skating, gardening, and travel.

After retirement Cairns continued to follow the course of chemistry and other sciences with interest, but he seldom played an active role. He occasionally attended scientific meetings, such as dinner meetings of the editors of

Organic Syntheses or *Organic Reactions,* when the meetings happened to be nearby. He at last had time to pursue his hobby of gardening, especially the raising of unusual varieties of dahlias. However, age gradually caught up with him, and on September 26, 1994, he died in Wilmington at age eighty. Besides his wife and three children, Cairns was survived by a sister, Eleanor Cairns Everington of Stony Plain, Alberta; eight grandchildren; and three great-grandchildren.

Robert M.Joyce was a close friend and colleague of Cairns for four decades, beginning in graduate school days at the University of Illinois and extending through extensive collaboration in the DuPont Company. He well described Cairns as "an inspiring leader with a sharp eye for spotting chemical talent and a great sense for putting the right person in the right job."[3]

NOTES

1. J.S.Miller and A.J.Epstein, "Designer Magnets," *Chem. Eng. News* 73 (No. 40) (Oct. 2, 1995):30–41.
2. Cyclopropanes from unsaturated compounds, methylene iodide, and zinc-copper couple. *Org. React.* 20(1973):1–131.
3. R.M.Joyce. "Theodore L.Cairns," *Org. React.* 47(1995):vii– viii.

SELECTED BIBLIOGRAPHY

1936 With R.B.Sandin. Formation of cyclic azo compounds from 2,2'-diaminobiphenyls. *J. Am. Chem. Soc.* 58:2019.

1939 With R.Adams. 2-substituted biphenyls. *J. Am. Chem. Soc.* 61:2179.

1946 N-methylol polyamides. U.S. Patent 2,393,972.

1947 Polyamide/formaldehyde reactions and products thereof. U.S. Patent 2,430,860.

N-alkoxymethyl polyamides. U.S. Patent 2,430,908.

1948 Polyamides. U.S. Patent 2,441,057.

With R.E.Benson. Chemical reactions of caprolactam. *J. Am. Chem. Soc.* 70:2115.

1949 With H.D.Foster, A.W.Larchar, A.K.Schneider, and R.S.Schreiber. Preparation and properties of N-methylol, N-alkoxymethyl, and N-alkylthiomethyl polyamides. *J. Am. Chem. Soc.* 71:665.

1950 With A.W.Larchar and B.C.McKusick. High-pressure synthesis of s-triazines. U.S. Patent 2,503,999.

With R.E.Benson. Some new reactions of cyclooctatetraene. *J. Am. Chem. Soc.* 72:5355.

1951 N-vinylalkyleneureas and polymers thereof. U.S. Patent 2,541,152.

1952 With V.A.Engelhardt, H.L.Jackson, G.H.Kalb, and J.C.Sauer. The reaction of acetylene with acrylic compounds. *J. Am. Chem. Soc.* 74:5636.

With G.L.Evans, A.W.Larchar, and B.C.McKusick. Gem-dithiols. *J. Am. Chem. Soc.* 74:3982.

With A.W.Larchar and B.C.McKusick. The trimerization of nitriles at high pressures. *J. Am. Chem. Soc.* 74:3633.

1954 With D.D.Coffman, R.Cramer, A.W.Larchar, and B.C.McKusick. Olefin-carbon monoxide-alcohol copolymers. *J. Am. Chem. Soc.* 76:3024.

1957 With others. Cyanocarbon chemistry: Synthesis and chemistry of tetracyanoethylene . *J. Am. Chem. Soc.* 79:2340.

1958 With others. Preparation and reactions of tetracyanoethylene. *J. Am. Chem. Soc.* 80:2775.

With B.C.McKusick, R.E.Heckert, D.D.Coffman, and H.F. Mower. Cyanocarbon chemistry. VI. Tricyanovinylamines. *J. Am. Chem. Soc.* 80:2806.

1960 With C.G.Krespan and B.C.McKusick. Dithietene and bicyclooctatriene ring systems from bis-(fluoroalkyl) acetylenes. *J. Am. Chem. Soc.* 82:1515.

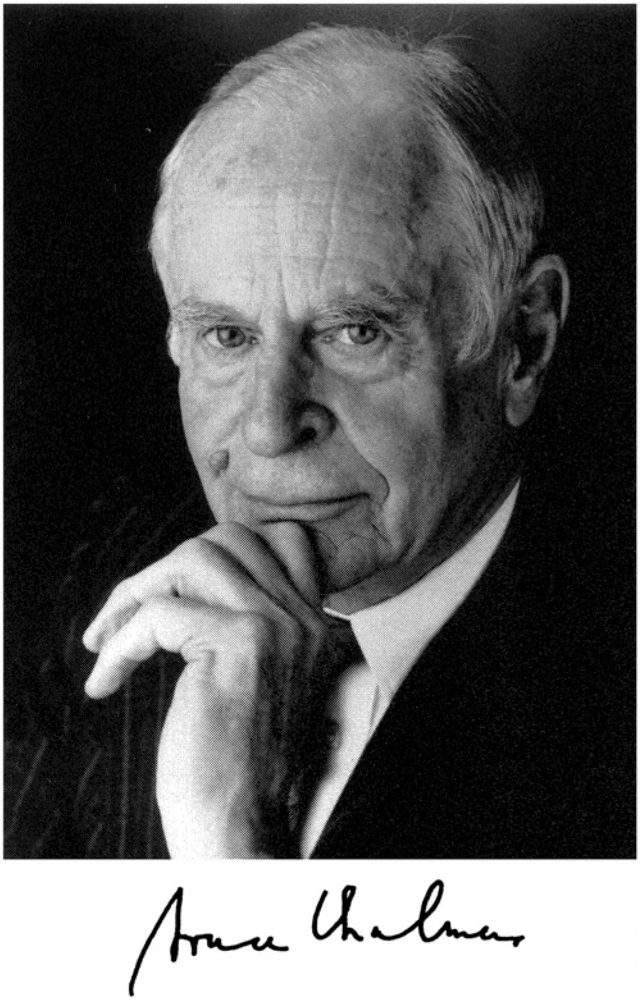
1961 With C.G.Krespan and B.C.McKusick. Bis-(polyfluoroalkyl) acetylenes. II. Bicyclooctatrienes through 1,4-addition of bis-(polyfluoro)acetylenes to aromatic rings. *J. Am. Chem. Soc.* 83:3428.

With B.C.McKusick. Cyanocarbon chemistry. *Angew. Chem.* 73:520.

1962 With D.R.Eaton, A.D.Josey, R.E.Benson, and W.D.Phillips. Unpaired electron distribution in pi-systems. *J. Am. Chem. Soc.* 84:4100.

1963 With B.Graham and H.G.Tanner. Radiation grafting onto preswollen polymers. U.S. Patent 3,101,275.

1964 With E.G.McGeer. Colored 1:1 pi complexes of tetracyanoethylene and aromatic compounds. U.S. Patent 3,140,308.
1965 With E.Graef. Tetracyanoethylene. U.S. Patent 3,166,584.



BRUCE CHALMERS

October 15, 1907–May 25, 1990

BY DAVID TURNBULL

BRUCE CHALMERS HAD A notable career as a scientist, educator, and editor. He outlined his career in his professional biography, which was published in the thirtieth anniversary volume[1] of "Progress in Materials Science," a series for which he was the founder-editor. This volume, consisting of articles by some of his former students and professional colleagues, was published to honor him. I have relied heavily on that account[2] in preparing this memoir.

Bruce was born in 1907, a son of Stephen and Clara (Rosenhain) Chalmers, and was reared in London. His father, a descendent of the Scottish Camerons, was a mathematics teacher; he died in 1919, when Bruce was twelve years old. Bruce's inclination toward science developed quite early and was stimulated in part by his father and especially his older brother Alan, who became a physicist and professor at the University of Durham. However, the major influence on his choice of career was that of his maternal uncle, Walter Rosenhain, a leading metallurgist who, during World War I, was superintendent of the Department of Metallurgy and Metallurgical Chemistry in the National Physical Laboratory. Rosenhain became well known for developing one of the earliest models for intercrystalline boundary struc ture. It is interesting that Bruce came, by a somewhat circuitous route, to play a leading role in developing the much more sophisticated modern theory for such structures.

In a review published in 1917,[3] Rosenhain wrote with great clarity and eloquence of the emergence of a “New Metallurgy” based on fundamental research in chemistry and physics. Bruce's career contributed greatly to the development of strong bonds between metallurgy and physics and chemistry, and the continued advancement of the “New Metallurgy”, but with somewhat more emphasis on the metallurgy bond to physics than to chemistry.

Bruce lived at home throughout his secondary and university training. He attended University College of London University and earned a B.Sc. in physics in two years, by-passing the normally required third year. He was accepted as a Ph.D. student by Professor of Physics E.N. DaC. Andrade. Bruce was highly inspired by Andrade, both for his achievements as a scientist and as an educator. Andrade in 1910 “was one of the first to recognize that the mechanical behavior of metals could properly be regarded as a problem in physics.” He discovered the $t^{1/3}$ law of creep. For his Ph.D. thesis, Andrade suggested that Bruce investigate the change in resistivity accompanying the creep of metals with a hexagonal crystal structure, such as cadmium. Indeed, the resistivity did change with deformation, reflecting the change in crystallographic orientation with slip. In connection with his investigation, Bruce had to put together X-ray diffraction equipment, which was then lacking in the physics department. Bruce learned much from Andrade about how a professor and research student should interact most effectively. Bruce greatly admired Andrade's way with research students, which was to have them *work* with him rather than *for* him and to allocate credit fairly for any discoveries.

Bruce received his Ph.D. degree in 1932 during the depth of the Great Depression, when positions were difficult to find. After a year of postdoctoral study he was appointed lecturer in physics at Sir John Cass College of London University, a technical institute that mainly served part-time evening students who had taken industrial jobs immediately following their secondary education. Bruce taught in the evenings five times weekly, so his days were more or less free for research.

At this time, many physicists shared Andrade's interest in the plastic properties of single crystals, and G.I.Taylor, Egon Orowan, and others were developing the dislocation models for plastic flow. Bruce was drawn into these activities, and he devised high precision measurements of plastic creep rates of single crystals at low stress levels. He developed a simple method of growing tin single crystals with any specified crystallographic orientation. Understanding the flow behavior of single crystals would be essential to interpreting that of the more complicated polycrystalline solids. In the course of his investigations, Bruce found that one of his presumed single crystals actually was composed of two crystals separated by a boundary along the entire length of the cylindrical specimen. From visual and optical microscope observations, he noted that all the crystals he grew exhibited sub-boundaries and other imperfections. The origin, structure, and property effects of these imperfections fascinated Bruce, and they became the focus of his research throughout his career.

After five years at Cass, Bruce accepted a position as physicist at the Tin Research Institute, a laboratory sponsored by the International Tin Research and Development Council. In addition to his studies of the mechanical behavior of tin, he investigated the physics of the process of making tin plate by dipping steel into molten tin. He was required to develop methods for examining the micro-topography of the surface of the tin layer and to do theoretical work on the origin of the porosity often present in the layer.

In 1938 Bruce married Ema Arnouts, who was a warm and supportive companion throughout his life. They became the parents of one son, Stephen, and four daughters, Carol, Jane, Alison, and Heather.

Soon after the beginning of World War II, Bruce joined the metals research section of the British Ministry of Supply, and he investigated the heat treatment of armor piercing shot and the non-destructive evaluation of their quality. Early in 1944 he was appointed head of the Metallurgy Division of the Royal Aircraft Establishment at Farnborough, where he was concerned with problems of materials failures, as in aircraft crashes, and development of alloys with high strength and low density. These problems led him more deeply into the general area of structure-property relations, which are central to physical metallurgy.

In 1946 he joined the Atomic Energy Research Establishment opening at Harwell as head of its Metallurgy Division. There he formulated a program directed at the development of nuclear reactor materials and assembled a research staff to carry it out. His staff members remember him as an inspiring leader, but he found the burdens and bureaucratic controversies attending administration quite distasteful. Having enjoyed his teaching experience at Cass, he was attracted to the possibility of returning to academia.

This prospect soon materialized, and in 1948 he became a professor of physical metallurgy at the University of Toronto. At Toronto he attracted a large group of students in whom he aroused a great enthusiasm for metallurgical research. It was his practice to have most of his research group accompany him to National Metallurgical Society meetings in the United States, where Bruce introduced them to leading scientists. The students eagerly attended the technical sessions and at intersessions boldly assailed the speakers with ideas and often-embarrassing questions about their results.

This practice of promoting student participation in scientific meetings reflects his sagacity as an educator and the close, cordial relations he always had with his students at Toronto and later at Harvard. He and his students performed the experiments and analyses that laid the foundations of our present understanding of the origin and nature of the grain and subgrain morphology formed in the crystallization of liquids. Their analyses took due account of the heat and material transport and interface movement and morphology attending crystallization. Especially important was their concept of “constitutional undercooling”, which accounted for the role of impurities in the development of cellular and dendritic structures. He and student Karl Aust at Toronto grew sets of tin and lead bicrystals with a range of misorientations and measured the relative grain boundary energy dependences on the crystallographic misorientations. These energies and those of FeSi alloys measured independently by C.B.Dunn at General Electric were in remarkable agreement with the predictions of the dislocation model for tilt-type grain boundaries developed at Bell Labs by W.T.Read and William Shockley.

In 1953 Bruce accepted appointment as Gordon McKay professor of metallurgy in the Division of Applied Sciences at Harvard University. This position attracted him in part because the absence of departmental boundaries would permit him to interact freely with the solid state physics and applied mechanics groups in the division. Then, and for a considerable time thereafter, graduate students were admitted to the division and to the physics department with no initial commitment to any professor. Thus, they had one to two years to explore possible Ph.D. thesis topics and to seek an advisor. This policy meant that each professor had ac cess to a brilliant group of students, and Bruce and his successors in the materials science-metallurgy option benefited greatly from it.

During the period 1930–70 in the United States and Europe there was extensive interdisciplinary cooperation of metallurgists with physicists, physical chemists, and applied mechanicians, which transformed metallurgy from an art to a science and laid the foundation for what we now label materials science. Among the physicists and physical chemists who were most prominent in effecting this transformation were Frederick Seitz, Clarence Zener, Conyers Herring, Charles Frank, Nevill Mott, W.Shockley, John Bardeen, and Harvey Brooks. From the metallurgical side there were Cyril Stanley Smith, L.S.Darken, Alan Cottrell, Morris Cohen, Paul Beck, R.F.Mehl, C.S.Barrett, A.Guinier, J.H. Hollomon, and other members of his group at the General Electric Research Laboratory.

Through his activities as a scientist and editor, Bruce played a central role in this transformation. He was the founding editor of the continuing series of treatises "Progress in Metal Physics," now "Progress in Materials Science." Bruce and co-editors Ronald King. W.Hume-Rothery, J.W.Christian, and T.B.Massalski, who joined him from time to time, attracted a very distinguished group of contributors from diverse fields. These volumes were highly influential all over the world in the education of graduate students studying metallurgy, solid mechanics, and materials science generally. They also played an important part in defining the scope and limits of materials science. Later Bruce became the first and longtime editor of a newly founded (in 1953) journal *Acta Metallurgica* (now *Acta Materialia*). This journal was founded in response to the impression that the metallurgical society journals had become too limited in their scope and too permissive on the quality of the papers they accepted. Bruce imposed high standards for publication, as he did for articles solicited for the Progress series. *Acta Metallurgica* became the journal of choice for metallurgists and other materials scientists worldwide, and with the Progress volumes it set the standard for research in these areas. While imposing high standards on the papers accepted, Bruce was highly receptive to new ideas and theories and exercised a liberal policy on their acceptability for publication. Capt. Robert Maxwell's Pergamon Press published *Acta Metallurgica* and ultimately the Progress volumes. Bruce had a sometimes-adversarial relation with Maxwell, who from Bruce's standpoint was often too concerned with the commercial aspects of publishing scientific articles.

At Harvard, Bruce continued to guide research on solidification and the structure and behavior of grain boundaries. He and his students demonstrated that the appearance of equiaxed grain structures in the crystallization of pure liquids generally results from dendritic breakup. Often this breakup is effected by convective currents in the liquid. They showed that, when these currents are suppressed in molten aluminum by imposition of a magnetic field, a columnar morphology forms in crystallization under conditions where an equiaxed one normally would have appeared. Also, they developed a beautiful visual demonstration of dendritic breakup in the freezing of water in an “ice machine” programmed to cycle water between a temperature above its freezing point and a lower temperature at the point of dendritic breakup. This experiment was on exhibit at the Brussels World's Fair in 1958, and is now sometimes displayed at the Boston Museum of Science.

At Harvard, he also wrote several books, one of which was the widely cited *Principles of Solidification* (1964); two other books were *Physical Metallurgy* and *Energy*. Most of his students and postdoctorals were stimulated, partly by his example, to dedicate their entire careers to science, and many have achieved distinction in academia, government, and industrial high-technology laboratories. He and I were colleagues at Harvard from 1962 on. When we discussed theoretical ideas or models, he always focused on the experimental support for them. In his quiet low-key way, he exerted tremendous influence on the development of interdisciplinary relations and the consciousness of a materials science bonding the underlying disciplines.

In the latter part of his Harvard career, Bruce developed a strong interest in undergraduate education, and in 1964 President Pusey appointed him master of Winthrop House. Pusey noted Bruce's remarkably broad intellectual perspective and his deep appreciation of the humanities. From my association with Bruce, I can attest to his wide knowledge of literature and to his deep insights into history and politics. The Harvard houses were patterned after the college system of Cambridge and Oxford. While each provided a community for about 400 students, they never acquired the central educational and policy roles in the university that the British colleges have. Nevertheless, Bruce with Ema's enthusiastic support fostered a friendly, intellectually vibrant atmosphere in Winthrop House. Often outstanding persons in a variety of fields visited the house to speak and interact with the students. Bruce had a strong rapport with the students, and the *Crimson,* the college newspaper, while normally critical of the university administration, often lauded Bruce and rated him one of the best of the house masters.

During the period 1967–72 student activists often disrupted Harvard and other universities. At Harvard, the activists sought to have the university administration and faculty publicly denounce the United States' Vietnam policy and to bar the Reserve Officer Training Corps from the campus. In the spring of 1968 a militant group occupied the central administration building after ousting the college administrators. President Pusey called in the outside police who removed, not too gently, the occupying students. This action was met with great indignation by students and faculty with more moderate views, as well as by the militants, and a college-wide student strike was threatened. The student body as a whole met in Harvard Stadium to consider a strike. One of the few faculty members invited to speak at this meeting, Bruce gave a conciliatory talk, and the strike was voted down. However, the college was disrupted for the rest of the spring term by fierce debates and various actions threatened by the most militant students. The extreme militancy and unrest persisted until the spring of 1972, when it ceased rather suddenly.

Throughout this period, Bruce continued to play a mediating role in reconciling the students and the administration, and thwarting the rashest actions (e.g., torching a university building) attempted by student radicals. His was always a moderate voice counseling the administration against harsh action toward the student protesters and trying to convince the students that their real quarrel was with the national— rather than the university—administration.

Also in his later years at Harvard, Bruce became especially interested in the problems of energy production and conservation. Based on his knowledge of solidification mechanisms, he conceived a process for casting silicon in a single crystal ribbon form, which might be suitable for photovoltaic applications and which could be processed with minimal material loss. He and his postdoctoral fellow Tom Surek induced the Mobil Tyco Corporation to sponsor research to test and exploit this idea, and Mobil Tyco Solar Energy Corporation carried on such a program for several years. A number of serious difficulties were overcome, but eventually a process labeled edge-defined, film-fed growth (EFG) was developed. Throughout this development Bruce played a central role in giving advice and mentoring the engineers and scientists working on the project.

A German corporation, ASE Americas, undertook the actual commercialization. Its products are hollow Si tubes with octagonal cross sections. Wafers are formed from the octagonal crystal by laser cutting. Overall materials losses are less than 8%, compared with 50% in the processing of bulk crystals. The company produces modules in the form of octagons about 10 cm across each face and approximately 300 microns thick. Most of the modules produce 50 watts of electricity. Currently the annual production is 4 megawatts of solar cell capacity, but production is being expanded quite rapidly so that shortly production will reach some 10 megawatts per year. By the year 2000, about 20 megawatts of capacity is projected. The current cost of the finished product is about $4.00 per watt. The company's solar cells operate at an efficiency of 14%.

Bruce and Ema left Winthrop House in 1973 and moved to Falmouth on Cape Cod. He continued his teaching at Harvard until 1977, when he retired as professor emeritus. He kept up the consulting already alluded to and played an active part in Falmouth community affairs. In 1986, as vice-chairman of the Falmouth Tricentennial Committee, he wrote an intriguing history of the town (published in the *Book of Falmouth*), covering the entire period from the founding of the town to 1986. His recreational activities included sailing, hiking, reading, writing, photography and color printing, and fabrication of various objects in his home workshop.

In addition to election to the National Academy of Sciences (in 1975), he received a number of other noteworthy honors, including a fellowship in the American Academy of Arts and Sciences, honorary memberships in various for eign scientific and technical societies, the Saveur Award from the American Society of Metals, and the Clamer Medal of the Franklin Institute. In 1989 the Minerals, Metals, and Materials Society created the Bruce Chalmers Award, and Bruce was the first recipient—for distinguished contributions to the science and technology of solidification processing.

In 1988 he learned that he had a condition that turned into multiple myeloma. He courageously continued his consulting and community activities until his death on May 25, 1990. Ema, their five children, and eleven grandchildren survive him.

I AM INDEBTED TO Ema and Stephen Chalmers and Alison Chalmers Rodin for supplying much personal information on Bruce's life. I thank Professor John Hutchinson of Harvard University and Kalies Juris, now vice-president for research at ASE Americas, for supplying information on the status of the silicon ribbon strip casting process.

NOTES

1. B.Chalmers, professional biography. In *Progress in Materials Science,* Chalmers anniversary volume, ed. J.W.Christian, P.Haasen, and T.B.Massalski. Pergamon: New York, 1981.

2. Bruce Chalmers memorial minute. Harvey Brooks, William Paul, Frans Spaepen, and David Turnbull (chair), Faculty of Arts and Sciences, Harvard University, May 21, 1991.

3. W.Rosenhain. The modern science of metals, pure and applied. In *Science and the Nation,* ed. A.C.Seward. Cambridge University Press, 1917.

SELECTED BIBLIOGRAPHY

1936 Micro-plasticity in crystals of tin. *Proc. R. Soc. Lond. A* 156:427.

1940 The mechanical effects of intercrystalline boundaries. *Proc. Phys. Soc.* 52:127.

1948 With R.King and R.Shuttleworth. The thermal etching of silver. *Proc. R. Soc. Lond. A* 193:465.

1950 With K.T.Aust. The specific energy of crystal boundaries in tin. *Proc. R. Soc. Lond. A* 201:210.

1953 With W.A.Tiller, K.A.Jackson, and J.W.Rutter. The redistribution of solute atoms during the solidification of metals. *Acta Met.* 1:428.

1954 Melting and freezing. Inst. of Metals lecture. *Trans. AIME. J. Met.* 6:519.

1955 With C.Elbaum. The topography of solid-liquid interfaces of metal crystals growing from the melt. *Can. J. Phys.* 33:196.

1958 With K.A.Jackson. Freezing of liquids in porous media with special reference to frost heave in soils. *J. Appl. Phys.* 29:1178.

Growth of crystals of pure materials and of the solvents of solutions. In *Growth and Perfection of Crystals,* ed. R.Doremus, B.W.Roberts, and D.Turnbull, p. 291. New York: Wiley & Sons.

1962 With P.Doherty. The origin of lineage substructure in aluminum. *Trans. Met. Soc. AIME* 224:1124.

1963 Structure of ingots. *J. Aust. Inst. Met.* 8:255.

1964 *Principles of Solidification.* New York: Wiley & Sons.

Interactions between particles and a solid-liquid interface. *J. Appl. Phys.* 35:2987.

1965 With R.B.Williamson. Crystal multiplication without nucleation. *Science* 148:1717.

Dynamic nucleation. In *Structure, Properties, Solid Interaction,* ed. T.J. Hughel, p. 308. Amsterdam: Elsevier.

1966 With D.R.Uhlmann and T.P.Seward III. The effect of magnetic fields on the structure of metal alloy castings. *Trans. Met. Soc.* 235:527.

1969 With M.Weins, H.Gleiter, and M.F.Ashby. Structure of high angle grain boundaries. *Scripta Met.* 3:601.

1971 Structure of grain boundaries. In *Structure and Properties of Metal Surfaces.* In honor of Professor Honda, Japan Honda Memrial Commemoration.

1972 With H.Gleiter. High angle grain boundaries. In *Progress in Materials Science.* New York: Pergamon.

Katherine Esau

KATHERINE ESAU

April 3, 1898–June 4, 1997

BY RAY F.EVERT

KATHERINE ESAU, WORLD renowned botanist, recipient of the National Medal of Science, author of six textbooks, and teacher par excellence, died June 4, 1997, at her home in Santa Barbara, California. She was ninety-nine years young.

Her work on plant structure covered seven-plus decades and led to much of the current research on plant function. Throughout her career, Esau continued research on phloem both in relation to the effects of phloem-limited viruses on plant structure and development and to the unique structure of the sieve tube as a conduit for food. She demonstrated an exceptional ability for attacking basic problems and she set new standards of excellence for the investigation of anatomical problems in the plant sciences.

Esau was born on April 3, 1898, in the city of Yekaterinoslav, now called Dnepropetrovsk, in the Ukraine. She lived there until the end of 1918, when she and her family fled to Germany during the Bolshevik Revolution. Her family was Mennonite, descendants of the German Mennonites that Catherine the Great invited to Russia to promote agriculture on the Ukrainian steppes. Naturally suspicious of anyone from the outside, the Ukrainians ostracized the Mennonites, who lived in colonies, developed very successful farms, schooled their children, and practiced their religion. To break down this wall of distrust, the Ukrainian authorities asked the Mennonites to send some of their children to Russian schools. Esau's uncle and father were the first from their colony to study in Russian schools. Her Uncle Jacob studied medicine. Her father Johan trained as a mechanical engineer and became the city engineer of Yekaterinoslav. Building the city's waterworks, a streetcar line, and large city buildings, including several schools, he worked very hard to make the city more liveable. Consequently, he was well liked by the citizenry, who elected him mayor. An innovation of his was to borrow money from France for the building projects. With the turmoil of the Bolshevik Revolution and being a city leader, it was a only matter of time before the Bolsheviks marked him for execution. The revolutionary government removed him from his post and kept him under constant surveillance. The Esau home was searched for banned goods, and valuable items were taken.

With the advent of World War I, the German Army advanced and succeeded in occupying the Ukraine. Most of the peaceful population welcomed this turn of events, because it saved them from Bolshevik occupation and from invasion by the unorganized bands that were massacring people and destroying property. John Esau was reinstated as mayor. As the war wound down, the German officers warned that the Esau family would be in great danger after the army left and advised them to flee with them to Germany. This was heartbreaking for the older Esaus, because their roots were in Yekaterinoslav, where their children Katherine and Paul and Mrs. Esau (the former Margrethe Toews) were born. The Esaus, along with many other people, followed the German advice. Paul was an administrator on a ship in the Black Sea and was unable to go with them on the journey, but he joined them soon after they arrived in

Berlin. They traveled third class in a train with wooden benches, together with the officers, the injured, and other refugees. The journey to Berlin lasted two weeks rather than the usual two days because of various difficulties and obstacles put in their way by the revolutionary government in the cities through which they passed. The day after the Esaus left Yekaterinoslav, posters appeared in the city proclaiming that the new city managers were looking for her father, whom they characterized as a member of the counterrevolutionary bourgeoisie and an enemy of the country.

During the turbulent last years the family spent in Russia, Esau's father gave money to a family friend to deposit in a Swiss bank in case the Esaus needed to leave the country in a hurry. The friend proved worthy of their friendship, and the money was there when the family reached Berlin. Later when the friend needed money to develop a patent on an oil-well part, Esau's father staked him. That investment provided the means for the Esaus to live comfortably for the rest of their lives.

When the Esaus fled Russia, Katherine Esau had completed her first year of study at the Golitsin Women's Agricultural College in Moscow. Fortunately, she had asked for a transcript of her course work and grades at the end of the term, and upon the family's arrival in Berlin, she registered in the Berlin Landwirtschaftliche Hochschule (Agricultural College of Berlin), where she resumed her studies, this time in German, not Russian. Her brother Paul also made the transition and studied chemistry. As part of her studies, she spent two semesters in Hohenheim near Stuttgart, where she enrolled in various agricultural courses. After two more semesters in Berlin and a final examination, she received the title “Landwirtschaftlehrerin.” Following additional studies, she passed a Zusatzprufung in plant breeding given by the famous geneticist Erwin Bauer, who urged her to re turn to Russia, saying that the country needed her. Fortunately for the world and the advancement of science, she did not heed his advice.

From Berlin, Esau went to a large estate in northern Germany that housed a model seed-breeding station for wheat. She joined the workers there in the fields and barns doing various chores. The son of the farm's owner was very smitten with Katherine, but he could not persuade her to marry him. She returned to Berlin, where a teaching assistantship awaited, but by then the Esaus had decided to settle in the United States, and preparations were underway to do so. The political situation in Germany was deteriorating and John Esau felt his family would be safer in the United States.

The Esaus left for America in mid-October of 1922. Brother Paul stayed behind to finish his last year of studies in chemistry. The Esaus crossed the continent by train and the ocean by boat; like so many other immigrants, they entered the United States at Ellis Island. Their destination was Reedley, California, where there was a large Mennonite community. Esau's father wanted to buy a farm for her to manage, but she convinced him that she needed more working knowledge of California agriculture and American-style management. At first, she worked as a house cleaner and childcare worker in Fresno, California, all the while perfecting her English and learning American customs.

When Esau felt comfortable with the language and American practices, she took a job with Sloan Seed Company in Oxnard, California. She later moved to the Spreckles Sugar Company in the Salinas Valley, where she bred strains of sugar beets for resistance to the virus causing curly-top disease. At that time, she began to consider continuing her education. It was serendipity that Professor Wilfred Robbins of the University of California, Davis, campus made a visit to the company, and Esau was asked to show him her re search project. She inquired about the possibility of study and later an invitation was extended to her to do graduate work at Davis.

Esau arrived in Davis in the fall of 1927, registering as a graduate student in the College of Agriculture for the 1928 spring semester. (The Davis campus did not award a Ph.D. at that time, so the degree was awarded from Berkeley.) She intended to develop a sugar beet that was resistant to curly-top virus. However, that would have required releasing the beet leafhopper into the university fields to infect the sugar beets. This was opposed by other plant researchers and growers, and she was told that it was incompatible with other crop research going on at Davis. Accordingly, she changed the direction of her research to the study of the transmission of curly-top virus and its effect on the sugar beet phloem, directing her research from applied to the more basic study of plant anatomy as it relates to the disease.

Esau received her Ph.D. in 1932 from Berkeley. She remained at Davis as an instructor, later becoming professor of botany. Esau left Davis in 1963, close to her official retirement date, to join her long-time research collaborator Vernon I. Cheadle at the University of California, Santa Barbara, where he was chancellor. She remained actively engaged in research for twenty-four more years! Esau considered the years in Santa Barbara her most productive and fulfilling. She had been introduced to electron microscopy just before leaving Davis, and she was interested in applying this new tool to her anatomical research. She collaborated and published with many people during this period. Today the electron microscope facility bears her name.

During her tenure at Davis, Esau studied both diseased and healthy plants, including celery, tobacco, carrot, and pear. Her work with Cheadle on the comparative structure of the secondary phloem of dicotyledons in the 1950s provided valuable information regarding the evolutionary specialization of the phloem tissue in relation to function. In 1953 her classic *Plant Anatomy*—known worldwide as the bible of plant anatomy—was published. This was followed by *Anatomy of Seed Plants* in 1960. Both of these books have been published in several languages, including Russian, and have extended her influence on the quality of instruction of plant anatomy into classrooms all over the world. The developmental aspects of her studies matured into *Vascular Differentiation in Plants* (1965) and her interest in virus-plant host relations into *Plants, Viruses and Insects* (1961) and *Viruses in Plant Hosts* (1968). In 1969 Gebrüder Borntraeger published *The Phloem.* In it Esau reviewed the structure and development of phloem beginning with the earliest records of the tissue. She redrew many of the old illustrations from the original articles and books. Her mastery of languages, including French, Spanish, English, Russian, German, and Portuguese, allowed her to prepare a thorough review of the very early and important German, Russian, and French articles.

Esau was a superb teacher, in part because she genuinely liked students. She never failed to reply to a note or letter from a student, offering encouragement and praise. Even in her early nineties she answered any correspondence she received from a student. She once said to a friend, “Don't they know I'm retired?”

Her course in plant anatomy was exceptional. A gift for story telling, total command of and enthusiasm for the subject matter, and a delightful sense of humor made her a truly outstanding teacher. On one occasion when she began a lecture humorously with “Once upon a time,” a graduate student quipped, “Aha, another of Esau's fables!” Her abilities as a teacher and researcher were recognized by fellow staff members when in 1946 while still an associate professor she was selected to give the Faculty Research Lecture at Davis.

Although she served as major professor to only fifteen doctoral students, there are numerous botanists, including many who have never met her but have studied her papers and books, who consider themselves her students. She instilled in her students an appreciation of precision and rigor that go into truly excellent studies of plant structure and development. In every aspect of her work she set new standards of excellence.

Esau served as president of the Botanical Society of America in 1951, and in 1956 was one of the original recipients of the Merit Award of that society at its fiftieth anniversary meeting. The Certificate of Merit read: “Katherine Esau, plant anatomist and histologist, for her numerous contributions on tissue development of vascular plants and in particular for her outstanding studies on the structure, development, and evolution of phloem.”

Katherine Esau was the personification of excellence and integrity. Despite her numerous successes and many honors, she remained modest and close to her Mennonite roots. In 1959, when questioned about her election to the National Academy of Sciences, she commented “I never worried about being a woman. It never occurred to me that that was an important thing. I always thought that women could do just as well as men…. My surprise at being elected to the National Academy of Sciences was not because I was a woman, but because I didn't think that I had done enough to be elected.” Some of her other honors include honorary degrees from Mills College, Oakland, California (1962) and the University of California (1966) and election to the American Academy of Arts and Sciences, the American Philosophical Society, and the Swedish Royal Academy of Science.

In 1989, she was awarded the National Medal of Science. The citation accompanying the medal read: “In recognition of her distinguished service to the American community of plant biologists, and for the excellence of her pioneering research, both basic and applied, on plant structure and development, which has spanned more than six decades; for her superlative performance as an educator, in the classroom and through her books; for the encouragement and inspiration she has given a legion of young aspiring plant biologists; and for providing a special role model for women in science.”

SELECTED BIBLIOGRAPHY

1930 Studies of the breeding of sugar beets for resistance to curly top. *Hilgardia* 4:417–41.
1933 Pathologic changes in the anatomy of leaves of the sugar beet, *Beta vulgaris* L., affected by curly top. *Phytopathology* 23:679–712.
1935 Ontogeny of the phloem in sugar beets affected by the curly-top disease. *Am. J. Bot.* 22:149–63.
1943 Origin and development of primary vascular tissues in seed plants. *Bot. Rev.* 9:125–206.
1944 Apomixis in guayule. *Proc. Natl. Acad. Sci. U.S.A.* 30:352–55.
1948 Some anatomical aspects of plant virus disease problems. II. *Bot. Rev.* 14:413–49.
1950 Development and structure of the phloem tissue. II. *Bot. Rev.* 16:67– 114.
1953 *Plant Anatomy*. New York: Wiley.
1954 Primary vascular differentiation in plants. *Biol. Rev.* 29:46–86.
1956 An anatomist's view of virus diseases. *Am. J. Bot.* 43:739–48.

1957 Phloem degeneration in *Gramineae* affected by the barley yellow-dwarf virus. *Am. J. Bot.* 44:245–51.
1960 *Anatomy of Seed Plants*. New York: Wiley.
1961 *Plants, Viruses, and Insects*. Cambridge, Mass.: Harvard University Press.
1965 With R.H.Gill. Observations on cytokinesis. *Planta* 67:168–81.
Plant Anatomy, 2nd ed. New York: Wiley.
Vascular Differentiation in Plants. New York: Holt, Rinehart & Winston.
1967 Anatomy of plant virus infections. *Annu. Rev. Phytopathol.* 5:45–76.
1968 *Viruses in Plant Hosts: Form, Distribution, and Pathologic Effects*. The 1968 John Charles Walker Lectures, with a foreword by G.S. Pound. Madison: University of Wisconsin Press.
1969 With R.H.Gill. Tobacco mosaic virus in dividing cells of *Nicotiana. Virology* 38:464–72.
The Phloem: Handbuch der Pflanzenanatomie. Band V. Teil 2, Histologie. Berlin-Stuttgart: Gebrüder Borntraeger.
1971 With R.H.Gill. Aggregation of endoplasmic reticulum and its relation to the nucleus in a differentiating sieve element. *J. Ultrastruct. Res.* 34:144–58.
1972 With L.L.Hoefert. Development of infection with beet western yellows virus in the sugar beet. *Virology* 48:724–38.

With L.L.Hoefert. Ultrastructure of sugar beet leaves infected with beet western yellows virus. *J. Ultrastruct. Res.* 40:556–71.

With R.H.Gill. Nucleus and endoplasmic reticulum in differentiating protophloem of *Nicotiana tabacum. J. Ultrastruct. Res.* 41:160– 75.

1976 With A.C.Magyarosy and V.Breazeale. Studies of the mycoplasma-like organism (MLO) in spinach leaf affected by the aster yellow disease. *Protoplasma* 90:189–203.

1977 *Anatomy of Seed Plants,* 2nd ed. New York: Wiley.

1981 With L.L.Hoefert. Beet yellow stunt virus in the phloem of *Sonchus oleraceus* L. *J. Ultrastruct. Res.* 75:326–38.

1982 With J.Thorsch. Nuclear crystalloids in sieve elements of species of *Echium* (Boraginaceae). *J. Cell. Sci.* 54:149–60.

1985 With J.Thorsch. Sieve plate pores and plasmodesmata, the communication channels of the symplast: Ultrastructural aspects and developmental relations. *Am. J. Bot.* 72:1641–53.

1991 With R.H.Gill. Distribution of vacuoles and some other organelles in dividing cells. *Bot. Gaz.* 152:397–407.

MAXWELL FINLAND

March 15, 1902–October 25, 1987

BY FREDERICK C.ROBBINS

DR. FINLAND, KNOWN TO most people as Max, was a giant in the field of infectious diseases, although physically he was far from a giant, his height being not much over 5 feet. He was a prodigious worker and his bibliography included more than 800 scientific articles and a large number of chapters in books, meeting proceedings, and various reports. Some of his studies on the natural history and pathogenesis of infectious diseases were classics, such as his series of reports on pneumonia. He was a pioneer in the assessment of antibiotics, including their use and misuse, in recognizing the significance of antibiotic resistance, and in pointing out the importance of hospital infections and their control. He was an exemplar of the ideal academic physician, who, in addition to conducting research, was a teacher and a superb physician.

This paragon was born in a small town in the Ukraine in 1902. His forebears included a great-grandfather who was the grand rabbi of Krakow and a grandfather who was cantor in Zashkov. In spite of this impressive background he did not seem to have much involvement in formal religion. At the age of four he came with his family to live in Boston. Like so many immigrants, Max's family valued education. He graduated from Boston English High, ranked second in his class, and was accepted at Harvard College with a scholarship. He thrived in the exciting environment of Harvard of that day. He had such stimulating professors at James Bryant Conant (chemistry and later president), Louis Fiezer (chemistry), Richard Cabot (social ethics), Zachariah Chaffee C. (constitutional law), and Winthrop John Vanleuven Osterhout (botany). Max and some other students established a club for Hebrew speakers where they heard reports from Israel. He also taught in a Hebrew school, for which he received some pay. Indeed, by one means or another he largely supported himself throughout his education.

In 1922 Max entered Harvard Medical School. There he came under the influence of Hans Zinsser, chairman of the Department of Microbiology, and Milton Rosenau, chairman of preventive medicine and hygiene. Rosenau was an impressive figure and Zinsser was an exceptionally dynamic and charismatic individual. James Howard Means, chief of medicine at the Massachusetts General Hospital was also influential. After graduation, Max became an intern on the 2nd Medical Service at the Boston City Hospital (BCH). At that time the situation at BCH was almost ideal for someone interested in academic medicine. The Harvard Medical Unit included two medical wards (the 2nd and 4th) and the Thorndike Memorial Laboratory. In addition, there was the excellent Pathology Unit, headed by Mallory, and the South Department, which was the Contagious Disease Hospital. In addition to Harvard, both Tufts and Boston Universities conducted teaching units at the hospital. The Harvard Unit must have been a heady environment for a young man. The Thorndike Laboratory and the clinical services were essentially one unit and extensive clinical research was being conducted there. Max apparently had planned to go into practice, but as it turned out he found the environ ment at BCH so highly compatible that he spent almost his entire career there as a member of its Harvard Unit.

After his internship Max accepted the position of pneumonia resident at BCH, but he also worked in Rosenau's department, where anti-pneumococcal serum was being produced. In 1929 Finland was asked by Dr. Nye to join his laboratory at the Thorndike. Thus began one of the most remarkable careers in the field of infectious diseases.

The first studies conducted by Max and his associates dealt with pneumonia. At that time the only treatment for pneumococcal pneumonia was administration of type-specific antiserum. The process of treating patients was cumbersome, to say the least. A naso-pharyngeal swab was taken and placed in a tube containing culture medium. After a few hours of incubation when enough bacteria had proliferated, material from the culture was exposed to type-specific antisera. If there was a match between the antiserum and the chemical composition of the polysaccharide on the surface of the bacterium, the capsule would swell and it could be seen with an ordinary light microscope (known as the Quellung reaction). If Quellung occurred, the corresponding antiserum (horse or rabbit) was administered to the patient. The patients usually survived the infection, but they invariably suffered from serum sickness, which could be most unpleasant. Finland and his fellows did a series of studies on the treatment of pneumococcal infection conducted with meticulous care, a hallmark of Finland's research throughout. When sulfonamides became available the infectious disease group at the Thorndike was among the first to conduct systematic clinical studies with the backup provided by the Thorndike laboratories.

With the advent of penicillin, again the Finland group did many of the fundamental studies of the antibacterial spectrum, pharmacokinetics, and to some extent mecha nism of action. This was repeated as the various broad-spectrum antibiotics were developed. The studies involved careful clinical observations integrated with laboratory investigations and were examples of what the BCH unit made possible.

Finland early recognized that bacteria were developing resistance to the antibiotics in general use. He realized the implications of this and that the indiscriminate use of antibiotics in the hospital and in the community was important in promoting the development of resistance. He recommended the reservation of certain antibiotics for use only in special circumstances in order to preserve their availability for emergencies. He also was one of the first to sound the alarm about the frequency and importance of infections acquired in the hospital.

Although the contributions just mentioned were important, they by no means reflect the scope of his interests. In fact, he and his collaborators (mainly fellows) explored almost every aspect of infectious diseases that one could mention. The infectious diseases division of Thorndike, headed by Finland for most of its existence, was tremendously productive. As already mentioned, Finland's personal bibliography includes more than 800 scientific papers and an additional 20 to 30 chapters of books and contributions to published proceedings of meetings and symposia.

Finland had a great attraction for young physicians who came to work with him. He took a personal interest in each one and was careful to see that they were recognized for their contributions. Characteristic of him is the history he wrote of the Division of Infectious Diseases. The description of each project begins by identifying the fellow or fellows involved. It is not clear just how many fellows passed through his division, but it probably exceeded 100. The author of this memoir recognized 27 as leaders in the field throughout this country and abroad. It includes such names as Wesley Spink, Lowell Rantz, John Dingle, Charles Rammelkamp, George Gee Jackson, Lewis Thomas, Calvin Kunin, Theodore Eickhoff, and Edward Kass, to name a few.

Finland spent most of his career at BCH in the Harvard Medical Unit. His home base was the Thorndike Memorial Laboratory, a key element in the Harvard Unit. Before retirement in 1968 he had become its director and the director of the Harvard Unit. The Thorndike was an extraordinary organization. It was headed by such luminaries as Francis Peabody, George Minot, William Castle, and Max Finland. On the staff were such well-known figures as Chester Keefer (later to move to Boston University), Soma Weiss, Charles Davidson, Joseph Wearn, and Thomas Hale Ham. Although the Thorndike was not very impressive physically, this remarkable group of people was immensely productive, and it influenced a large body of young men and women who collectively had a profound impact on medical care, research, and teaching for several generations. The famous statement by Francis Peabody in his paper "The Care of the Patient," "The secret of the care of the patient is in caring for the patient," governed the behavior of the Harvard Unit. Although the close relationship between the Thorndike and the Harvard clinical services was exploited to the benefit of clinical investigation, the patients were always treated with respect, even though they were almost exclusively from the poorer segment of society. When I was a fourth-year Harvard student on the Harvard service, George Minot was our visitant. Of course, I regarded him with awe. However, I was most impressed when on rounds we were examining a woman who was in for pneumonia or some acute illness. She worked as a waitress, was single, and had many personal problems. Minot sat down by her bedside and spent 10 to 15 minutes discussing her personal difficulties with evident concern for this aspect of her life. To him she was not just a case of pneumonia. This made quite an impact on the students and seemed to exemplify Dr. Peabody's dictum in action.

In 1973 the Thorndike Memorial Laboratory severed its relationship with BCH and moved to the Beth Israel Hospital, a major Harvard affiliate located about one block from the Basic Sciences quadrangle. Indeed, Harvard severed all relationships with BCH when the governing body of BCH decided to affiliate with a single medical school and chose Boston University. This decision was the result of a number of factors, including the shrinking patient population at the hospital and the close proximity of the Boston University Hospital to BCH. In any case, it ended a 50-year relationship between Harvard and the hospital, which had been remarkably productive and one in which Max Finland had played such a key role.

As mentioned before, Max was a small man physically, but this never seemed to affect his behavior. From the time Max entered Harvard College he was associated with Harvard and its medical school until his death in 1987. He was loyal to the institution and displayed this in many ways including sizeable contributions from his personal resources. He was also influential in stimulating others to contribute, and it is estimated that he was responsible for contributions of approximately $6 million. In recognition of his many contributions to Harvard and to the health of the public, the Max Finland professorship in clinical pharmacology was funded at Harvard Medical School. The other institutions that commanded his loyalty were the BCH, the Harvard Medical Unit, and in particular the Thorndike Laboratory.

Max never married, but he had a devoted extended family in his many friends and the large number of young people for whom he served as mentor, teacher, and friend. He was a prodigious worker, but he always had time to discuss a problem with colleagues, including fellows or even house officers and students. He enjoyed the symphony, usually taking along a friend or fellow. He also entertained at the Athens Restaurant or a favorite Chinese restaurant (Ye Hong Guey's). My wife and I had the pleasure of joining him a couple of times at the Chinese restaurant, and they were convivial occasions, with Max the perfect host.

In 1968 Max retired and became the Minot professor emeritus and shortly thereafter he moved his office to the Channing Laboratory headed by Edward Kass. He continued to publish and to supervise fellows. He was also given an appointment at the Veterans Administration Hospital. Thus, Max continued to display one of his main attributes: a remarkable capacity for hard work. One of the prodigious tasks he undertook in his retirement was the editing of a three-volume history of the Harvard Medical Unit at Boston City Hospital. The history includes brief statements by many of the young physicians who spent some time in the unit and who represent a large proportion of the important contributors to academic medicine for over half a century.

Finland received many honors. He was a member of the National Academy of Sciences and recipient of the Kober Medal of the Association of American Physicians, the Bristol Award of the Infectious Diseases Society of America, the Chapin Award of the City of Providence, the Philips Award of the American College of Physicians, the Oscar B.Hunter Award of the American Society of Clinical Pharmacology and Therapeutics, and the Sheen Award of the American Medical Association. He received honorary degrees from Western Reserve and Thomas Jefferson Universities. In 1982 Finland was awarded a doctor of science (honoris causa) degree from Harvard University, something Harvard does not often do for its own faculty. The citation reads: “The

University hails a distinguished and loyal son, who for sixty years as physician, teacher, and scholar has given wisdom, energy, and substance to the advancement of clinical medicine."

As a fitting end to this memoir I quote from the presentation of Finland's good friend and associate Charles Davidson when he presented the Kober Medal: "We honor today a man who, with a friendly smile and a quiet and modest manner, has achieved great distinction not only because of his extraordinary contributions to American medicine but also because of his beneficial influence on so many patients, students, colleagues, and friends."

REFERENCES

Davidson, C.S. 1978. Presentation of the George M.Kober Medal for 1978 to Maxwell Finland. *Trans. Assoc. Am. Phys.* 91:51–62.

Finland, M. 1982–83. *The Harvard Medical Unit at Boston City Hospital, Harvard Medical School* vols. I, II, and III. Distributed by the University Press of Virginia for the Francis A. Countway Library of Medicine.

Peabody, F.W. 1927. The care of the patient. *J. Am. Med. Assoc.* 88:877–82.

SELECTED BIBLIOGRAPHY

1930 Serum treatment of lobar pneumonia. *N. Engl. J. Med.* 202:1244–47.

1931 With W.D.Sutliff. Type 1 lobar pneumonia treated with concentrated pneumococci antibody (Felton). *J. Am. Med. Assoc.* 96:1465.

With W.D.Sutliff. Specific cutaneous reaction and circulatory antibodies in course of lobar pneumonia. No serum. *J. Exp. Med.* 54:637.

With W.D.Sutliff. Specific cutaneous reaction and circulatory antibodies in course of lobar pneumonia. Serum. *J. Exp. Med.* 54:653.

1933 With W.D.Sutliff. Immunity reaction of human subjects to strains of pneumococci other than 1, 2, and 3. *J. Exp. Med.* 57:95.

1935 With J.M.Ruegsegger. Immunization of human subjects with specific carbohydrates of Type 3 and related Type 8 pneumococcus. *J. Clin. Invest.* 14:829–32.

1936 With R.C.Tilghman. Bacteriological and immunologic studies in families with pneumococcal infections; development of type specific antibodies in healthy contact carriers. *J. Clin. Invest.* 15:501.

1939 Specific treatment of pneumococci pneumonia analysis of results of serum therapy and chemotherapy at BCH from July 1938 through June 1939. *Ann. Intern. Med.* 13:1567–93.

1942 Spread of pneumococcal and streptococcal infections in hospital wards and in families. *Aerobiology,* pp. 212–22.

1944 With M.Meads, H.W.Harris, and B.A.Samper. Treatment of meningococcal meningitis with penicillin. *N. Engl. J. Med.* 231:509– 17.

1945 Cold agglutinins; cold iso hemogglatins in primary atypical pneumonia of unknown etiology with rate on occurrence of hemolytic anemia in these cases. *J. Clin. Invest.* 24:458–73.

1946 With C.S.Davidson and S.M.Levenson. Chemotherapy and control of infection among victims of Coconut Grove disaster. *Surg. Gynec. Obst.* 82:151–73.

With R.Murray, L.Kilham, and C.Wilcox. Development of streptomycin resistance of gram negative bacilli in vitro and during treatment. *Proc. Soc. Exp. Biol. Med.* 63:470–74.

1947 Use of penicillin in infection other than bacterial endocarditis. *Adv. Intern. Med.* 2:350.

1968 With F.F.Barrett and J.I.Casey. Infections and antibiotic use among patients at Boston City Hospital, February 1967. *N. Engl. J. Med.* 278(1):5–9.

1970 With F.F.Barrett, J.I.Casey, and C.Wilcox. Bacteriophage types and antibiotic susceptibility of *Staphylococcus aureus*. Boston City Hospital, 1967. *Arch. Intern. Med.* 125(5):867–73.

1973 Excursions into epidemiology: Selected studies during the past four decades at Boston City Hospital. *J. Infect. Dis.* 128(1):76–124.

1974 With J.E.J.McGowan. Infection and antibiotic usage at Boston City Hospital: Changes in prevalence during the decade 1964–1973. *J. Infect. Dis.* 129(4):421–28.

1975 With J.E.J.McGowan and M.W.Barnes. Bacteremia at Boston City Hospital: Occurrence and mortality during 12 selected years (1935– 1972), with special reference to hospital-acquired cases. *J. Infect. Dis.* 132(3):316–35.

Relationships of antibiotics in animal feeds and salmonellosis in animals and man. *J. Anim. Sci.* 40 (6):1222–40.

1976 With C.Garner, C.Wilcox, and L.D.Sabath. Susceptibility of "enterobacteria" to aminoglycoside antibiotics: comparisons with tetracyclines, polymyxins, chloramphenicol, and spectinomycin. *J. Infect. Dis.* 134(suppl.):57–74.

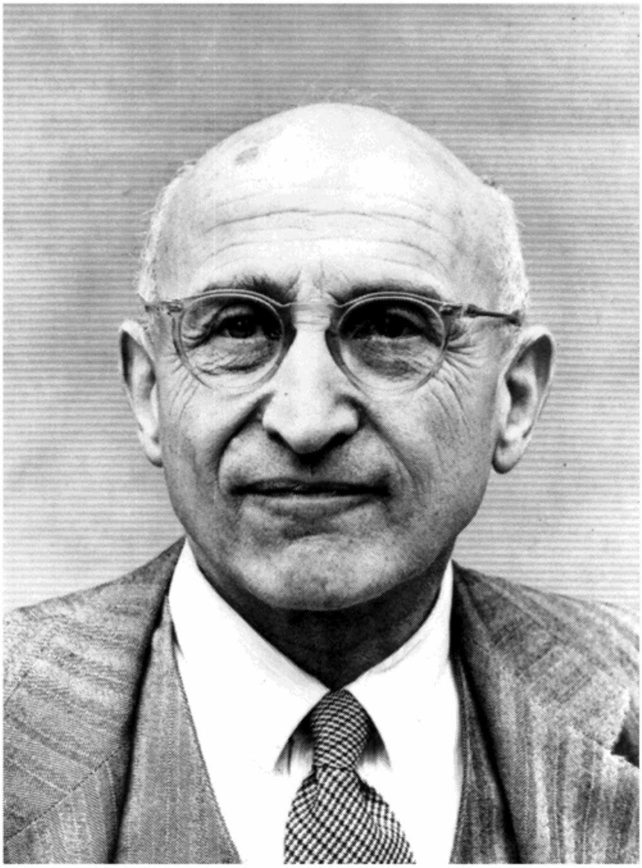
1977 With M.W.Barnes. Changes in occurrence of capsular serotypes of *Streptococcus pneumoniae* at Boston City Hospital during selected years between 1935 and 1974. *J. Clin. Microbiol.* 5 (2):154–66.

1979 Pneumonia and pneumococcal infections, with special reference to pneumococcal pneumonia. The 1979 J.Burns Amberson lecture. *Am. Rev. Respir. Dis.* 120(3):481–502.

Emergence of antibiotic resistance in hospitals, 1935–1975. *Rev. Infect. Dis.* 1(1):4–22.

1984 With E.Strauss and O.L.Peterson. Landmark article June 14, 1941: Sulfadiazine. Therapeutic evaluation and toxic effects on four hundred and forty-six patients. By Maxwell Finland, Elias Strauss, and Osler L.Peterson. *J. Am. Med. Assoc.* 251(11):1467–74.

With M.A.Barry and D.E.Craven. Serotypes of *Streptococcus pneumoniae* isolated from blood cultures at Boston City Hospital between 1979 and 1982. *J. Infect. Dis.* 149(3):449–52.



Courtesy of the California Institute of Technology Archives, Pasadena

Beno Gutenberg.

BENO GUTENBERG

June 4, 1889–January 25, 1960

BY LEON KNOPOFF

BENO GUTENBERG WAS THE foremost observational seismologist of the twentieth century. He combined exquisite analysis of seismic records with powerful analytical, interpretive, and modeling skills to contribute many important discoveries of the structure of the solid Earth and its atmosphere. Perhaps his best known contribution was the precise location of the core of the Earth and the identification of its elastic properties. Other major contributions include the travel-time curves; the discovery of very long-period seismic waves with large amplitudes that circle the Earth; the identification of differences in crustal structure between continents and oceans, including the discovery of a significantly thin crust in the Pacific; the discovery of a low-velocity layer in the mantle (which he interpreted as the zone of decoupling of horizontal motions of the surficial parts from the deeper parts of the Earth); the creation of the magnitude scale for earthquakes; the relation between magnitudes and energies for earthquakes; the famous universal magnitude-frequency relation for earthquake distributions; the first density distribution for the mantle; the study of the temperature distribution in the Earth; the understanding of microseisms; and the structure of the atmosphere.

Beno Gutenberg was born in 1889 in Darmstadt, Germany, where his father owned a small soap factory. Beno was the eldest of two sons; his brother Arthur was his junior by four years. Both parents came from merchant families. His father's ambition was that Beno would step into the family business, as would his younger brother, but Beno wanted to study science, having little interest in the business. In the *gymnasium* he became involved in the operation of the meteorological station, and this aroused an interest in weather forecasting and climatology, which led him to undertake meteorological studies at the university.

In the summer of 1907 Gutenberg entered the Technische Hochschule in Darmstadt. He learned that a course on instrumental observations of geophysical phenomena was being offered by Emil Wiechert at the Institute of Geophysics of the University of Göttingen, and he moved there in 1908. Wiechert had a major reputation in both seismology and electromagnetic theory. In the latter area, he is identified with the Liénard-Wiechert potential. He proposed that X rays are electromagnetic waves, and from his measurement of *e/m* for cathode rays, he was the first to announce that cathode rays (electrons) are particles of subatomic mass from 2,000 to 4,000 times less massive than the hydrogen atom shortly before J.J.Thomson took the extra step of identifying the mass precisely. Wiechert was the inventor of a seismograph in widespread use in the first half of the twentieth century, and he had studied the problem of constructing the velocity structure of a spherical Earth from travel-times of seismic impulses, having derived an integral equation also identified with the names of Herglotz and Bateman. Wiechert had also inferred that the Earth must have a central iron core.

The four students in Wiechert's course were introduced into observational methods in meteorology, the handling of seismographs and the reading of seismograms, and the determination of exact (astronomical) time. Gutenberg took lectures from Wiechert on terrestrial magnetism, tides, and geodesy. He took lectures in physics, pure and applied mathematics, elasticity, algebra, and logic from Born, Hilbert, Klein, E.Landau, Madelung, Minkowski, Prandtl, Runge, K.Schwarzschild, Voigt, and Weyl. Gutenberg took a course in geophysics to prepare better for his work in meteorology.

At the end of a course in seismology in Gutenberg's third year, Wiechert told him that he had progressed to the limits of knowledge in seismology and advised him to start his thesis research; Gutenberg selected a study of microseisms. In 1910 Gutenberg made a trip to the coast of Norway, and was able to correlate surf in Norway with microseisms in Göttingen. Microseisms are small disturbances, more or less continuously recorded by sensitive seismometers, and form the background motion upon which earthquake recordings are superimposed. As Gutenberg was later to discover, microseisms are mainly associated with storms in the deep oceans that are at times very distant from the recording station, and less so with surf. Gutenberg's first published paper, which was on microseisms, appeared in 1910. Gutenberg was concerned with the problems of microseisms even at the end of his career.

The possibilities of modern instrumental seismology were not recognized until the end of the nineteenth century. Indeed, the first recording of a distant earthquake was only made on February 25, 1889. So the time was ripe in the first decade of the twentieth century for a bright young investigator to attack the problems of the seismic wave velocity in the Earth's interior through the application of readings of high quality instrumental data. Like many of the prominent seismologists of the first half of the twentieth century, Gutenberg took up the subject without previous intentions. He was attracted by the opportunities for research in a comparatively new subject.

One of Wiechert's assistants, Karl Zöppritz, had been concerned with the calculation of the reflection and transmission coefficients of elastic waves. At about the time of Gutenberg's arrival in Göttingen, Zöppritz died of a massive infection at the age of twenty-seven. Wiechert passed along an unfinished manuscript by Zöppritz on the relation between the amplitudes of seismic waves and velocity variations at depth, with the recommendation that the paper be finished by Gutenberg and Ludwig Geiger, who was Wiechert's other assistant. This event opened the door to a series of studies of the use of amplitudes of seismic waves to determine the structure of the Earth; Gutenberg's interest in amplitudes lasted throughout his career.

Two papers on amplitudes by Geiger and Gutenberg appeared (1912) as part of the series "Über Erdbenwellen" by Wiechert and his students. The two papers presented new results on the structure of the solid Earth determined from the amplitudes and travel-times of seismic waves. Geiger and Gutenberg attempted to determine the amplitude-distance relation for P-waves, but there are enormous fluctuations in the amplitudes from station to station, especially because of differences in instrumentation and in the local geology. Geiger and Gutenberg avoided local influences by taking the ratio of amplitudes of PP/P (i.e., of waves with one bounce off the outer surface of the Earth to waves with no bounce). They observed a large increase in the ratio at about 40° and 95°. The increase at 40° was attributed to a decrease in the amplitude of P; as was to be discovered later, the increase was actually due to an increase in P at 20°, and hence of PP at 40°. An abrupt change of amplitudes with distance is a strong indicator of inhomogeneity in the velocity distribution at depth. Thus, Geiger and

Gutenberg inferred that there was a significant decrease in the velocity gradient at a depth of about 1200 km in the mantle, instead of the more appropriate sharp increase in gradient starting at a depth of around 410 to 440 km. Their model had two additional discontinuous velocity gradients at depths greater than 1200 km. The increases in amplitudes at 20°, called the 20° discontinuity, are today identified with two steps in the properties of the mantle at depths around 410 km and 670 km. The decrease in velocity gradient at about 1200 km and the absence of a sharp increase in velocity at shallower depths persisted in Gutenberg's models to the end of his career (1958), although the depth of the decrease was reduced to about 900 km in the later models. Indeed, Gutenberg (1934) stated, “There is no indication of a discontinuity in the mantle of the Earth at larger depths (than 200 km)…and none corresponding to an epicentral distance of about 20°” and again (1953), “There is no evidence of a discontinuity in the mantle between the low-velocity layer and a depth of about 900 km….” Geiger and Gutenberg correctly interpreted the second increase in the ratio as due to a decrease in the amplitude of P. This was the onset of the shadow zone due to the decrease in seismic velocities in the core.

In 1911 Gutenberg submitted his dissertation on microseisms entitled “Die seismische Bodenunruhe” (1912), written under the supervision of Wiechert. The oral examination was held on May 3, 1911, and Gutenberg was awarded the degree of doctor of philosophy *valde laudabili,* with geophysics as his major and geometry and applied mathematics as minor subjects. Wiechert's citation read, “The author has applied extraordinary diligence. About two million facts are used! The discussions are carried out with much skill, and the results are of considerable importance for science.”

Gutenberg worked in a postdoctoral capacity at the Insti tute of Geophysics at Göttingen during the year following the award of his doctoral degree. At that time he began his famous work of the systematic study of seismic waves through the interior of the Earth. From Göttingen recordings, he observed that the seismic phase P′ had an increase of amplitudes at a distance of about 143°. He extended the range of amplitudes to greater distances, which allowed him to interpret the shadow zone between 95° and 143° as cast by a low-velocity core at great depth in the Earth and of considerable contrast to the region above.

In 1897 Wiechert had proposed that the Earth had an iron core starting at a depth of about 1400 km, and in 1906 Oldham had interpreted seismographic data to propose that the core began at a depth of about 3900 km. Gutenberg calculated “the travel-times of waves to be reflected and refracted at the surface of the core, outside as well as inside”; the waves refracted at the core-mantle boundary are the P′ or PKP phases, and the reflected waves are the PcP phases. Gutenberg determined the depth to the top of the core as 2900 km from the surface. He established that the core has a sharp boundary and specified the values of the P-wave velocities in the mantle and in the core (1914).

To do the calculation, Gutenberg developed new, accurate travel-time curves for both P- and S-waves for distances greater than 80°, which allowed him to determine the slope with high accuracy. His velocity distribution for the mantle was similar to the 1912 model. The precision of Gutenberg's determination of the depth to the core is astounding and would be so at any time. More than twenty years later, Gutenberg and Richter (1936) used the times of the reflections PcP from the upper surface of the core and derived the same depth to the core boundary. In 1939 Harold Jeffreys, using his powerful seismological and statistical skills in a calculation with his own travel-time data, derived the result

2898 ± 3 km, which is the value in common use today. Jeffreys also verified that the core-mantle boundary was sharp. Jeffreys noted that, although his and Gutenberg's travel-time curves agree within one second, the first derivatives were significantly different, which was (and is) important, since the ratio of radius to velocity r/v for the ray whose maximum penetration is to radius r is equal to the derivative of the time-angular distance relation. After the monumental discovery of the core, two major seismological plums of deep-earth structure remained to be discovered, namely Lehman's discovery of the inner core and Gutenberg's work on the absence of S-phases in the core.

Gutenberg began a year of military service in October 1912. In October 1913 he started to work as a seismologist with the title of scientific assistant at the Central Bureau of the International Association of Seismology (IAS) at Strassburg, working on microseisms, travel-time curves, and the crustal structure of Europe. His work at Strassburg was interrupted by the outbreak of World War I in August 1914 after only ten months on the job. He was quickly inducted into the German army and served in the infantry. Almost immediately, Gutenberg was wounded in the head by a grenade (his helmet saved his life). Upon recovery, he returned to Strassburg, where he was assigned to the training of officers. In 1916 he volunteered for the weather forecasting service and was sent to the Central Station for Meteorology near Berlin. Gutenberg shuttled between the Russian, French, and Belgian fronts as a meteorologist attached to the chemical warfare engineers, having been assigned the problem of the prediction of the likelihood of backward drift onto the German soldiers of the poison gases that their own army had released. He was also assigned the problem of measurement of the location of cannons from the travel-times of sound transmission, a problem of great similarity to that of the location of earthquake sources. His later work on the structure of the atmosphere (e.g., 1926, 1930) had its genesis at this time.

During the war he spent as much time as possible at Strassburg and worked on routine interpretation of seismograms at the Central Bureau in Strassburg under O.Hecker. The IAS and the Central Bureau were dissolved on March 31, 1916, and Gutenberg became scientific assistant for the Meteorological Service at the German Imperial Station for Earthquake Research at the University of Strassburg. He was concerned with seismological problems during much of the war, meteorological work permitting.

With the return of Alsace to France at the end of World War I, Gutenberg was unemployed, and he returned to Darmstadt. He was an applicant for his old post at the now-French seismic station in Strasbourg, but he was not successful, even though he was soon to be the most famous seismologist in Western Europe.

After the war, the German interior ministry placed Gutenberg in an earthquake research institute that was planned for Jena, but because of the chaos in postwar Germany, the institute existed only on paper. In 1923 Hecker was appointed to the Jena position, but the interior ministry could not (or would not) appoint Gutenberg. (Gutenberg received a letter of greeting on the occasion of the thirtieth anniversary of the Jena Institute addressed to its long-time workers and colleagues that he considered to be a “wry joke.”)

Since he could not find a scientific position after the war, Gutenberg worked in his father's soap factory in Darmstadt from 1918 to 1930. Arthur had died in the war in 1915, and Beno was under some pressure to help with the family business. After his father's death in 1927, Beno took over the factory. He met Hertha Dernburg at activities of Jewish sporting and democratic clubs in Darmstadt. They were married on August 14, 1919. Gutenberg continued to be active in local Jewish causes, and was a member and later president of the local chapter of B'nai Brith.

From 1918 Gutenberg worked on geophysical problems at his home in Darmstadt, when he had free time, mainly during evenings and weekends. He used seismographic recordings that were obtained from the institute at Frankfurt —a long tram ride away. He obtained recordings and other information from other observatories by correspondence. Starting in 1923, a steady stream of important papers began to appear from his study in Darmstadt. In *Der Aufbau derErde* (1925) Gutenberg constructed accurate travel-time curves for seismic waves in which all the important seismic phases were shown to 180°, including some phases triply reflected from the surface.

Gutenberg confirmed and made precise the observations of Tams, Angenheister, and Macelwane in 1921–22, in which the velocities of propagation of surface waves were faster across the oceanic than across the continental portions of the Earth's surface (1924). For this he used measurements of the velocities of both Love and Rayleigh waves at a number of periods from all the prewar seismographic records at Strassburg, from all records at Jena, and selected records from other stations. He proposed a method of inversion of the dispersion of surface waves to determine upper mantle structure that was similar to the method ultimately applied in the late 1950s. His inversion for crustal thickness (he managed to confuse group and phase velocities) gave a thick crust under the continents and a thinner crust under the oceans, with a crustal thickness of only 5 km under the Pacific. The latter was a remarkably foresightful result in view of direct substantiation about thirty years later when exploration of the oceanic crust became possible. From these results, Gutenberg became convinced that there were large structural differences between continents and oceans in the outermost parts of the Earth, a view that was to play a significant part in his model of continental drift (1936). Because of his observation of differences between continental and oceanic upper mantle structure, Gutenberg was convinced of the likelihood of horizontal mobility long before it became fashionable in the geophysical community. He began a study of the rheological problems associated with Wegener's theory of continental drift and developed his own theory of flow in the mantle (1927).

Gutenberg was especially pleased with his calculation of the distribution of the density, and hence the elastic moduli, as a function of depth in the Earth (1923). The calculation may be considered crude by today's standards, since it relied on a linear density-versus-depth model in each of the four layers in the mantle model of 1912, but it is important, because it was the first construction of a density distribution for the mantle. Later, Bullen introduced the constraint of compressibility, and still later the density was obtained by Gilbert and Dziewonski and others from inversion of the free oscillation spectrum. This was the starting point for a new discipline concerned with the chemical composition of the Earth's interior. In his later discussions of composition, Gutenberg relied mainly on the density models of Bullen, Bullard, and Birch, rather than his own.

Gutenberg's unhappy financial situation did not escape the notice of Professor Franz Linke, director of the Institute of Meteorology and Geophysics of the University of Frankfurt. Linke proposed to Gutenberg that he obtain his *Habilitation,* which was awarded at the University of Frankfurt on July 25, 1924, with his book *Die seismische Bodenunruhe* as his *Habilitationsschrift.* Although it had the same title as his thesis, the book presented new results on microseisms and the structure of the Earth. After an introductory lee ture with the title "New Results on the Structure of the Earth's Crust," he became privat-dozent, the equivalent of an instructor and held lectures in geophysics at the University of Frankfurt from 1924 to 1930. He lectured on seismology, applied geophysics, oceanography, tides, and the structure of the Earth. The salary at Frankfurt was a percentage of the tuition paid by his students, but it was still insufficient to support himself and his family, and now he had the additional workload of his numerous lectures. During this period, Gutenberg and his wife devoted their full efforts to operation of the factory under hardship; in the immediate postwar period they bartered soap both for raw materials for their products and for personal necessities.

His financial situation did not change significantly when, on October 21, 1926, the science faculty of the University of Frankfurt elected him to the position of *ausserordentlicher Professor,* a non-state funded position, which he held from 1926 to 1930. The citation of the faculty in his election read, "It is not consonant with his scientific importance that Dr. Gutenberg has been a merchant-official in his father's soap factory in Darmstadt since the end of the war in order to support himself. Our faculty has conferred upon him the Habilitation on July 25, 1924. His lectures are delights from semester to semester and attract more and more students. We fear that under this double life as merchant and scholar, his productivity will gradually disappear. It is unlikely that such productive scientific work as has appeared in recent years from Dr. Gutenberg can persist for much longer if he continues to have his full-time merchant's responsibility."

Despite these concerns voiced by the faculty, the professorship was poorly funded and Gutenberg's stipend continued to be derived principally from his position as privatdozent. Later he added a position as director of the seismological station of the university, located on the Kleine Feldberg in the Taunus mountain range on the opposite side of Frankfurt from Darmstadt. The two scientific positions were still insufficient to support him and his family, which now included two children.

Gutenberg used his stipend as director of the Kleine Feldberg station to start the publication of a bulletin. After working with the Galitzin pendulum seismographs, Gutenberg showed no further direct interest in instrumentation; he was, of course, an avid user of the recordings that could be obtained from the seismographs. Each year he spent two or three periods of one to two weeks each in the Taunus. He enjoyed the quiet, which enabled him to think creatively in solitude on his daily, and occasionally twice daily, walks to the peak. The path is now named “the Benoweg” in his honor. Gutenberg was invited to the dedication of the Benoweg in August 1959, a few months before death, but he did not attend.

A number of well-known seismologists visited the house at Mühlstrasse 6 in Darmstadt, including Inge Lehmann in 1926 and James Macelwane and Perry Byerly in 1929. Although Beno spent his days at the factory, the visitors received his full attention in the evenings, on weekends, and in his spare time. Lehmann has stated that on the occasion of her visit with Gutenberg in 1926, they simply worked together without any communication of a personal nature. She also stated that Gutenberg was a wonderful teacher and that she owed him her excellent introduction to seismology and that he gave of his time unselfishly. Her impression did not change when she visited Gutenberg in Pasadena.

Contrary to the pessimistic forecast of the Frankfurt faculty, Gutenberg continued to turn out many papers and a remarkable series of books. The *Lehrbuch der Geophysik* (1929) was written and compiled in the study of the house on Mühlstrasse. The *Lehrbuch was* a major undertaking that provided a complete description of the understanding of the geophysics of the solid Earth, the oceans, and the atmosphere up to that time. The topics included evolution of the Earth and its geologic structure, volcanoes, mechanics of the atmosphere, and ice ages. There were nine contributors, including Linke and L.Weickmann. Gutenberg wrote a fourth of the volume, whose length exceeded a thousand pages. Interestingly, Gutenberg's bylines for his own chapters in the *Lehrbuch* are “B.Gutenberg, Darmstadt,” while the title page and table of contents identify him as “B. Gutenberg, Frankfurt.”

It is clear that the massive undertaking of the *Lehrbuch* was a project developed almost completely from his home and that Mühlstrasse 6 had become the center of German seismology and geophysics. Of special note in the *Lehrbuch* are remarks that appear on the concluding pages, in which he speculates about the possibility of earthquake prediction and admits that the density of seismographs at the time was too small to allow for the collection of adequate data to help with this question. Gutenberg's optimism was in sharp contrast to the pessimism of his later colleague Richter.

Wiechert died in March 1928, and Gutenberg was on the list to succeed him at Göttingen. He also had hoped to be the successor to Angenheister at Potsdam. But these hopes were not fulfilled, and he still could not find a permanent, decently paying position, despite his Olympian reputation among geophysicists. There are indications that his Jewish background played a part in all this. Indeed, Max Born remarked, in the case of the rejection of von Karman's candidacy, that there was concern over the number of Jews in the science faculty at Göttingen.

The *Lehrbuch* was a precursor to the even more monumental *Handbuch der Geophysik,* which was planned to be a series of ten volumes. Gutenberg accepted and successfully carried forward the editing of this daunting task. Although the job of editing the encyclopedia was accepted before his departure for Pasadena in 1930, the work continued afterward. The volumes appeared in irregular order; I was unable to locate volume 5, and it may not have been published. Volumes 1, 2, 4, 6, 7, and part of volume 9 appeared between 1931 and 1936. In 1937 the Nazis removed his name from the enterprise. Although the editorship of volume 3 (1940) was given to Weickmann, Gutenberg's chapters on forces in the Earth's crust and geotectonic hypotheses were not removed. Strangely, Gutenberg's bylines for these two sections in volume 3 identify him as being at Frankfurt, even though he had left for Pasadena ten years earlier. Linke's name appears as editor of volume 8.

The last part-volume to appear, volume 8(3), did so in 1955, thirteen years after the appearance of the volume preceding it. It was a slender and poor shadow of the earlier volumes. Clearly, without Gutenberg at the helm, the enthusiasm for the project had dissipated. Gutenberg contributed about 750 pages of text to chapters in volumes 2, 3, 4, and 9 between 1932 and 1940.

In 1921 the Carnegie Institution of Washington initiated studies in seismology at its Mount Wilson Observatory laboratory in Pasadena in cooperation with the California Institute of Technology. Laboratory director H.O.Wood with the concurrence of J.C.Merriam, president of the Carnegie, focused on understanding California earthquakes. Wood's scientific interests were mainly in instrumentation used to locate epicenters of earthquakes in southern California; faults then could be identified and statistics of local earthquakes could be constructed so that they might contribute to the prediction of strong earthquakes. To do this he had constructed a small but effective network of identical seismographs of his own design. The network was inaugurated in 1923 with Pasadena as the focal station.

In 1927 the Carnegie activity in seismology was moved to a new Seismological Laboratory that had been built by Caltech. By 1929 it was clear that the Carnegie program diverged from the directions that R.A.Millikan and the Caltech faculty thought important, which was that seismological studies should address global as well as regional problems. After all, could not the growing collection of records of near and distant earthquakes gathered by the best local network of instruments in the world be applied to the more basic problems of seismic wave propagation and the structure of the Earth?

A meeting of leading seismologists was convened in October 1929 to assess the program, and Gutenberg, Jeffreys, Byerly, and Macelwane attended as external visitors. (This was the first time that the two giants of geophysics and seismology—Jeffreys and Gutenberg—had met.) One of the recommendations was for the establishment of a chair at Caltech. It was clear that the decision to establish the chair had been made before the meeting had convened. As Gutenberg left Pasadena, Millikan remarked significantly that he hoped he would be seeing Gutenberg soon. Gutenberg, reflecting on the arduous sea and land voyage, thought not. Upon hearing the rumor that Harvard was also interested in Gutenberg, Millikan accelerated the process. Millikan's telegram arrived in Darmstadt two months after the meeting, “Could you consider seismological post here if satisfactory arrangements could be made?” Gutenberg's wire, mimicking Millikan's, read, “Would consider post if arrangements satisfactory.” Six weeks after the telegram, the official letter arrived in Darmstadt in January 1930, offering him a full professorship in geophysics and meteorology at Caltech, as well as an appointment at the Seismological Laboratory.

Linke and the science faculty at Frankfurt were thus prodded to make a counteroffer. Frankfurt organized a search for a position in Germany for Gutenberg, but the hasty effort was half-hearted and expectedly unsuccessful. After some correspondence, mainly concerning salaries, in which Millikan raised his initial offer by 40%, Gutenberg accepted the position at Caltech (June 4, 1930); the dean of sciences at Frankfurt congratulated him. In September 1930, Beno, Hertha, and their children Arthur and Stephanie came to the United States to start a new life at Caltech. The beginning of the rapid development of the laboratory coincides with Gutenberg's arrival in Pasadena.

In 1936 responsibility for the Seismological Laboratory was transferred from the Carnegie Institution of Washington to the California Institute of Technology. From 1935 onward, Gutenberg was the de facto head of the Seismological Laboratory because of the illness of Wood. Although Wood had resisted the proposal to change the direction of the laboratory away from the phenomenology of earthquake occurrence and toward theory and interpretation, he stated when Gutenberg was appointed that the effort in southern California needed Gutenberg's talents and experience to help in the determination of epicenters and origin times (a misuse of Gutenberg's talents), adding “We need Gutenberg more than Europe does.” Wood persisted in his belief that the focus of the laboratory should be on local seismicity and not on global geophysics. Wood remarked in 1938 that the true cost of his illness could be measured by the relentless effort of the Seismological Laboratory on distant earthquakes.

Even in Gutenberg's first year at Pasadena, it was clear that the work of the laboratory was undergoing a revitalization that reflected his presence. At the annual meeting of the Seismological Society of America in 1931, six of the fourteen papers on seismology came from Caltech, and three of these bore Gutenberg's name. In contrast, at the meeting of the society in 1929, there were no contributions from the Seismological Laboratory among the five papers on seismology. Over the years, many students and colleagues came to Pasadena, attracted by the wealth of new ideas and methods of doing earthquake research and by the intellectual power of Gutenberg and his two colleagues Benioff and Richter. The global center of Seismological research had shifted from Darmstadt to Pasadena.

Gutenberg had now moved from aseismic Hessen to the active seismic environment of southern California. Having spent his life to that time studying seismic wave propagation from distant earthquakes, his new career allowed him to study earthquakes at close range. He was now able to work on both local and global Seismological problems. He could use the records from the Caltech network of Wood-Anderson torsion seismometers, later to be upgraded to the electromagnetic seismographs invented by Benioff. Gutenberg quickly realized that the network could be used for more than the location of local earthquakes. The recordings could be applied not only to the study of local Earth structure in southern California but also to structural issues of a global nature, to Wood's dismay, as remarked. Gutenberg attacked the issues of microseisms in California and those of the structure of the Earth's crust in California. He used the laboratory's archive of seismograms from a number of southern California earthquakes to determine local Earth structure (1943).

Wood retired in 1947, and from that time Gutenberg was the official director as well. While this appointment clarified matters at the laboratory and generated coherence to its work, it had the unavoidable disadvantage of taking much of Gutenberg's research time for administration. He worked hard to stabilize the relationship between the Caltech administration and the laboratory during some difficult times.

On the global stage, Gutenberg and Richter wrote four monumental papers, *On Seismic Waves* (1934, 1935, 1936, 1939), covering the problems of travel-times for the many body waves in the Earth, amplitudes, surface waves, and deep-focus earthquakes. A bible for the observationalist, the papers represent the foundations of modern observational seismology. Although the titles reflect those in the earlier series of the Göttingen school, they are in no sense a revisiting of the older; rather, they are a magnificent original contribution containing all that was known about observations of the propagation of seismic waves through the Earth. The travel-time curves in the first of these papers (1934) were an exhaustive catalog of the properties of most of the identifiable seismic phases, including phases involving many multiple bounces off the surface of the Earth from within; multiple reflections off the core boundary; multiples within the core; and many mixed phases, such as ScSSKP and others. The travel-time curves of the 1934 paper preceded the Jeffreys-Bullen curves by one year, and the two sets of curves were in excellent agreement for the important phases.

The travel-time relations in the Gutenberg-Richter curves for roughly 30 phases are identified only by letters. Of particular interest is the branch labeled "G." This was a long-period wave with strikingly large amplitudes from the Solomon Islands earthquake of October 3, 1931, to which Gutenberg and Richter (1954) assigned magnitude 7.9 (the periods were as long as 135 sec). The large amplitudes were even more striking in view of the short-period pass-band of the seismometers of the time. Gutenberg and Richter identified the G-waves as horizontally polarized shear surface waves, which we now identify as very long-period Love waves. For the Solomon Islands earthquake, two later arrivals of the G-waves, labeled G2 and G3, were observed at some stations of the world. Gutenberg and Richter identified G3 as the G-wave phase, which had traveled one complete circuit of the Earth farther than G1. The observation that surface waves can undergo many circuits of the Earth is an important preliminary to the study of the free oscillations of the Earth that began in the 1960s.

In his work on the core, Gutenberg had noted that the wave motions at sites in the shadow zone did not vanish. He assumed that these waves were the consequence of diffraction by the boundary of the core. In her 1936 paper entitled "P'" Lehmann showed that the amplitudes of these waves in part of the shadow zone were too large, and she proposed that they were due to an inner core of the Earth lying within Gutenberg's core. Gutenberg and Richter (1938) reanalyzed the data on P' in the shadow zone and, using realistic models for the velocities in the core, concluded that the boundary of the inner core involved a gradual transition over a distance of 300 km starting at a radius that was 100 km less than Lehmann's value. Their P-wave velocity in the inner core of 11.3 km/sec was 2.7 km/sec higher than Lehmann's and was precisely consistent with the current estimate derived from analysis of the free oscillation spectrum of the Earth. The transition zone has disappeared from modern models of the inner core/outer core boundary. In the 1914 paper, the P-wave velocity at the top of the core was 8.5 km/sec. In view of the high velocities in the inner core, this value was lowered in the 1938 paper to 8.0 km/sec, a figure that is the same as the value today.

Jeffreys has described Gutenberg as being hesitant to ac cept the liquidity of the core. Gutenberg was quite aware of the non-seismological arguments for a liquid core from the tides and from the figure of the Earth. But in Gutenberg's view, a very low but finite velocity for S-waves corresponding to a low rigidity could still imply that the core could be mechanically weak, and there was no way to identify absolutely the absence of S-wave propagation through the core. The absence of observable S-wave propagation in the core could only place an upper bound on the value of the velocity. In the 1914 paper setting forth the discovery of the core, Gutenberg calculated the arrival times of S-phases through the core under the assumption that the Poisson's ratio for the core was 0.27, which would have given velocities around 4.7 km/sec, but he found no arrivals at these times. In the 1923 paper for the elastic constants in the interior, Gutenberg's velocity-depth curves show that the S-wave velocities in the core are less than about 1.2 km/sec. In later papers, the bound is lowered progressively. By 1959 (pp. 277–79), taking the tidal and figure-of-the-Earth issues into account, the velocity for the still unobserved S-waves was “probably less, possible much less than” 0.3 km/sec; the value for velocity is my interpretation of his threshold for the shear modulus.

In work done in Germany, Gutenberg used the variation of amplitudes to deduce the existence of a weak low-velocity zone for P-waves in the upper mantle at a depth of between 70 and 80 km (1932, vol. 4, p. 213). He also speculated that the low-velocity zone might start at the top of the mantle. For more than twenty years, he steadfastly held to his position of the existence of the zone, even though there were times when he was almost alone in this view. In 1949, using southern California data, he lowered the depth to the low-velocity zone to around 100 km below the surface; the zone had a much stronger contrast to the regions above and below than in his earlier study. He identified an additional low-velocity zone in the upper crust of southern California just above the Conrad discontinuity (1934), which was confirmed by showing that the arrivals of P-waves from explosions were earlier than those from earthquakes (1951). Gutenberg also speculated on the possibility that there were also low velocities in the lower crust above the Mohorovičic discontinuity. In the 1950s his tenacious view began to be accepted, especially from the evidence given by Press and Ewing in 1956 and by later authors of dispersion of surface waves by the mantle channel.

Throughout his career, Gutenberg was concerned with the horizontal mobility of the continents. How was horizontal mobility driven? His early models (1930) for continental drift involved centrifugal and nonisostatic forces; forces due to thermal contraction were added later. In September 1950 in Hershey, Pennsylvania, Gutenberg was a co-organizer of and active participant in a conference on flow in the Earth's interior, where much discussion centered on convection (1951). One of the conclusions of the conference was the importance of convection in the mantle of the Earth driven by gravitational instabilities of thermal origin. The proponents of convection David Griggs, Harry Hess, and Felix Vening Meinesz were prominent. The opposite view was represented by Francis Birch. Gutenberg stated, "…we observe at the surface, phenomena that are possibly connected with convection currents at spots where the currents are going down (or coming up) and that all such observations refer to belts surrounding the Pacific basin," and that the bottom of the Pacific is moving in the same direction relative to the continents in California, Japan, Philippines, and New Zealand. These may be prescient statements of the present view that the major part of subduction takes place around the rim of the Pacific and about plate motions, although plate tectonics was eighteen years in the future.

Gutenberg's ideas about convection must have been encouraged by the 1950 conference, because convection in the mantle appears as an important component of his thinking. In *Internal Constitution of the Earth* (1951), he suggests that convection may be the most potent of all mechanisms for causing continental drift. He also constructed a temperature profile for the mantle to a depth of 600 km based on the assumption that convection below a depth of 80 km will lower the temperature gradient. The depth of 80 km was chosen to correspond to the depth of his low-velocity zone, a zone that he associated with low strength due to elevated temperature. In today's language, the lithosphere, that is, the uppermost 80 km of the Earth (his value for the thickness) and especially under the oceans, moves as a consequence of mantle convection. The temperatures below 80 km were significantly lower than present estimates. Later, in *Physics of the Earth's Interior* (1959), he extended the temperature curve smoothly to the center of the Earth. In making the extension, Gutenberg relied on estimates of the melting temperature of iron at core and inner core pressures that were considerably lower than present-day estimates, and thus derived rather lower temperatures than present-day estimates.

Gutenberg had never experienced an actual earthquake until a few years after his arrival in Pasadena. The story often has been told that Einstein and Gutenberg were walking across the Caltech campus in the late afternoon of March 9, 1933, so deeply engrossed in their scientific conversation that they failed to notice the shaking of the ground in the disastrous Long Beach earthquake and were only made aware of the earthquake by colleagues shortly thereafter. The story was confirmed by wives of both men, so it must have been true. Gutenberg did feel many of the large aftershocks of the Long Beach earthquake.

The large number of earthquakes in southern California presented a natural challenge to quantify the earthquake process. The start was the construction of a magnitude scale for local earthquakes *(M_L)* in southern California using the uniformity of the Wood-Anderson seismographs in the local network. Gutenberg had an important influence on Richter's publication in 1935 of the local magnitude scale. Shortly thereafter, Gutenberg and Richter (1936) constructed the surface wave magnitude scale for distant earthquakes *(M_S),* and the surface wave magnitudes were normalized according to the local scale. For the first time, the magnitudes of the largest earthquakes were identified as having magnitudes from 8 to 8.5. Gutenberg and Richter established the magnitude scale for deep earthquakes, which do not excite surface waves (1945). The magnitude scale for local earthquakes was mainly due to Richter; the magnitude scale for distant earthquakes, with application to the largest earthquakes, was due to both men, with Gutenberg as the prime mover. Some people have expressed unhappiness that the magnitude scale has not been called the Gutenberg-Richter scale.

Magnitudes are of importance in that they provide a unifying parameter about which a variety of properties of earthquakes can be related (1942). The development of the magnitude scale, Gutenberg's calculation of the energies radiated in seismic waves (1956), and Benioff's ideas about strain accumulation and relaxation allowed for the opening of a new science: the study of the Earth's seismicity. This was first explored in the monumental book by Gutenberg and Richter, *The Seismicity of the Earth* (1954), in which the statistics of local earthquakes were described. This was the first major catalog of great earthquakes worldwide, classified not only as to time and location but also as to magnitude. The statistics showed the famous universal, log-linear frequency magnitude relation with a slope close to 1.0. The calculation of energies for selected earthquakes had been done earlier (1929, pp. 187, 296). Gutenberg now embarked on a study to relate the energy released in an earthquake to its magnitude. For the first time the energy flux in earthquakes could be calculated. The energy-frequency relation could now be shown to be a power law with exponent close to −2/3. In later years, the power law relation has been a paradigm for the modeling of seismicity and the earthquake process.

In 1926 the National Research Council appointed a committee to prepare a report entitled *Internal Constitution of the Earth.* Little appears to have been done until 1937, when Gutenberg was appointed to chair the committee and was charged with the task of reorganizing it. With characteristic vigor, the task was completed in short order, and the volume appeared in 1939; the printing was exhausted quickly. In view of the progress made since 1939, a second revised edition appeared in 1951 with major changes from and additions to the first edition. The purpose of the volume was to give a scientific reader outside the field an overview of the status of the field and an identification of the unsolved problems. The volume responded to the original charge, but it was also a resource of great completeness where students could find copious references to additional material. It thus became almost immediately a major and highly cited scientific resource for the evidence and interpretation of the composition, temperature, elastic properties, figure, density, gravity, origin and evolution, and the stress-strain state of the interior. The volume shows that major new results and major new ideas had developed in the years since the *Handbuch.* Gutenberg was no mere managing editor, having written more than half of the volume himself. Where differences of opinion arose among the authors, no effort was made to reach consensus; each author developed his material unrestrained.

Only months before his death, Gutenberg's last book *Physics of the Earth's Interior* (1959) was published. A companion volume on seismology was planned, but death intervened. The book focuses on many of the same subjects as *Internal Constitution,* but this time the exposition is a personal, systematic journey through crust, mantle, and core before attacking the issues of temperature, density, tides, etc. The book is a remarkable summary of the properties of the interior of the Earth and an accounting of the vast number of sources for this information, many of them coming from Gutenberg's own measurements. Gutenberg showed himself to have been a voracious reader of the literature. The book is a deep, thoughtful, and thorough monograph on geophysics and proves that Gutenberg was not merely a great seismologist. Remarkable in his swan song is his prescient identification of topics that shortly would elicit much interest. I mention two such areas.

The first topic was his brief identification of the importance of studies of the resonance spectrum of the Earth. Would not Gutenberg have been excited by the unprecedented observations of the rich resonance spectra that resulted from recordings of the great Chilean earthquake of 1960, which occurred four months after his death? The observations of these spectra triggered a grand flowering of activity in inverse theory and ultra-long period seismology; in particular, density estimates for the Earth's interior could be derived from the spectra without assumptions about compressibility or other properties.

The second topic was Gutenberg's commitment to the ideas of convection and continental drift, although he did not connect the two. That linkage was not to happen for another nine years, with the development of the plate tectonics model. However, he made a definite insightful assertion that convection would be shown to be responsible for mountain building and earthquakes, and from his reconstruction of the continental geometry into an almost single mass in Cretaceous times, that climates on the long-time scale will depend on understanding continental drift.

After the assumption of power by the Nazis, Gutenberg kept his contacts in Germany alive. During these prewar years, he helped many Jewish scientists escape from Germany. Notable among these were Viktor Conrad, editor of *Gerlands Beiträge,* and Helmut Landsberg. Landsberg had been Gutenberg's student and was his successor as director of the earthquake service at the Taunus observatory. In 1934 Landsberg fled to the United States with Gutenberg's help and became professor at Pennsylvania State College in College Park.

Gutenberg returned to the problem of microseisms and their meteorological causes while working on a project for the U.S. Navy during and after World War II. Gutenberg was a valuable World War II consultant to the Navy, applying his knowledge of the structure of the upper atmosphere to the problems of ballistics. He also worked on applications of the observations of microseisms to locate hurricanes in the Caribbean and the western Pacific. Within a few weeks of the war's end, Gutenberg wrote to the Navy stating that for many years he had been suggesting the use of microseisms for forecasting of storms, especially hurricanes. No longer were microseisms associated with surf, he asserted, but now they were associated with differential loading of the ocean bottom by distant storms. The theory was given by M.Longuet-Higgins in 1950. The results that he had obtained in the Caribbean area exceeded by far his most optimistic hopes. In the spring of 1947 the Navy sent Gutenberg to Japan, Guam, and the Philippines to do additional research on microseisms, as well as to consult on the problems of the revival of seismological research in these countries in the wake of the war.

Gutenberg was small of stature, very personable, and lively. He was well organized and kept to a precise daily schedule. Although his scientific demands on himself were rigorous, Gutenberg was gentle and self-effacing in his relationship with others. He was helpful to anyone who asked a question of him and was tolerant of critics. Gutenberg was a man who could give his colleagues and students a liberal education in scientific method, made pleasantly easy by kindness, patience, amazing industry, and a delightful sense of humor. He was a cultured individual, well read, and with wide interests—reflections of his broad European education.

Having inherited his mother's musical talents, Beno learned to play the piano, a skill that was to last him through his entire life. In his earliest years in Darmstadt, Beno sang in the synagogue choir and often played the organ. In the Pasadena years, Einstein played the violin in chamber music events organized at the Gutenberg home. At the end of World War II, in a reprise of the bartering activity of earlier years, Gutenberg collected the accumulated royalties on his publications in Germany by payment in the form of numerous piano scores for two and four hands.

His bookplate shows the Owl of Wisdom with a seismogram in its beak in flight around the Göttingen Institute of Geophysics. The text on the bookplate repeats the motto of the *Lehrbuch:*

Viele Zeichen gibt uns die Natur,
Leitet uns auf der Erkenntnis Spur,
Weist uns ihre wunderbaren Bahnen,
Lässt die Seele uns des Weltalls ahnen!

Frank Press, former president of the National Academy of Sciences, has stated, “Gutenberg was absolutely dedicated to seismology, especially to observational data and their interpretation. His work carries the mark of much self-confidence in his ability to examine data not as a statistician but as a skillful interpreter and synthesizer.” Throughout his entire career, Gutenberg's reputation rested on the solid foundation of his own reading with great accuracy and insight of the arrival times and other properties of seismic waves on the seismic records. In contrast to the modus operandi of many modern scientists, Press continues, “Gutenberg could draw conclusions from sparse and noisy data with uncanny insight that” structures such as a core and a low-velocity zone and continent-ocean differences could be stated to exist. Gutenberg's belief in the power of the data to resolve issues of differences in models is given by the penultimate sentence of *Physics of the Earth's Interior.* THE DATA “MUST BE GREATLY AMPLIFIED AND STRENGTHENED” (Gutenberg's capitalization and punctuation). Gutenberg dominated the field of observational seismology as no one before or after. At the time of his death, Byerly remarked, “It is rare that anyone writes a paper in seismology without referring to him.”

When one reads the list of Beno Gutenberg's contributions to the full range of seismological studies—spanning seismicity, wave propagation in the Earth, and the physics of the Earth's interior—one must be in awe of his insights, his breadth, his thoroughness, his vigor, and especially his creativity.

I AM MOST GRATEFUL to Caltech for allowing me access to a variety of documents in its archival files of Beno Gutenberg and Charles F. Richter and to a transcript of an oral history interview with Hertha Gutenberg. The history of the Seismological Laboratory as given in

Milikan's School by Judith R.Goodstein (W.W.Norton, New York, 1991) was valuable. I have also found the following biographical articles helpful:

P.Byerly. *Trans. Am. Geophys. Union* 34(1954):353–54.
C.F.Richter. *Proc. Geol. Soc. Am.,* Annual Report for 1960, (1962):93– 104.
H.Jeffreys. *Q. J. Astron. Soc.* 1(1960):239–42.
J.Schweitzer. *Mitteil. Deutsch. Geophys. Gesell.* 3(1989):8–10.
D.L.Anderson. In *Encyclopedia of Earth Sciences,* vol. 1, pp. 444–45. Macmillan: New York, 1996.

If it should be perceived that I have appropriated some of the language in the above documents, the accusation is well founded. These authors have been a most valuable resource, not only for their thoughts on the life and science of Beno Gutenberg but also in their admirable choice of words.

HONORS

1945	Member, National Academy of Sciences Honorary member, Royal Society of New Zealand
1945–47	President, Seismological Society of America
1947	Foreign member, Academia dei Lincei Honorary member, Finnish Geographical Society
1948	Foreign member, Finnish Academy of Letters and Sciences
1949	Foreign member, Royal Swedish Academy of Sciences Member, Washington Academy of Sciences
1950	Fellow, American Academy of Arts and Sciences
1951–54	President, International Association of Seismology and the Physics of the Earth's Interior
1952	Prix Lagrange, Royal Belgian Academy of Sciences
1953	Bowie Medal, American Geophysical Union
1954	Foreign Member, Geological Society (London)
1955	Honorary doctorate, University of Uppsala
1956	Emil-Wiechert Medal, German Geophysical Society

SELECTED BIBLIOGRAPHY

1912 With K.Zöppritz and L.Geiger. Über Erdbenwellen V.Konstitution des Erdinnern, erschlossen aus dem Bodenverrückungsverhältnis der einmal reflektierten zu den direkten Longitudinalwellen, und einige andere Beobachtungen über Erdbenwellen. *Nachr. d. Kön. Ges. d. Wiss. Göttingen, math.-phys. Kl.* 121–206.

With L.Geiger. Über Erdbebenwellen VI. Konstitution des Erdinnern, erschlossen aus der Intensität longitudinaler und transversaler Erdbenwellen, und einige Beobachtungen an den Vorläufern. *Nachr. d. Kön. Ges. d. Wiss. Göttingen, math.-phys. Kl.* 623–75.

Die seismische Bodenunruhe (dissertation). *Gerl. Beiträge z. Geophys.* 11:314–53.

1914 Über Erdbenwellen VIIA. Beobachtungen an Registrierungen von Fernbeben in Göttingen und Folgerungen über die Konstitution des Erdkörpers. *Nachr. d. Kön. Ges. d. Wiss. Göttingen, math.-phys. Kl.* 125–76.

1923 Die elastischen Konstanten im Erdinnern. *Phys. Zs.* 24:296–99.

1924 Dispersion und Extinktion von seismischen Oberflächenwellen und der Aufbau der obersten Erdschichten. *Phys. Zs.* 25:377–81.

1925 *Der Aufbau der Erde*. Berlin: Gebr. Bornträger.

1926 Die Geschwindigkeit des Schalles in der Atmosphäre. *Phys. Zs.* 27:84– 86.

1927 Die Veränderungen der Erdkruste durch Fliessbewegungen. *Gerl. Beiträge z. Geophys.* 16:239–47; 18:281–91.

1929 Die physikalische Vorgänge bei Erdbeben. In *Lehrbuch der Geophysik,* ed., pp. 220–307. Berlin: Gebr. Bornträger.

Der Physikalische Aufbau des Erdkörpers. In *Lehrbuch der Geophysik,* ed., pp. 536–81. Berlin: Gebr. Bornträger.

1930 Schallgeschwindigkeit und Temperatur in der Stratosphäre. *Gerl. Beioträge z. Geophys.* 27:217–25.

Hypotheses on the development of the Earth. *J. Wash. Acad. Sci.* 20:17–25.

1932 Theorie der Erdbebenwellen. In *Handbuch der Geophysik,* vol. 4, ed., pp. 1–150. Berlin: Gebr. Bornträger.

Beobachtungen von Erdbebenwellen. In *Handbuch der Geophysik,* vol. 4, ed., pp. 151–263. Berlin: Gebr. Bornträger.

Der Aufbau der Atmosphäre. In *Handbuch der Geophysik,* vol. 9, ed., pp. 1–88. Berlin: Gebr. Bornträger.

Die Schallausbreitung in der Atmosphäre. In *Handbuch der Geophysik,* vol. 9, ed., pp. 89–145. Berlin: Gebr. Bornträger.

1933 Abkühlung und Temperatur der Erde. In *Handbuch der Geophysik,* vol. 2, ed., pp. 1–35. Berlin: Gebr. Bornträger.

Der Physikalische Aufbau der Erde. In *Handbuch der Geophysik,* vol. 2, ed., pp. 440–564. Berlin: Gebr. Bornträger.

1934 The propagation of the longitudinal waves produced by the Long Beach earthquake. *Gerl. Beiträge z. Geophys.* 41:114–20.

With C.F.Richter. On seismic waves. I. *Gerl. Beiträge z. Geophys.* 43:56–133.

1935 With C.F.Richter. On seismic waves. II. *Gerl. Beiträge z. Geophys.* 45:280–360.

1936 With C.F.Richter. On seismic waves. III. *Gerl. Beiträge z. Geophys.* 47:73–131.

The structure of the Earth's crust and the spreading of the continents. *Bull. Geol. Soc. Am.* 47:1587–1610.

1938 With C.F.Richter. P′ and the Earth's core. *Mon. Not. Roy. Astron. Soc. Geophys. Suppl.* 4:363–72.

1939 With C.F.Richter. On seismic waves. IV. *Gerl. Beiträge z. Geophys.* 54:94–136.

1940 Kräfte in der Erdkruste. In *Handbuch der Geophysik,* vol. 3, ed., pp. 1– 31. Berlin: Gebr. Bornträger.

Geotektonische Hypothesen. In *Handbuch der Geophysik,* vol. 3, ed., pp. 442–544. Berlin: Gebr. Bornträger.

1942 With C.F.Richter. Earthquake magnitude, intensity, energy and acceleration. *Bull. Seismol. Soc. Am.* 32:163–91.

1943 Seismological evidence for roots of mountains. *Bull. Geol. Soc. Am.* 54:473–98.

1945 Magnitude determination for deep-focus earthquakes. *Bull. Seismol. Soc. Am.* 35:117–30.

1948 On the layer of relatively low wave velocity at a depth of about 80 kilometers. *Bull. Seismol. Soc. Am.* 38:121–48.

1951 The cooling of the Earth and the temperature in its interior. In *Internal Constitution of the Earth,* ed., pp. 150–66. New York: Dover.

Forces in the Earth. In *Internal Constitution of the Earth,* ed., pp. 167– 77. New York: Dover.
Hypotheses on the development of the Earth. In *Internal Constitution of the Earth,* ed., pp. 178–226. New York: Dover.
With C.F.Richter. Evidence from deep focus earthquakes. In *Internal Constitution of the Earth,* ed., pp. 305–313. New York: Dover.
With C.F.Richter. Structure of the crust. In *Internal Constitution of the Earth,* ed., pp. 314–39. New York: Dover.
The elastic constants in the interior of the Earth. In *Internal Constitution of the Earth,* ed., pp. 364–81. New York: Dover.
With H.Benioff. Strain characteristics of the Earth's interior. In *Internal Constitution of the Earth,* ed., pp. 382–407. New York: Dover.
With H.Benioff, J.M.Burgers, and D.Griggs. Colloquium on plastic flow and deformation within the Earth. *Trans. Am. Geophys. Union* 32:497–543.
Travel times from blasts in southern California. *Bull. Seismol. Soc. Am.* 41:5–12.
1953 Wave velocities at depths between 50 and 600 kilometers. *Bull. Seismol. Soc. Am.* 43:223–32.
1954 With C.F.Richter. *Seismicity of the Earth and Associated Phenomena*. Princeton, N.J.: Princeton University Press.
1956 The energy of earthquakes. *Q. J. Geol. Soc. Lond.* 112:1–14.
1958 Velocity of seismic waves in the Earth's mantle. *Trans. Am. Geophys. Union* 39:486–89.
1959 *Physics of the Earth's Interior.* New York: Academic Press.

MARY R.HAAS

January 12, 1910–May 17, 1996

BY KENNETH L.PIKE

THE WORK OF MARY HAAS has a special fascination for me, since she finished her doctoral dissertation on Tunica, an American Indian language, in 1935, the same year that I started my studies of linguistics (with the Summer Institute of Linguistics) and went to Mexico to study an Indian language (Mixtec). This was the explosive age of descriptive linguistics, which we shared and which was especially focused on American Indian languages. She studied with Sapir and some of the other leaders, as I did (I got my clue to the analysis of tone from Sapir at one of the early summer sessions of the Linguistic Society of America at the University of Michigan).

Haas's first article (on Nitinat spoken on Vancouver Island) was published in 1932 jointly with Morris Swadesh, her husband from 1931 to 1937, whose articles on phonemics in 1934 and 1937 were useful to me, supplementing work by Leonard Bloomfield and Edward Sapir. Haas's early concentration was on the description of American Indian languages of North America; later she and other descriptive linguists shifted their attention to the east during the war to help the U.S. Armed Forces understand languages that had not been well known to Americans.

I asked Paulette Hopple, who worked in Thailand for many years with the Summer Institute of Linguistics, for a comment on Haas. She replied, "I first met Mary Haas in 1979 at the Sino-Tibetan Conference in Paris, where we discussed numeral classifier systems in Mayan, Thai, and Burmese. Although delighted and awed by her knowledge and experience in linguistics, what intrigued me about Mary was her personal approachability and humility. She communicated personal interest, compassion, a gentle sense of humor, including an ability to laugh [at difficult] circumstances."

BRIEF SUMMARY OF PROFESSIONAL CAREER

Haas was born in Richmond, Indiana, graduated there from high school and college, did graduate work (1930–31) in Chicago on comparative philology, and did her Ph.D. in linguistics (1931–35) on the American Indian language Tunica at Yale. After that, she carried on various research tasks on American Indian languages under the anthropology department at Yale and the American Philosophical Society, 1935–41; on Thai, 1941–45, under the American Council of Learned Societies; and research in connection with her appointments at the University of California, Berkeley, 1946–53. Along with her research, she had various regular university appointments at Berkeley: lecturer in Siamese (Thai) for an Army training program, 1943–44; lecturer in Siamese and linguistics, 1947–53; associate professor, 1953–57; professor, 1957–77; acting chairman of linguistics, 1956–57; and chairman, 1958–64. She had numerous short-term (e.g., summer or one-semester) appointments for lectures in anthropology, Thai, or linguistics in various places in the United States and Canada. She received honorary doctorates from Northwestern University (1975), University of Chicago (1976), Earlham College, Richmond, Indiana (1980), and Ohio State University (1980). She was a member of various professional societies, including the National Academy of Sciences (1978). In addition, she was vice-president of the Linguistic Society of America in 1956 and president in 1963.

PHONOLOGY BITS AMONG THE TUNICA

Because of my own interest in phonology for the years 1935–50, I will start by discussing a few of the phonological issues that Haas faced in studying the North American language Tunica (an "isolate" with historical relationships not clear). The consonants of Tunica (Haas 1941, pp. 13–14) have one surprise: the voiced stops /b, d, g/ occur "only in a few isolated words (of foreign or probably foreign origin)"; but the voiceless fricatives have no voiced counterparts. Each syllable begins with a consonant, some end with a consonant, some clusters of two consonants come medially in a word, and some consonant clusters may be preceded by /n/. Vowels are normally short, unless in stressed syllables.

In Tunica (1941, pp. 19–20) stressed syllables with their pitch relations are also of interest to me. The first stressed syllable of a phrase may be stronger than the unstressed ones and is often (but not necessarily, and not with semantic implications) a bit higher in pitch. Stressed syllables as a whole, however, enter into various "phrasal pitch contours," or "melodies." In them, a final stressed syllable may be a bit higher than the penultimate one, or the final one may have a falling melody, or it may have a rising one, or a falling-rising one, or may be lower than the preceding syllable. Some monosyllabic prefixes (and some other forms) have special phonological rules (pp. 20–34), which I do not summarize here. The predicative word of a main clause (p. 89) will have high melody if it is indicative, low if quotative, rising if interrogative, and falling if imperative.

SOME MORPHOLOGICAL BITS IN TUNICA

With her Tunica (1941) Haas has some morphemic analysis of long words. This is interesting, since there are numerous words with up to six syllables in agglutinative arrangement. For example (p. 52), "The semelfactive paradigm consists of a causative stem plus the semelfactive forms of the causative auxiliary." For example:

?uhpíhusíntak?ahča

They would hide him (literally, cause him to hide) from *?uhk-+píhu..c.* to cause…to hide (hence "to hide")+ *-sínta,* feminine dual or plural semelfactive,+-*k?ahča,* future positive. [Note: ? signifies a glottal stop.]

For a full text with detailed analysis see pp. 135–43. Unfortunately, in her presentation it is often very difficult for the beginner to see where morphemes in a word or phrase begin or end.

A NOTE ON TUNICA SYNTAX AND TEXTS

Haas has a discussion of syntax (1941, pp. 89–134) with texts illustrated on pp. 135–43, and in 1950 with extensive texts (with notes giving morphemic analyses). She discusses (1941, pp. 90–91) simple versus compound and complex sentences (with compound ones having two or more main clauses and complex ones having a main clause plus one or more subordinate clauses of dependent, complementary, relative, or adverbial types). The following illustration is a simple sentence with just one clause:

Háyišíku,	*tóníku, ?*	*uhká'lin?uhkéni*
The One above	man	created it is said.
indep. subj.	indep. obj.	predicate word

Clauses (pp. 91–93) are of two principle types, main and subordinate. Subordinates are dependent (subordinate only to the main verb), complementary, relative, and adverbial. The dependent clauses have a subordinating postfix on the predicate (details on pp. 91–102; noun classification for gender and number, pp. 102–03; preverbs and postfixes, pp. 114–26; word classes in syntactic uses, pp. 126–34, including exclamatives and imitatives, p. 134).

Texts (1950) include myths (solar, thunder, origins of corn or beans); tales (about eagles, owls, submarine people); animal stories; historical or pseudohistorical narratives (revenge, migrations, robberies); personal narratives (about families, or rabbits, or war); ethnological data (about food types, house construction, fire, fever remedies, shooting ghosts); and miscellaneous (one-eyed beings, water animals, woodpeckers, the ocean dried up). These occur in Tunica in English translation with footnote alternative literal translations or comments.

A RESEARCH AND TEACHING SHIFT TO THAI

In 1941 Haas started fieldwork in the phonology and syntax of Thai (Siamese) at the University of Michigan because of the need for speakers of Asiatic languages as war developed. She had help in this from the American Council of Learned Societies. (She married one of the speakers of Thai, Heng R.Subhanka; they were divorced some years later.) While doing research on Thai (1942–43), she was concurrently an instructor in oriental languages at the University of Michigan. Moving to the University of California, Berkeley, she lectured on Thai for the Army Specialized

Training Program. Her publications on Thai were considerable, for example, *Spoken Thai,* book I in 1945 and book II in 1948, with Heng R.Subhanka and the *Thai-English Student's Dictionary* (1964).

THAI TONES

In 1958 Haas discussed the tones of four Thai dialects with tone patterns differing in their relation to consonants and to geographical occurrence. Thai itself has high, mid, low, rising, and falling tones (the dialect of Nakhonsithammarat has seven tones). Proto-Thai presumably had four tone categories, the first three of which were found only with a syllable having a long vowel, semivowel, or nasal, while the other occurred only with syllables having a final stop; and the initial consonant (voiceless versus voiced) conditioned the development of the different tones. For Thai, note:

High in *nóg,* “bird”
Mid in *bin,* “to fly”
Low in *sìb,* “ten”
Rising in *măa,* “dog”
Falling in *kâw,* “nine”

A NOTE ON THAI WORDS AND SYNTAX

Many Thai words are comprised of single syllables. Some samples were given above in the illustration of tones. Many more are given in Haas 1955. Some of these combine to make complex words. For example (p. 264):

Khwaam: the sense, substance (as of a letter), but in special usage often placed in front of a verb to form an abstract noun. May often be translated -*ness, ity, -th, -tion,* etc. Thus, *khwaamklua* is “fear.”

However (1962, p. 49), “Since Thai is conventionally written without any spaces between words, the English-speaking student has no clue as to which elements form a semantic unit and which do not.” As for syntax (1964, p. *xx*): “The typical sentence contains subject, verb, object, in that order, e.g.,

kháw	sýy'	nya'
He	buys	meat

ON HISTORICAL LINGUISTICS

In 1969 Haas wrote a book on the prehistory of languages in relation to general principles. This includes phonological types of change, morphological reconstruction, problems of classification, and diffusion. Included also are some tables for Wiyot-Yorok-Algonquian-Gulf (p. 62), proto-Muskogean (p. 42), Algonkian and Yurok cognates (p. 67), and pre-Muskogean and Tunica (pp. 63–64). As indicated above, Haas treats Tunica as an isolate without strongly provable relations to other languages, but she suggests that Tunica may be related to the pre-proto-Muskogean, based not on detailed lexical evidence, but (pp. 63–64) on some of its partially similar affix features.

ON LANGUAGE TEACHING AND LEARNING

In a manual published in 1945 (and reprinted in 1978), Haas and Subhanka wrote that “Prosecution of the war created the need for these materials to teach *spoken* language.” (The material was especially indebted to Henry Lee Smith, Jr., of the Language Section in the Education Branch and liaison with the Intensive Language Program, through J. Milton Cowan.) The sections include basic sentences, new words (and “how to take apart the words and phrases… and to make new words and phrases on the same model”), hints on pronunciation, and "a number of new ways of saying things." Part one (in book one) includes "Getting around," "Buying things," "Meeting people," "Family and friends," "What do you do for a living?" and 'Review." "Part two" includes "How do you like the weather?" "Getting a room in a hotel," "Getting dressed," "Let's go eat," "A shopping trip," and "Review." Part three (in Book Two): "On the train," "At the beach," "Let's go to the game," "Making a call," "At the play," and "Review." Part 4: "Getting a passport," "At the university," "Going to the doctor's," "The bank and the post office," "Home and neighbors," and "Review." Part Five: "Geography," "Agriculture," "Industry," "Government," "The country and its people," and "Review." The materials are on phonograph records for practice hearing.

In a more theoretical article Haas (1953) discusses the important relation of linguistics to language teaching. She mentions the work of Boas, Sapir, and Bloomfield, and how the earlier descriptive work is still needed, but it needs approaches to teaching applications (as Bloomfield tried to show). Reading is not enough. Memorization of paradigms is not enough; conversational teaching is needed. For many students beginning the study of a foreign language, these approaches should best precede detailed analytical work. She mentions also some materials of personal interest to me (e.g., the work of Charles C.Fries and the English Language Institute at the University of Michigan, where I worked for a time on the intonation of American English, which she also refers to).

In a short, excellent, and easy-to-understand earlier article (1943), Haas gives instructions to help the beginning student learn any language. The linguist can learn a language quickly (p. 202) by working with "a native speaker, whom he treats not as a teacher but purely as a source of information." But the linguist must also teach the student the techniques of eliciting information, and analyzing and organizing the speaker's data, including appropriate phonetics. And (p. 205) “many of the fundamental features of the [analytical] method were first developed in the study of American Indian languages, which often present unusually difficult phonetic and grammatical systems.” The students may either “participate with the linguist in the analysis of the language to be learned” or (p. 206) the linguist analyzes in advance and acts “as a model for imitation,” teaching “instead of only guiding.”

Haas's students (pp. 206–207) in Thai are “first of all taught to use a phonemic notation” so that “they may concentrate on the pronunciation from the very beginning,” and they can use it to “carry on all their work…. This practice reflects one of the basic assumptions of our method: SPEAKING MUST COME BEFORE READING.” About half of the student's time goes into “more traditional” work with “grammatical discussion, word study, translation of Thai into English and English into Thai,” and drill on “troublesome points of grammar.” The other half goes into drill “to develop good pronunciation and, later on, the ability to converse in Thai.” The drill consists of three kinds: (1) exercises of imitation to train students to imitate exactly “so that they may be understood”; (2) exercises of dictation so that students may write down what they hear, improve perception, and “record new words even before they have learned the traditional native alphabet”; and (3) exercises of recognition and response to “train them to understand and answer what they hear, so that they may gain experience in the actual use of the language as a means of social intercourse.”

The dictation exercises are “intended to improve the student's ability to hear, not his ability to spell.” And in early stages the informant dictates only words that the class have already studied; then "short sentences containing familiar words"; then "sentences containing new words"; and finally "whole passages with old and new words mingled." Later, the student learns how to correct mistakes "by comparing the troublesome feature of a new word with a similar feature of some word already known." And when the tone of a word is not clearly heard, it is studied by comparing it with a word "whose tone is known to him." He does the same (p. 208) for vowels, aspiration, etc. After the student "has learned several hundred words and has acquired reasonable facility in conversation through the use of the phonemic writing alone, then—but not until then—he begins to learn the traditional system of writing." Haas informs us (p. 208) that her experience tells her that students like to learn a foreign language this way. "It gives them a sense of reality and the assurance that they are actually on the way."

FOR THIS MEMOIR I have drawn heavily on Haas's curriculum vitae provided by the National Academy of Sciences, and I am grateful for a recent obituary by Golla, which includes a Haas bibliography of about 130 items (V.Golla. Mary R.Haas (obituary). *Language* 73(4):826–37).

SELECTED BIBLIOGRAPHY

1932 With M.Swadesh. A visit to the other world; a Nitinat text. *IJAL* 7:195–208.

1941 Tunica. In *Handbook of American Indian Languages,* vol. 4. New York: Augustin Publishers.

1943 The linguist as a teacher of languages. *Language* 19:203–208.

1950 Tunica texts. In *University of California Publications in Linguistics,* vol. 6. Los Angeles: University of California Press.

1953 The application of linguistics to language teaching. In *Anthropology Today,* ed. Kroeber, pp. 807–18. Chicago: University of Chicago Press.

1955 Thai vocabulary. In *Program in Oriental Languages,* A:2. Washington, D.C.: American Council of Learned Societies.

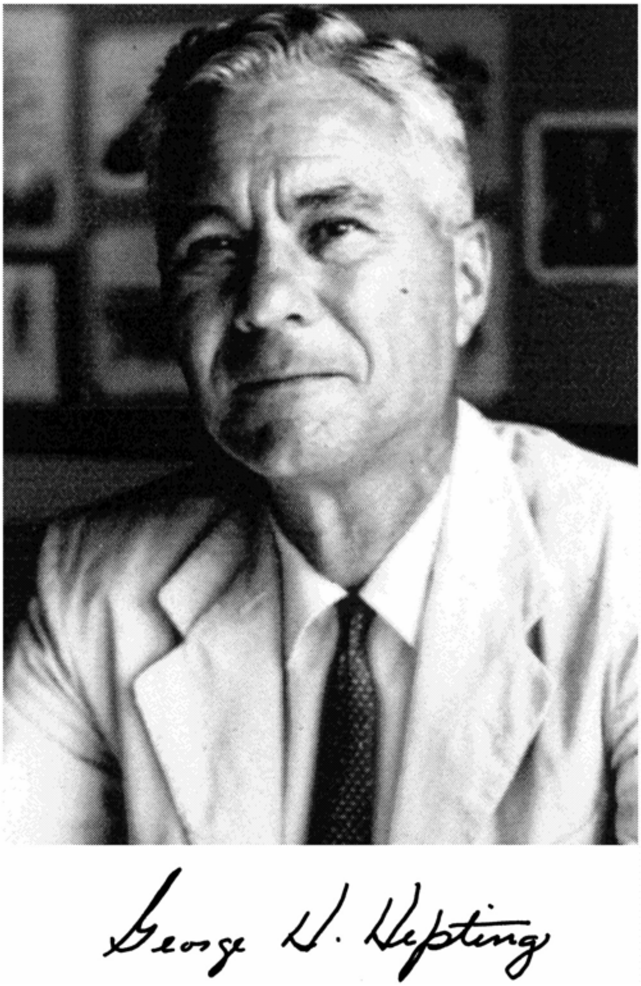
1958 The tones of four Tai dialects. *Bull. Inst. Hist. Philol.* 29:817–26.

1962 What belongs in a bilingual dictionary? In *Problems in Lexicography,* eds. F.W.Householder and S.Soporta. IJAL 28:45–50

1964 *Thai-English Student's Dictionary*. Stanford: Stanford University Press.

1969 *The Prehistory of Languages.* Paris: Mouton.

With H.R.Subhanka. *Spoken Thai,* books I and II. Ithaca, N.Y.: Spoken Language Services.



GEORGE HENRY HEPTING

September 1, 1907–April 29, 1988

BY ELLIS B.COWLING, ARTHUR KELMAN, AND HARRY R.POWERS, JR.

GEORGE HENRY HEPTING grew up in the city environment of Brooklyn, but early in his life he developed a deep love for and scientific interest in forestry. He became America's most skilled scientist in the theory and practice of forest pathology. He studied how long-lived forest trees, unlike most plants, cope with the long-term changes in their biological, physical, and chemical environments. He devoted his remarkably energetic life to learning, understanding, and teaching how trees survive disease stresses induced by biotic and abiotic agents in forest nurseries, as individual trees, in young sapling stands, in naturally regenerated and planted stands, in old-growth forests, and in landscapes and watersheds. Hepting focused his innovative spirit, curiosity, and high intelligence in seeking the ways to use this understanding to develop practical management practices that would reduce or minimize disease losses and deterioration of wood in service. From the research he and close coworkers completed have come many tangible benefits. Throughout his life, he was devoted to maintaining the rich heritage of this country's forests and wildlife resources in national, state, and city parks and trees in residential, commercial, and recreational landscapes—resources that are important not only economically but also for the spirit and aesthetic quality of life in America.

George Hepting was born in Brooklyn, New York, on September 1, 1907. After attending public schools in Brooklyn, he completed his undergraduate studies in forestry at Cornell University in 1929. One of Hepting's most inspiring undergraduate teachers was a plant pathologist, H.H. Whetzel. In an unpublished autobiography Hepting described how he decided to become a forest pathologist:

Two of my required courses were general plant pathology and forest pathology. Through these courses I came under the influence of Professor H. H.Whetzel. I soon found myself developing a strong interest in his courses. The ills of trees, like the ills of mankind, fascinated me. I was astounded to learn that in the short space of twenty-five years a disease (chestnut blight) carried from the Orient had started in our chestnut trees in New York and had swept completely through the eastern states to Alabama, on its way exterminating this valuable species. White pine blister rust, the Dutch elm disease, and many others were well on their way to destroying millions of dollars worth of timber and street trees in a matter of a few years. Here was something for me. Here was a field of work in which a man would work with trees. I discovered that the U.S. Department of Agriculture had a Division of Forest Pathology with about fifty technical men scattered the length and breadth of the country doing research on tree diseases, and that an occasional state or university had a man or two who spent some time on tree diseases. This handful of men armed with the limited knowledge of a new profession was trying to solve the multitude of problems in forest pathology. In addition to the hundreds of native diseases that did a tremendous amount of cumulative damage, serious new major diseases were appearing in the country at an alarming rate of about one every five years. I resolved to be a forest pathologist, if I could find a place in this tiny field. Professor Whetzel encouraged me and offered me valuable assistance. This time I knew the kind of work that I would be doing and that I would like it.

Prior to the completion of his Ph.D. at Cornell in 1933, Hepting joined a tiny cadre of scientists in the U.S. Department of Agriculture that was charged with protecting the forests of America against disease. He remained with the Division of Forest Disease Research after he received his doctorate on a forest pathology problem and rose through the ranks from field assistant in 1931, through chief of the Division of Forest Disease Research at the Southeastern Forest Experiment Station from 1953 to 1961, to principal research scientist affiliated with the Forest Service's Washington office from 1962 to 1971 (assigned mainly to the Southeastern Forest Experiment Station in Asheville, North Carolina). He retired from the Forest Service as chief plant pathologist in 1971. From 1967 through 1984 he served as a visiting professor in the Department of Plant Pathology and the School of Forest Resources at North Carolina State University. He died on April 20, 1988.

During much of his life he had to cope with a series of illnesses requiring surgery and hospital stays with continuing complex medical problems and twice daily medications that would have drastically limited the productivity of most individuals. Few people other than close colleagues and family members were ever cognizant of the full extent and seriousness of these problems. Because of Hepting's strong will and remarkable personal courage, he always remained fully committed to his profession and related responsibilities.

Although deeply involved in his professional life, Hepting greatly enjoyed one hobby, the selection and polishing of unique gemstones. As was typical of all his activities, he perfected his techniques and became very knowledgeable and recognized for his proficiency in this field.

In 1936 George Hepting married Anna Love, who predeceased him on May 13, 1986. Both are buried in the Lewis Memorial Cemetery in Asheville, North Carolina. Hepting is survived by six family members: his sister Aimee Hepting of Syosett, New York; sons George Carleton Hepting of New York City and John Bartram Hepting, who was named for John Bartram, a distinguished horticulturist and world-renowned botanist of Philadelphia and a direct ancestor of Anna Love Hepting.

The Department of Plant Pathology at North Carolina State University maintains a complete file of his nearly 200 scientific publications, his extensive library of nearly two thousand reprints and books, and copies of the fascinating and often humorous autobiographical resumé of the first half of his career.

The range of problems in which Hepting became involved represents a remarkable scope in terms of the diversity of fungi involved and complexity of the factors to be considered in developing effective means of reducing losses. Only a few examples of his many contributions will be noted in detail.

Hepting's first research project was on heart rot of forest trees. He determined the impact of fire scars, basal wounds, and stump sprouts on infection and spread of decay in many species of trees. He was the first to describe the remarkable mechanisms with which trees restrict the development of decay and discoloration in stems to “tissues extant at time of wounding.” This phenomenon is now known as compartmentalization. His work on the hardwood decays and their origins resulted in a Farmer's Bulletin that established a set of sound principles for effective disease management of eastern hardwood forests. This bulletin was used extensively by the Civilian Conservation Corps as a guideline for management of federal forests, and it still serves as a basic guide for foresters in management of hardwood forests.

Before and during World War II, he studied fungal discolorations in felled timber and lumber of southern pines. He also quantified the impact of discolorations and decay on the strength of wood veneers used in military aircraft. In his unpublished autobiography, he described his novel experiences in this new area of research:

We in forest pathology did not have to look for war problems; they fell into our laps from all directions. Wood was a major war material and we in the

Division of Forest Pathology had the greatest fund of information on wood's defects of any group in the nation. We knew about wood decay and how to prevent it.

Under his direction the tiny group of men and women in his division immediately shifted the emphasis of their work from tree disease investigations to studies of problems of wood in service. The Navy and Coast Guard wanted information on the prevention of decay in wooden boats and they also planned to build some wooden airplanes and gliders. The Army was already building all-wood training planes and was considering wood gliders. Furthermore, they had costly wood decay problems in buildings, truck bodies, and bridge timbers.

By the time of World War II, there was a critical shortage in aircraft metals. Most of the available light metals were to go into combat planes—bombers and fighters—so that the great bulk of thousands of training planes would have to be made of wood. Gliders, of which we were to require a great number, were also to be made largely of wood. Yellow poplar, one of the most important aircraft veneer species, is subject to many discolorations in the living tree. Early in the war, most of this colored poplar wood was being discarded from the aircraft grades on suspicion that it was weak. Nobody knew for sure whether or not it was weak, but the manufacturers did not trust it, and the Army did not like the looks of it. I was asked to undertake a study to determine whether the discolorations so common in yellow poplar really indicated decreased strength of wood. I immediately went to several veneer mills and obtained hundreds of samples, including all of the common discolorations and normal-colored wood as well. We carefully matched each discolored stick with an adjacent normal-colored stick and sent the samples to the Forest Products Laboratory for testing. When the results were analyzed, we found that the great bulk of discolored wood was normal in strength, and that only browns, indicating rot, were weak. These results were released promptly to the veneer industry, the aircraft industry, and the Army. The harmless discolorations were then accepted. The production of poplar aircraft veneer went up 25%.

Wooden gliders were being turned out in quantity. Since our training fields each had from one hundred to several hundred aircraft, space to house these great numbers of airplanes could not easily be provided. Therefore, they generally remained in the open all of the time, exposed to the elements. Since most of the kinds of wood used in aircraft were known to decay readily under conditions of high moisture and warmth, and since there was no tendency among manufacturers to treat this wood chemically against decay, it seemed to us that some serious decay problems might develop in our Army airplanes. In December of 1942 I asked my chief, if he would let me go into the field and study the problem of deterioration in wooden military airplanes and gliders. He agreed that sooner or later the armed services would run into trouble from decay in aircraft, so he assigned me to this work and ordered me to report to the Army Air Forces Materiel Command at Wright Field, to make arrangements for my surveys at Army fields.

Hepting visited dozens of Army airfields in the East, South, and Middle West, checking the all-wood airplanes and wooden parts of others for signs of decay. Subsequently Hepting and his group discovered a number of factors leading to decay problems and developed procedures to eliminate or reduce them. Technical orders were issued to improve stringency of inspection and cleaning of drains in all wood aircraft. Hundreds of planes were grounded for repairs, and the prospects of serious accidents were considerably reduced. Revised specifications were made for airplane manufacturers on improving the design of drainage systems. Thus, through the efforts of Hepting and his research group, the deterioration problems in wood aircraft were corrected early in the war. The importance of this major contribution to one phase of the war effort has not been fully recognized. Upon completion of this excursion into problems of deterioration and decay of wood products in the armed services, Hepting and his staff devoted their research to a series of problems affecting forest trees in the south.

Littleleaf disease of southern pines proved to be one of his greatest challenges. He organized research teams to investigate different aspects of the problem and stimulated both industry and government to support these efforts. In the end, he determined that the little-leaf disease resulted from a progressive deficiency of nitrogen induced by a complex interaction among certain soil conditions, feeder-root pathogens, land-use practices, and stand density that developed in many shortleaf pine stands as the trees increased with age.

A destructive wilt disease of mimosa began to cause high mortality in this species in North Carolina in the late 1930s. In Hepting's investigation of the problem he identified the causal fungus as a previously undescribed species of *Fusarium;* his report on these studies was one of the first descriptions of a tree disease caused by a species in this taxonomic group. In the several decades that followed it was not possible to develop a means of preventing the spread of this pathogen, and the disease essentially eliminated mimosa from the District of Columbia to Alabama. In recognition that the only effective means of control was the development of resistant cultivars, Hepting and Richard Toole screened thousands of mimosa genotypes after World War II and discovered a number of highly resistant selections. From these selections the cultivars “Charlotte” and “Tryon” were developed and patented. As required by law, the patent was assigned to the Secretary of Agriculture, who released it to the nursery trade through the American Nurserymen's Association. Several decades after the release of these cultivars, they were still being widely planted, and they continue to be resistant to this day.

Hepting and coworkers discovered a number of previously undescribed diseases that were damaging southern tree species, including the pitch-canker disease of southern pines, and identified the specific causal fungi. Subsequently they found that pine trees inoculated artificially with the pitch-canker fungus were stimulated to induce oleoresin flow with desirable results. This procedure was patented and used commercially.

When the oak wilt disease began to spread in the southern United States, Hepting assumed a leadership role in a national effort to gain an understanding of the biology of the pathogen and manner of dissemination. He designed and supervised large-scale surveys to determine the extent of spread into Tennessee and western North Carolina. During these efforts he discovered the role of mating types in the life history of the oak wilt fungus, a finding that he considered one of his most personally satisfying scientific achievements.

In mid-career Hepting had a key role in resolving a controversy involving the use of antibiotics for control of white pine blister rust. In the late 1950s a U.S. Forest Service technician, employed in the white pine blister-rust control project in Idaho, published a series of papers in which claims were made that an antibiotic (Actidione BR) sprayed as a basal application on blister-rust cankers followed by a second antibiotic (Phytoactin) could effectively prevent development of the rust fungus. Hepting became very skeptical of these findings. Studies were initiated by members of his staff (Harry Powers and others) to determine independently the effectiveness of these compounds on white pines. In sharp contrast to the findings in Idaho, Powers' results indicated that application of antibiotics might reduce sporulation of the rust fungus, but they did not eradicate the rust fungus in established infections. Actidione also was tested in the Southeast for the control of fusiform rust, a destructive disease of southern pines similar to white pine blister rust in its effects on pine trees. Results obtained in these studies were also negative.

In 1960 Hepting and a group of Forest Service administrators and one non-Forest Service pathologist, Arthur

Kelman, then on the faculty of North Carolina State University, visited the established test plots in Idaho and Washington, including areas where Phytoactin had been applied by costly aerial sprays. In the course of this survey, it became clear that the prior claims for effectiveness of antibiotic sprays could not be substantiated. In some plots evidence was found that a hyperparasite of the rust fungus *(Tuberculina maxima)* had become established in rust cankers and suppressed the growth of the rust fungus. Apparently the effects of the hyperparasite on the rust fungus in the cankers had been overlooked and mistakenly attributed to the presumed effects of the antibiotic. Based on these findings and Hepting's insistence on the need for the effective experimental controls and proper design and interpretation of results, a major costly federal program was discontinued, resulting in savings of many millions of dollars. It should be noted that initially Hepting was severely criticized for raising questions about this rust control program, which was widely praised as an innovative control measure designed to save the highly valuable white pine stands of the western United States. However, Hepting had the courage to persevere until the evidence was obtained to justify fully his conclusions that data on control lacked validity.

In his administrative role Hepting directed pioneering research on annosus root rot, soil fumigations in forest nurseries, and the role of ozone and other photochemical oxidants as causes of disease in forests. His 1963 paper on climate and forest diseases is considered the authoritative treatise on climatology and plant pathology. He developed the first computerized system for information retrieval in forestry and his 1971 text, “Diseases of Forest and Shade Trees in the United States,” provides the most comprehensive encyclopedia of knowledge on these topics. He wrote a definitive history of the failure of efforts to control chest nut blight and of similar attempts to control the Dutch elm disease after these diseases were introduced into North America. In 1997, six years after retirement, he wrote with E.B.Cowling an historical resume of achievements and future progress in forest disease research. This publication also describes the impact of Hepting's contributions on the advancement of forest pathology, nationally and internationally.

On periodic visits to the campus while he held the post of visiting professor of forest pathology and forestry at North Carolina State University, he presented seminars and consulted with graduate students and faculty. In these sessions he provided encouragement, made critical assessments of research in progress, and served as a wise mentor and valuable source of knowledge. Hepting considered this phase of his career one of the most rewarding experiences of his professional life. In the evaluations by graduate students of their contacts with Hepting, they ranked their exposure to his wisdom and sharp wit as one of the highlights of their graduate education.

He was a cofounder of the Southwide Forest Disease Workshop, which is still the outstanding forum for forest pathologists in this region. It provided for the first time an opportunity for government, university and private industry research scientists, and related workers, as well as graduate students, to share information on research in progress and to develop the personal relationships that foster progress in cooperative research programs. His leadership in this and related activities resulted not only in strengthening forest disease research in the U.S. Forest Service but also in the universities in the southern United States. The conference indirectly had a role in the establishment and funding of industry-sponsored graduate fellowships in forest entomology and pathology. Hepting also had an influential role in the increased participation of forest pathologists in international forestry policy discussions and in the activities of the American Phytopathological Society, including the establishment of the subject matter committee on forest pathology. He was an associate editor of *Phytopathology* and for a number of years was a member of the Editorial Board of *Annual Review of Phytopathology*. He also served on several committees of the National Academy of Sciences and edited the National Research Council text entitled “Principles of Plant Disease Control.”

Hepting's achievements in forest pathology were recognized by many honors and awards. In 1969 he became the first forester elected to the National Academy of Sciences. He also received the Superior Service Award of the U.S. Department of Agriculture (1954) and the Barrington Moore Award for Outstanding Achievements in Forestry Research (1963). He was elected a fellow of the Society of American Foresters (1965) and of the American Phytopathological Society (1966). He received the first Southern Forest Pathologist Achievement Award (1967), the U.S. Department of Agriculture Merit Award for Achievement in Cost Reduction for development of an effective electronic literature retrieval system for forest pathology (1967), the Delta Airlines “Flying Colonel” Award for Service to Aviation (1972), the International Shade Tree Conference “Authors Citation Award” for his handbook on “Diseases of Forest and Shade Trees in the United States (1974), and the Weyerhaeuser Award for Outstanding Historical Writing from the Forest History Society (1974).

In the course of his career Hepting traveled extensively and completed research assignments in Europe, Puerto Rico, Haiti, and the U.S. Virgin Islands. He also served as a consultant to the forest products industries of New Zealand and Australia.

Few investigators in the forest sciences were able in a lifetime to make as many major contributions as Hepting did in solving diverse, complex problems. He had the ability to identify primary causal factors and rapidly gain the depth of understanding of disease situations that enabled him to devise practical approaches for management practices. Long before the concepts of integrated pest management became fashionable, Hepting emphasized the need to integrate disease hazard evaluations and knowledge of disease development processes into economically and biologically sound forest management systems. He also championed the need for basic research as a foundation for practical understanding and management of disease in forests. His role in the Timber Resources Review of 1953 also permanently changed our perception of the nature and magnitude of disease losses in forests.

Hepting was not only an effective leader in terms of his specific administrative assignments, but he was also an effective spokesperson for forest pathology and forestry in the United States. At the peak of his career he also became a recognized and influential international authority on forestry in the broad sense. In making an assessment of his career he stated:

It seems to me that there can be few walks of life in which a man following a specific occupation would lead a more varied existence than he would as a forest disease researcher. Within this seemingly restricted field, I have, over a period of 20 years been a rock breaker, a timber cruiser, a bacteriologist, an aircraft technologist, a lumberjack, a pathologist, a statistician, and an administrator. My territory has, from time to time, included much of our forestland from the Canadian border to the Gulf of Mexico and west to Texas and the Great Plains.

He had a remarkable ability to stimulate and challenge coworkers and professional colleagues to do their best, to see the larger picture, to share their ideas with others, and to help "make forest pathology pay." He was also willing to speak frankly and critically when he thought the occasion demanded. In this connection he insisted that his associates maintain the same high standards of scientific integrity and quality that he always demanded for himself in his own individual research endeavors. For these and other personal qualities, he earned the high regard and deep respect of his coworkers and members of his profession.

WE ACKNOWLEDGE WITH appreciation receipt of comments and letters from colleagues and former students who knew, appreciated, and were inspired by George Hepting. In particular we wish to thank the following individuals who shared their impressions of Dr. Hepting with us: Andrew Campbell, Alex Shigo, T.Kent Kirk, Robert Zabel, Arthur Verrall, Charles Berry, William Waters, Glenn Snow, Robert Patton, James Stewart, John Skelly, John Rishbeth, Kathleen Moore, and Arthur Schipper.

SELECTED BIBLIOGRAPHY

1935 Decay following fire in young Mississippi Delta hardwoods. U.S. Department of Agriculture Technical Bulletin, no. 494.

1936 With D.J.Blaisdell. A protective zone in red gum fire scars. *Phytopathology* 26:62–67.

1937 With G.G.Hedgcock. Decay in merchantable oak, yellow poplar, and basswood in the Appalachian region. U.S. Department of Agriculture Technical Bulletin, no. 570.

1938 With A.D.Chapman. Losses from heart rot in two short-leaf and loblolly pine stands. *J. For.* 36:1193–1201.

1939 A vascular wilt of the mimosa tree *(Albizzia julibrissin)*. U.S. Department of Agriculture Circular, no. 535.

1942 With E.R.Roth and R.F.Luxford. The significance of the discolorations in aircraft veneers: Yellow poplar. U.S. Department of Agriculture mimeo publication, no. 1375.

Reducing losses from tree diseases in eastern forests and farm woodlands. U.S. Department of Agriculture Farmers' Bulletin, no. 1887.

1943 With E.R.Roth. Origin and development of oak stump sprouts as affecting their likelihood to decay. *J. For.* 41:27–36.

1944 With A.A.Downs. Root and butt rot in planted white pine at Biltmore, North Carolina. *J. For.* 42:119–23.

1945 Reserve food storage in short-leaf pine in relation to little-leaf disease. *Phytopathology* 35:106–19.

With T.S.Buchanan and L.W.R.Jackson. Littleleaf disease of pine. U.S. Department of Agriculture Circular, no. 716.

1946 With E.R.Roth. Pitch canker, a new disease of some southern pines. *J. For.* 44:742–44.

1947 Stimulation of oleoresin flow in pines by a fungus. *Science* 105:209.

1948 With E.R.Roth and E.R.Toole. Nutritional aspects of the littleleaf disease of pine. *J. For.* 46:578–87.

1949 With E.R.Toole. Selection and propagation of *Albizzia* for resistance to Fusarium wilt. *Phytopathology* 39:63–70.

With G.M.Jemison. Timber stand improvement in the southern Appalachian region. U.S. Department of Agriculture Miscellaneous Publication, no. 693.

1952 With E.R.Toole and J.S.Boyce, Jr. Sexuality in the oak wilt fungus. *Phytopathology* 42:438–42.

1953 With W.A.Campbell and T.L.Copeland. Managing short-leaf pine in littleleaf disease areas. Southeastern Forest Experiment Station Paper, no. 25.

1955 The current status of oak wilt in the United States. *For. Sci.* 1:95– 103.

1963 Climate and forest diseases. *Annu. Rev. Phytopathol.* 1:31–50.

1964 Damage to forests from air pollution. *J. For.* 62:630–34.

1965 The INTREDIS register for world literature in forest pathology. In 1964 FAO/ICFRO Symposium on Internationally Dangerous Forest Diseases and Insects 2:1–8.

1968 Diseases of forest and tree crops caused by air pollutants. *Phytopathology* 58:1098–1101.

1974 Death of the American chestnut. *J. For. Hist.* 18:60–67

1977 With E.B.Cowling. Forest Pathology: Unique features and prospects. *Annu. Rev. Phytopathol.* 15:431–50.

Photo by Bachrach

Edwin C. Kemble

EDWIN C.KEMBLE

January 28, 1889–March 12, 1984

BY ALEXI ASSMUS

UNUSUAL AMONG PHYSICISTS but in consonance with his religious views, Edwin Crawford Kemble approached his career with humility. He spoke of his own research on molecular quantum physics depreciatingly, was reticent in accepting its importance for the growth of the American quantum physics community, and made little of his lifelong devotion to teaching. Perhaps we can regard his career more dispassionately, neither with embarrassment nor with a memoirist's false grandiosity.

Edwin C.Kemble began his college career at Ohio-Wesleyan University in 1906, but stayed there only a year before transferring to the Case School of Applied Science from which he received his B.S. in physics in 1911. He began graduate school at Harvard University in 1913 and completed his Ph.D. in physics in 1917. After a short time doing war work and a half semester teaching physics at Williams College, Kemble returned to Harvard in 1919 as an assistant professor in the physics department. He remained there the rest of his career, and was made chairman of the department in 1940. He spent a Guggenheim fellowship year in Europe in 1927–28. In 1925 Kemble married Harriet Mary Tindle. The couple had two children, Robert and Jean. Two years be fore their fiftieth wedding anniversary, Harriet died. In 1978 Kemble married Martha Chadbourne Kettelle, his Radcliffe fiancée from graduate student days.

As a graduate student Kemble made an exciting and courageous move into quantum theory and in 1919 Percy Bridgman, his thesis advisor, convinced him to accept the job of building up theoretical research in the Harvard physics department. Not only did Kemble introduce a theoretical sophistication at the university, but he also focused attention on quantum physics, a subject that generally had been ignored, both at Harvard and in the United States as a whole. In his first decade at Harvard, Kemble played a crucial role in the creation of a national research program in the application of quantum concepts to molecular structure and dynamics. In this endeavor, Kemble worked closely with young colleagues and graduate students. In later years he would turn his attention to college undergraduate and high school education.

The orientation towards community that was evident in Kemble's career reflected his upbringing in the home of Duston and Margaret (Day) Kemble, former Methodist missionaries. Kemble was born in 1889 in Delaware, Ohio. Like many of his colleagues, he was raised in a midwestern religious household that maintained an admiration for science, rather than an antagonism towards it. In fact, he described his minister father as to “some degree, an inventor.”[1] He began his college studies at Ohio-Wesleyan in preparation for missionary duties (1906–1907), but between his brother's urgings and his own inclinations he decided to transfer to the Case School of Applied Science and to follow in the footsteps of his engineer brother. After a summer spent working in his brother's business, the Case Machine Company, which had produced one of Minister Kemble's inventions, Kemble changed his mind once again and began a scientific career. This choice was not in conflict with his family's or with his own religious views. For Kemble, as for many other physicists of his generation, religion and science did mix. Religion brought to science a dedication to include others in a community that believed in a higher truth.

Case, the site of Kemble's first scientific education, was founded in 1880 as an engineering school in industrial Cleveland. By the time Kemble attended Case it had developed strengths in science. Dayton C.Miller, a nationally recognized scientist worked there in the physics of acoustics, but because many students at Case wanted to be physicists, Miller had only one or two students a year. Kemble was one of the few. While working on his undergraduate thesis project with Miller, Kemble burst into a week of productive, frenzied work, which, he told historian Thomas Kuhn fifty years later, “left one with a vivid sense of the way…mental activity propagates itself.”[2]

Kemble graduated from Case in 1911 and spent the following year as a physics instructor at the Carnegie Institute of Technology in Pittsburgh, a school founded, as was Case, in response to the growing demand for higher education for technologists. During that year, Miller obtained a graduate fellowship for Kemble at Harvard—a fellowship personally financed by Harvard Professor Wallace Sabine, a colleague of Miller's in acoustics. In 1913 Kemble came to Harvard as a graduate student.

At the time the physics department at Harvard was hospitable neither to the new quantum physics making its appearance on the Continent nor to a practice of physics that included theorists as well as experimentalists. (Theoretical physics had made its appearance in Europe thirty years prior.) It was not that Americans completely ignored quantum physics. Planck's blackbody radiation law was well known and an

American, Robert A.Millikan, was the one to put Einstein's photoelectric equation to an experimental test. (He expected to prove it wrong!) Physicists in the United States were primarily interested in experimental matters and had not confronted critically the quantum theory as a whole. Kemble was formally introduced to the new theory in G.W.Pierce's course on radiation, but the professor had much to say against it. Kemble, on the other hand, was drawn to the new ideas. “Everything with a quantum in it, with ‘h' in it, was exciting.”[3] His early enthusiasm took the form of two graduate “theses,” so-called papers required for graduate courses at Harvard. They were on an area of physics where quantum ideas were coming into conflict with older principles. The theses were on the problem of specific heats of solids and on the statement of the equipartition theorem. While considering dissertation topics, Kemble jumped at the ideas introduced in a talk by fellow student James B. Brinsmade on the recently introduced quantum theory of molecular spectra.

In the usual accounts of the history of physics little has been said about the unraveling of molecular structure, a feat accomplished by the study of molecular spectra. The focus had been on atomic structure, because it was in this area that the most interesting and foundational questions of quantum theory were addressed in the period 1916–25. During this time Niels Bohr became a central figure in the development of the new theory. Historians of modern physics have emphasized his work, especially his papers of 1913, which predicted accurately the spectra of atomic hydrogen. Yet, in 1913 and for several years after, Bohr's work was not part of the mainstream effort to develop a quantum theory. In fact, atomic structure and atomic spectra were hardly considered in the years between 1900 and 1916; instead, the focus was on the quantum behavior of collective sys tems (blackbody radiation and specific heats). The particular mechanical systems that were quantized—the oscillator and the rotator—were basic to molecular structure.

At the Solvay conference in 1911 the question of how to quantize the rotator was discussed thoroughly. In the laboratory of the organizer of the conference, Walther Nernst, work was being done on predicting the spectra of molecular gases, particularly HCl. A young Danish researcher, Niels Bjerrum, took the model of a "quantized" rotator and used it to predict accurately what is now called the vibrational-rotational spectra of molecules. Bjerrum made the analysis independently of and slightly prior to Bohr's application of quantized motion to atomic spectra.

It was to Bjerrum's theory of molecular spectra that Kemble turned as a graduate student. Kemble, so interested in "everything with a quantum in it" had found a problem. He wrote in his first paper: "The explanation of the structure of infrared bands of gases given by Bjerrum has led to striking direct confirmation of the quantum theory in the form first proposed by Planck (assuming absorption as well as emission by quanta), and gives to the study of these bands a large significance for the further development of the theory."[4] Kemble took Bjerrum's model of a molecule as a simple vibrating quantum rotator and modified it to include an-harmonic vibrations and interactions between vibrations and rotations. Bjerrum's formula for the spectral lines of molecular bands was $v_t=v_o\pm v_r$ where v_o is the vibrational frequency and v_r the rotational frequency quantized to give $v_r=nh/2\pi^2J$ (J the moment of inertia). (Bjerrum applied the traditional electrodynamic identification of radiation with mechanical frequencies.) With his inclusion of nonlinear terms Kemble obtained to second order $v=(v_o-a/\ v_r^2)\pm v_r$, the adjustment coming in a decrease in the vibrational frequency as the rotator speeded up, pulled apart, and sampled the non-linear range of the force holding the two atoms together.

Percy Bridgman supervised Kemble's work as a graduate student. Harvard's well-known experimentalist championed the cause of a young graduate student who wanted to do theory. Even though Bridgman could not help with the quantum theory, he did provide Kemble with a philosophy for doing physics, which Kemble described later as “heaven sent.” Inspired by Einstein's definitions of space and time, Bridgman came to believe that all concepts in physics must be definable in terms of measurable quantities. To define a concept meant to explain, at least in principle, how to measure it. He argued that concepts not definable in operational terms were meaningless.[5] Kemble embraced Bridgman's operationalism, as it came to be called, and made it central to his own understanding of quantum theory. Even though Bridgman's operationalism provided Kemble with a philosophy of quantum mechanics, Bridgman himself never felt comfortable with (nor did he ever accept) quantum mechanics.

Kemble was given permission to do a theoretical thesis (one of the first presented in this country), but only after his advisor managed to convince other members of the department of its value. A compromise was agreed upon; Kemble must have an experimental section, too. Kemble collaborated with Brinsmade, the fellow graduate student who had introduced him to Bjerrum's theory, to obtain beautiful molecular spectra, which confirmed Kemble's postulated anharmonicity of vibrational motion.

A short piece on Kemble in McGraw-Hill's Men of Science series sharply criticizes Kemble for his equating of radiation frequencies with mechanical frequencies and his ignorance of Bohr's new frequency condition that gives radiation frequencies as differences in energy (rather than as a function of mechanical motion).[6] The absence of Bohr's theory from Kemble's work sheds light on history, however, and should not lead to the conclusion that the young American was ignorant. When Kemble was working on his graduate thesis, Bohr's frequency condition did not apply to molecular dynamics; it was clear from Bohr's papers of 1913 that the condition applied only to electronic motion and not to the rotation and vibration of molecules. Kemble made no mistake in ignoring it. The straightforwardness and success of Bjerrum's more semi-classical approach, which equated radiation frequencies with mechanical ones, delayed the application of Bohr's frequency condition to the infrared spectra of molecules. In fact, Bohr's frequency condition led to difficulties. Why were so many frequencies forbidden? Partly due to this difficulty it was not until 1919 that a unified explanation of frequencies would apply to molecular and atomic spectra.

When Kemble graduated from Harvard in June of 1917 the country was at war. Kemble felt it his duty to develop airplane engines at Curtiss Aircraft Company, which he did until he was laid off precipitously as the war neared its end. Although Harvard wanted him back as a faculty member (in fact, the department had never wanted him to leave), a position could not be found immediately, and Kemble taught at Williams College for half a semester. When Harvard did make Kemble an offer, he was shocked at the low salary and the low status of the position he thought that implied. Kemble told the department that he would have to support his parents in the future and reminded them somewhat cryptically of the “shipwreck of an engagement” he had suffered in the past. (After his first wife died Kemble married his fiancée from his graduate student years.)

In a long letter designed to lure Kemble to Harvard, his old advisor Bridgman explained his plans to build up theory at Harvard and to support its growth across the country. Kemble's coming to the university was crucial to the plan. Bridgman outlined a restructured curriculum that had Kemble teaching four upper-level courses (two of them graduate): radiation theory; quantum theory of the infrared, photo-electricity, and specific heats; X-ray crystal structure; and a special topics course in theory. Previously the Harvard department, like others in the country, had focused on electromagnetism (e.g., radiotelegraphy, optics, and wave propagation). More than three-quarters of the physics classes given in 1919 fell under this rubric. Now Bridgman envisioned a move away from this concentration, and he wanted his former graduate student's help.

I am really enthusiastic about this scheme of courses. It comes pretty close to what I have been wanting for a long time. If we can get the courses well given, it ought to put Harvard pretty near the top in this country. What is more, it is a good beginning to putting the country on the map in theoretical physics. Course 22 [the special topics course] is designed especially for this, and would nominally be taken only by those students specializing in theoretical physics, of whom we shall hope for an increasing number. But you see that you are an essential part of this program. Don't you want to be a member of a Department that is trying to do this, and don't you feel the challenge in this?[7]

Kemble accepted the challenge. Establishing theoretical physics at Harvard and taking the department to the top was a heavy responsibility for a young man. Kemble started immediately. His first year at Harvard he taught one of the earliest courses in quantum theory given in the country. His approach to the subject was taken from Bridgman and exemplified the American approach to theory.

It seems to me essential that we approach the subject in a proper frame of mind. The quantum theory is an attempt to correlate and ultimately to give a partial explanation of a series of startling facts which are in apparent conflict with the laws of classical mechanics and classical electrodynamics. I say that it is an attempt to give a *{partial}* explanation of these facts because in the last analysis the physicists seek merely to formulate a few fundamental equations from which the behavior of matter may be predicted and into whose origin we will hardly inquire…. In such a subject as this we must not look for rigorous logical deductions and we must not make too much of the paradoxes which come up from time to time. The theory is simply justified by (a) the nature of the phenomena it is designed to explain, (b) the results already obtained in the shape of formulae which stand the test of quantitative comparison with the results of experiment, and (c) the gradual clarification of the fundamental ideas on which it rests.[8]

Kemble's first graduate student was John Van Vleck, and many followed in the next fifteen years (e.g., Clarence Zener, James H.Bartlett, Eugene Feenberg, and J.L.Dunham). Although Van Vleck and Kemble worked on the crossed-orbit model of the helium atom, most of Kemble's students used the quantum theory to shed light on molecular structure. In fact, this was generally true of the emerging quantum physics community in the United States during the twenties; the focus was on molecular structure, not, as in Europe, on atomic structure.

At this time there was a fine spectroscopic tradition in the country. Harrison Randall headed a major infrared spectroscopy laboratory at the University of Michigan. At the end of the nineteenth century, Ernst Fox Nichols at Cornell had developed the residual ray technique to isolate hard-to-detect infrared radiation, and his student William W.Coblentz had invented and improved instruments to detect infrared frequencies. Coblentz's three-volume work *Investigations of Infrared Spectra* became the reference work for molecular spectra, as had Heinrich Kayser and Carl Runge's for atomic spectra. The molecular dynamics of rotation and vibration generate spectra in the infrared. Electron motions in molecules and atoms generally produce spectra in the optical and higher frequencies.

The national origins of these two compendia (one Ameri can, the other German) point to the research focus each country took during the 1920s. While the Germans and other Europeans focused on atomic structure in their quest for the foundation of quantum theory, the Americans achieved maturity as physicists by studying the quantum nature of molecular structure. They shunned the waters of atomic physics and thus avoided competing with those whom Raymond T.Birge, molecular spectroscopist at Berkeley and Kemble's close correspondent, called the “atomic structure sharks.”

Kemble was at the center of the research program in molecular structure. Having introduced the quantum problem to the United States, he went on to chair the National Research Council's Committee on Radiation in Gases, which during its three-year-long preparation (1923–26) of a book-length report *Molecular Spectra in Gases,* served as the coordinating group for a national research program. Kemble represented Harvard and the east; Randall's group at Michigan was represented on the committee by Walter F.Colby, and Raymond T.Birge spoke for the west from his position as a skilled molecular spectroscopist at Berkeley. A postdoctoral fellow at Harvard, Robert S.Mulliken, played a large role in the research and writing of the report, although he was not an official member of the committee.

A crucial ingredient for the growth and success of the research program in molecular structure were the post-doctoral fellows, like Mulliken. Funded postdoctoral research and education was set up after World War I by the Rockefeller Foundation and the National Research Council. These two institutions chose to support physics and chemistry by creating a number of non-teaching, one- to two-year research positions for young Ph.D.s. The existence of these research positions, intermediate between professor and graduate stu dents, marked the beginning of the modern scientific research group.

One of the first such research groups was the one that surrounded Kemble at Harvard from 1923 to 1927. Mulliken arrived at Harvard in 1923 and in the following years was joined by three other postdoctoral fellows. The group worked to understand fluorescent band spectra, the Zeeman effect, and the vibrational-rotational bands that appear in the electronic spectra of molecules. Mulliken became known for his untangling of molecular isotopic effects.

The years 1923–26 were a fertile period for the understanding of molecular structure. Because the older quantum theory gave essentially the same energies for the rotator and oscillator as did the soon-to-come quantum mechanics, the conclusions reached about dynamical structure were to remain valid across the great divide of 1926 (the invention of quantum mechanics). The success of the molecular program pre-1926 moved Kemble to introduce the National Research Council's report with: “Although the theory of quanta has marvelously illuminated all branches of physics connected in any intimate way with atomic and molecular processes, few subjects have become more strikingly clarified than that of band (molecular) spectra.”[9]

The stability of molecules remained an insoluble problem in the context of the older quantum theory, however. The solution of the binding problem for the hydrogen molecule by Heitler and London in 1927, usually marks the beginning of quantum chemistry, but the discipline's roots go farther back. The education of American quantum physicists in the early twenties through the study of molecular structure set the stage for an American-dominated discipline of quantum chemistry in the late twenties and thirties; in this Kemble played a key role.

Right at the heyday of excitement over the discovery of quantum mechanics, in 1927–28, Kemble spent a Guggenheim fellowship year in Europe, mainly at Göttingen and Munich. Here Kemble made what he later called the worst policy decision of his life: to finish up an older quantum theoretical calculation for band spectra rather than throw himself wholeheartedly into learning the new theory. To friends in the United States he wrote that he could not make heads or tails of von Neumann's first lectures on quantum mechanics (and he mentioned that neither could Max Born). In the next decade, Kemble was to more than make up for his initial neglect of the theory.

On his return to the United States, Kemble wrote with E. V.Hill two long review articles on quantum theory for the first issues of *Reviews of Modern Physics*. The articles were the first published exposition of the new theory in the United States. Kemble continued to work on understanding the basis of the theory, considering the meaning of probability in the quantum case and the relation between the wave functions and the physical states of the system. Kemble's efforts to secure a mathematical foundation for quantum mechanics culminated in his textbook *Fundamental Principles of Quantum Mechanics* (1937), a book so detailed and mathematical in its attempt to ground quantum mechanics operationally that it was little used as a textbook. Kemble openly attributed his approach to Bridgman's. Foundational concepts should be based on explicitly measurable properties, not on intuitive ideas or metaphysical comforts.

The care and consideration Kemble brought to his understanding of quantum mechanics—in many ways a mea culpa for his earlier decision to disregard the theory in 1927—was antithetical to a pursuit of his own research in molecular structure. In 1969 in a short autobiographical sketch, he wrote, “I am proud of them [the papers and the book on the foundations of quantum mechanics] and too deeply interested in questions of clarity in the organization of knowledge to wish that I had taken a different course in 1929. But I did pay a high price for my interest in philosophy."[10]

With World War II came another shift in Kemble's career. Many of his colleagues worked for the duration of the war at MIT's Radiation Laboratory. Kemble, who chaired the physics department from 1940–1045, supervised the teaching of basic physics to military officers. He consulted for the Navy's underwater sound laboratory during the war and in 1945 was part of the overseas ALSOS mission, whose top-secret job was to uncover German atomic bomb research.

Kemble enjoyed and was intrigued by his wartime task of explaining physics to non-physicists. At war's end, he had a chance to continue this work. Reacting to the great role science played in the war, James B.Conant, president of Harvard, high-level administrator in the bomb project, and chemist, proposed to teach science to all Harvard undergraduates by teaching them the history of science. Conant hoped to highlight the importance of science for social change. Kemble enthusiastically joined the general education project, and a lunchtime group was set up in the physics department to try to enact the ambitious plan. (It included Kemble, I.Bernard Cohen, Gerald Holton, Thomas S.Kuhn, Philippe Le Corbeiller, and Leonard K.Nash.)

As part of the general education program, Kemble taught a course in the physical sciences to non-science majors. The cartons of student papers he kept attest to his love of the job and his belief that writing the history of science could stimulate the imagination of those who would have to manage what he called the "issues of the day,…war and peace, racial injustice, overpopulation, automation, the pollution and contamination of the atmosphere and water supply [and] the breakdown of traditional values."[11] During the fifties, Kemble worked on restructuring the curriculum for physics majors as well. His major contribution was to chair a committee that forwarded recommendations for a revision of standard electromagnetism courses given at the college level.

Kemble's concern about the conditions of modern society was integral to his political and personal life as well as to his teaching. He protested security restrictions in the National Science Foundation bill of 1950, encouraged scientists to join the Federation of American Scientists during the Cold War, and played a role in the peace movement as part of a Methodist congregation.

Kemble retired from Harvard in 1957, having spent all but three years there since the time he entered graduate school. For three years after retirement, he was director of Harvard's Academic Year Institute, where high-school teachers could study with university professors. The beneficiaries of Kemble's teaching were many: young postdoctoral researchers, graduate students, undergraduates (both scientists and non-scientists), and finally high school teachers (and indirectly their students). He served his scientific community in official capacities as chairman of the Physics Section of the National Academy of Sciences (1945–48) and as a member of the Executive Committee of the National Research Council's Division of Physical Sciences.

Kemble was embarrassed and always apologetic about his scientific output. “As you see, my career has not been one of great distinction,” he wrote.[12] The feeling was intensified by the high-caliber students he saw blossoming under him, physicists like John Van Vleck, Robert S.Mulliken, John C. Slater, and J.Robert Oppenheimer. After his wartime teaching experience, Kemble made a decision: “I saw myself spending the rest of my life panting to try to keep within hailing distance of what was going on. I deliberately quit being a scientist at that time although I continued to teach."[13]

Looking back at Kemble's entire career allows us to take a broader perspective than Kemble himself and recognize his value as a community builder, a task so in concert with his religious beliefs. Kemble's most important contributions to research were introducing the study of a quantum molecular structure to the United States and presiding over the budding research community that worked on the problem. Americans learned quantum physics by studying molecules. There is good reason to believe that this is why quantum chemistry was predominantly an American discipline when it emerged in the late twenties. It is foolish to attribute such large-scale developments to any one person, but it is reasonable to claim someone a place as one of perhaps several motivating forces. I believe that such a place belongs to Kemble.

Edwin Crawford Kemble died on March 12, 1984.

NOTES

1. E.Kemble interview with T.Kuhn, October 1, 1963. Archives for the History of Quantum Physics.

2. Ibid.

3. E.Kemble interview with T.Kuhn, May 11, 1962. Archives for the History of Quantum Physics.

4. E.Kemble. The distribution of angular velocities among diatomic gas. *Phys. Rev.* 8(1916):689.

5. P.Bridgman. *The Logic of Modern Physics*. New York: Macmillan, 1927.

6. *Modern Men of Science,* vol. 2, pp. 285–86. New York: McGraw-Hill, 1968.

7. P.Bridgman to E.Kemble. Lyman correspondence, March 16, 1919, box 8, folder K-1919, Harvard University Archives.

8. E.Kemble lecture notes for physics 16a, 1919–1920, box 18. Harvard University Archives.

9. E.Kemble et al. Molecular spectra in gases. In "Report on the

Committee on Radiation in Gases." Bull. no. 57, p. 9. Washington, D.C.: National Research Council, 1926.

10. E.Kemble to S.S.Ballard, December 20, 1969. Harvard University Archives.

11. E.C.Kemble. *Physical Science, Its Structure and Development,* p. 14. Cambridge, Mass.: MIT Press, 1966.

12. E.Kemble to S.Ballard, op. cit.

13. E.Kemble interview with T.Kuhn, October 1, 1963. Archives for the History of Quantum Physics.

SELECTED BIBLIOGRAPHY

1916 Note on the end effect in the electrostriction of cylindrical condensers. *Phys. Rev.* 7:614–24.

The distribution angular velocities among diatomic gas molecules. *Phys. Rev.* 8:689–700.

On the occurrence of harmonics in the infra-red absorption spectra of gases. *Phys. Rev.* 8:701–14.

1921 The probable normal state of the helium atom. *Phil. Mag.* 42:123– 33.

1923 With J.H.Van Vleck. On the theory of the temperature variation of the specific heat of hydrogen. *Phys. Rev.* 21:655–61.

1925 The application of the correspondence principle to degenerate systems and the relative intensities of band lines. *Phys. Rev.* 25:1–22.

1926 Molecular spectra in gases. Bull. no. 57. Washington, D.C.: National Research Council.

1927 With R.S.Mulliken. Zeeman effect in the Angstrom CO bands. *Phys. Rev.* 30:439–57.

The rotational distortion of multiplet electronic states in band spectra. *Phys. Rev.* 30:387–99.

1929 With E.L.Hill. On the Raman effect in gases. *Proc. Natl. Acad. Sci. U.S.A.* 15:387–92.

With E.L.Hill. General Principles of quantum mechanics. Part I. *Phys. Rev.* 1(suppl.):157–215.

1930 With E.L.Hill. General principles of quantum mechanics. Part II. *Rev. Mod. Phys.* 2:1–59.

With F.F.Rieke. The interaction between excited and unexcited hydrogen atoms at large distances. *Phys. Rev.* 36:153–54.

1935 The intensities of the vibration-rotation bands of HCl. *J. Chem. Phys.* 3:316–17.

The correlation of wave functions with the states of physical systems. *Phys. Rev.* 47:973–74.

1937 *The Fundamental Principles of Quantum Mechanics*. New York: McGraw-Hill.

1938 Operational reasoning, reality, and quantum mechanics. *J. Franklin Inst.* 225:263–75.

1939 Fluctuations, thermodynamic equilibrium and entropy. *Phys. Rev.* 48:549–61.

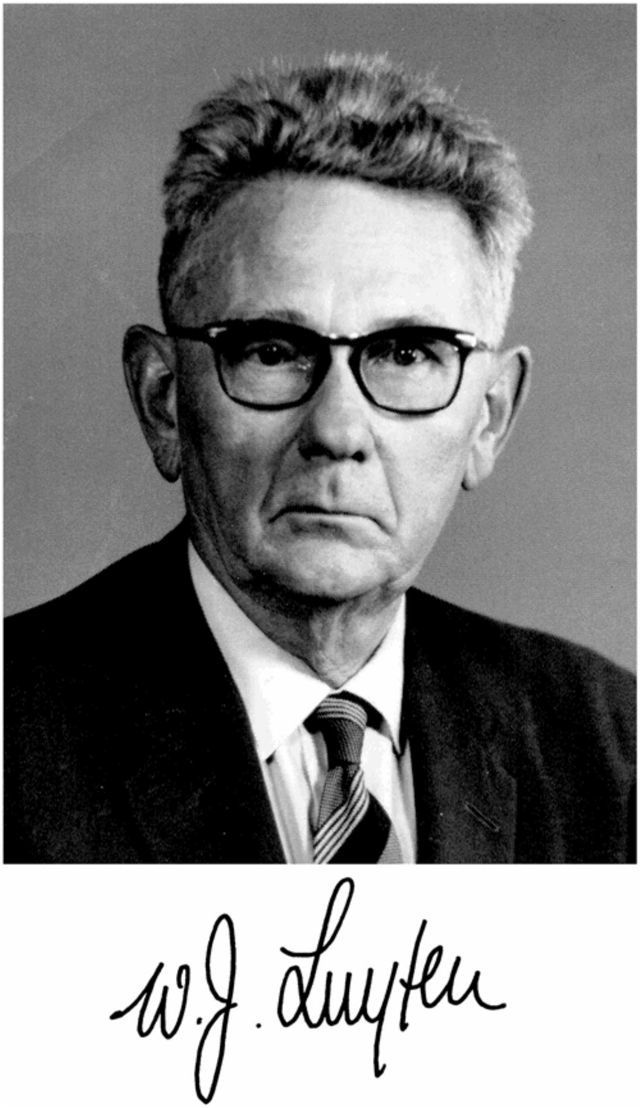
The quantum-mechanical basis of statistical mechanics. *Phys. Rev.* 56:1146–64.

1941 The probability concept. *Phil. Sci.* 8:204–32.

1950 With others. The teaching of electricity and magnetism at the college level. I. Logical standards and critical issues. II. Two outlines for college teachers. *Am. J. Phys.* 18:1–25, 69–88.

1951 Reality, measurement, and the state of the system in quantum mechanics. *Phil. Sci.* 18:273–99.

1954 Scientists and political action. *Sci. Mon.* 78:138–41.
1966 *Physical Science, Its Structure and Development.* Cambridge, Mass.: MIT Press.



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WILLEM JACOB LUYTEN

March 7, 1899–November 21, 1994

BY ARTHUR UPGREN

WILLEM LUYTEN WAS the central figure in the determination of the stellar luminosity function, the frequency function of stars by their luminosity. In this, his major research contribution, he followed in the tradition of Dutch astronomers, mostly of the Leiden Observatory, which began before 1900 with J.C.Kapteyn and included P.J.van Rhijn, Ejnar Hertzsprung, Willem De Sitter, and Jan H.Oort. Luyten was one of a number of distinguished students of these scientists who emigrated to the United States and had a memorable career. His contemporaries included Bart J. Bok, Dirk Brouwer, Gerard P.Kuiper, Jan Schilt, Kaj Aa. Strand, and Peter van de Kamp.

Luyten spoke of his ancestry as part French, originating in Provence in the fourteenth century. The family name may have been Lutin and derived from lute players and minstrels attending the popes who resided in Avignon. In 1377 the popes moved back to Rome and the progenitor lute player resettled at the Court of Burgundy. The dukes there united the cities of Holland, and during ducal rule over the next century, the family may have found its way to the Netherlands. His mother's family name of Francken reveals her origin there.

Luyten, himself, was born of parents of North Holland, who had settled in Indonesia, then a colony of the Netherlands. His birth on March 7, 1899, was in the city of Semarang in north-central Java, where his father taught French in the local high school. Luyten lived there until 1912, when the family moved back to the Netherlands. At that time he spoke Dutch and French; he also became fluent in German and English before his high school graduation. Later in college he mastered Latin and Greek, and still later, he picked up some Spanish and Italian, and finally, in 1927, Russian. He was rightfully proud of his ability to learn to read and speak so many languages.

Willem Luyten's interest in astronomy dates from the 1910 appearance of Halley's comet over his home in Semarang. He made his first astronomical observations on Java in 1912, and continued them while a student at the University of Amsterdam, where he received a B.A. degree in 1918. His earliest research was published at that time and he completed his doctoral thesis four years later at the University of Leiden, where he was awarded his Ph.D. degree in 1921. He was Hertzsprung's first student there. Luyten's thesis was based on 13,500 visual observations of variable stars, some of which he made in high school and others with the 6-inch refractor at the Leiden Observatory. His contacts at Leiden included Kapteyn, de Sitter, and Paul Ehrenfest, at whose home he socialized on occasion with Albert Einstein, Hendrik Lorentz, and A.S.Eddington.

Although he became interested in many lines of astronomical research, Luyten's lifelong interest centered on the properties of the common nearby stars and especially their proper motions. Near the end of his career, he participated in International Astronomical Union Colloquium No. 97 on wide components in double and multiple stars held in 1987 in Brussels, which was dedicated to him. There he gave a review of his lifetime of research on these objects. In it he remarked that:

We should remember that,…of the 6,000 stars [that] the average human eye could see in the entire sky, probably not more than thirty—or one-half of one percent—are less luminous than the Sun; that probably, of the 700-odd stars nearer than ten parsecs, at least 96% are less luminous than the Sun. There is not even ONE real yellow giant—such as Capella, Pollux, or Arcturus—nearer than ten parsecs and only about four Main-Sequence A stars.

He was always aware of the havoc this great dichotomy between the brightest and the nearest stars—fraught as it is with bias—could wreak upon anyone who did not take full account of it in their work.

Perhaps no one explored the immensity of this dichotomy in more detail than did Luyten. He turned his early interest in proper motions into a better calibration of the HR diagram than had been known at the time. His early years at the Lick Observatory and as a guest investigator at the Royal Greenwich Observatory witnessed his development and application of techniques using proper motions to estimate the distances of stars in large numbers. Through the use of Hertzsprung's concept of the reduced proper motion to obtain statistical parallaxes for common stars, he was the first to provide a realistic census of stars in the solar neighborhood and an HR diagram more truly representative of the fainter stars that dominate the solar neighborhood.

The reduced proper motion connects the apparent and absolute magnitudes (luminosities) with proper motion in much the same way as are the apparent and absolute magnitudes with trigonometric parallax. Just as the parallax fixes the absolute magnitude exactly, so do proper motions roughly determine it. Roughly, because proper motions of stars at a given distance differ considerably. But, if many stars are examined and the mean proper motion is calibrated on parallax, the method works.

It is worth noting that, not long ago, the only properties known about the majority of the nearest stellar neighbors were the apparent magnitude and the proper motion. In fact, the proper motion became the feature by which a faint nearby star could be recognized as such. In his autobiography published in 1987, Luyten cites his seventy years of work on this subject. His amazingly extensive and pioneering efforts in this domain dwarf those of anyone else. Since 1925 he determined over 200,000 proper motions, itself a testimonial to his stamina and dedication. In 1925 Luyten lost the sight of one eye in a tennis accident. Thus, he accomplished all of this with his remaining eye; it is probable that he has blinked, observed, and measured more stellar images than anyone else.

The preceding feat alone would merit a permanent place in the annals of astronomy, but his insight into the worth of the collected data lies even more at the center of his achievement. His Dutch predecessors—especially Kapteyn, van Rhijn, and his Danish mentor at Leiden, Ejnar Hertzsprung—picked up about where Sir William Herschel left off a century earlier in the study of the stellar makeup of the Milky Way. The luminosity function concept was well known by the time Luyten entered the scene, but it was he, working almost alone, who first filled in its faint end.

In 1923, after two years at the Lick Observatory, Luyten was offered a position at the Harvard College Observatory by Harlow Shapley. He spent the next seven years on its staff, the last two in Bloemfontein, South Africa. At both Lick and Harvard, Luyten was engaged in a number of other research subjects. While at Lick, he predicted and confirmed that the sodium D lines differ widely in intensity among the cooler stars, between giants and normal dwarfs of the same surface temperature. But his Harvard years became dominated by the study of proper motions that formed the major focus of research for the rest of his professional life.

At Harvard and Bloemfontein, he began his long association with the 0.6-meter Bruce refracting telescope. Between 1896 and 1910 at its former location at Arequipa, Peru, the telescope had been used to photograph almost the entire southern celestial hemisphere in three-hour exposures that reached the seventeenth magnitude. Altogether, the collection comprised more than 1,000 plates. These plates could serve as first-epoch observations for a large proper motion survey, and in 1927, with the aid of a Guggenheim Fellowship, the Bruce Proper Motion Survey began. Luyten took over 300 of the 1,000 plates forming the second-epoch material and blinked all of the plate pairs. Altogether 94,263 stars with significant proper motions were found. Most of these stars were brighter than magnitude 14.5 and had proper motions in excess of one-tenth of an arc second per year. The measurement of positions and proper motions for these stars took many years to acquire, and required a number of measurers, including myself during my undergraduate days at Minnesota. The final catalog appeared in 1963.

In compiling this catalog, Luyten showed much resource-fulness. In 1923 he published a paper in which he employed a cumulative probability plot, or probit plot, decades before its common use in astronomy. These plots outlined a technique for determining whether specific sets of data follow a Gaussian distribution by rendering the cumulative normal distribution into linear form. The test is often more robust than the Kolmogorov-Smirnov and similar goodness-of-fit tests for randomness.

In funding such a long-term project, he was creative and persistent; at different periods he acknowledged not only the National Science Foundation and the Office of Naval

Research but also other federal relief organizations, such as the federal student aid program and even the Works Progress Administration, along with a number of private philanthropic sources.

The Bruce Proper Motion Survey led to improvements in stellar kinematics at the faint end of the luminosity function, but it also provided a rich harvest of degenerate stars, known also as white dwarfs. These are end products of stellar evolution with degenerate matter in their interiors after the fusion process has compacted their atomic nuclei and compressed them into planet-size objects. One of the goals of the survey was to discover and identify many degenerate or white dwarf stars. Only three were known in 1921, when Luyten began his term at Lick, far too few to support the many theoretical studies made of them then and since. Luyten collaborated with E.F.Carpenter of the University of Arizona, E.Gaviola of the Cordoba Observatory, and G.Haro of the Tonantzintla Observatory to obtain colors of the faint proper motion stars found in the survey. From the colors, magnitudes, and assumed distances, the degenerates were identified as such and, by the time of its publication in 1963, Luyten had discovered the great majority of the several hundred then known.

With the completion of the Bruce survey project, Luyten sought to extend its achievements in the search for stellar neighbors, to fainter magnitudes, and to the northern celestial hemisphere, which was not observable with the Bruce telescope in its southerly locations. For these reasons, he initiated the immense project known as the National Geographic/Palomar Observatory survey. The name honors the principal sponsor and the 1.2–1.8-meter Palomar Schmidt telescope on which much of the plate material had already been obtained. This wide-field instrument had photographed the entire sky north of declination −34° and to stars of magnitude 18 and fainter.

This is the plate material that formed the Palomar Observatory Sky Survey of the 1950s and is still very useful today. It also provided an ideal first epoch for the measure of proper motions. Luyten quickly realized that the old blink machine at Minnesota, on which measures for the Bruce survey were made by hand, was much too slow for this project. He approached the Control Data Corporation with plans to build a rapid-scanning microdensitometer. The CDC machine, designed primarily by James Newcomb and Anton LaBonte, became the fastest of the new generation of automatic machines capable of measuring and blinking stellar images with high precision. It finally became possible to determine the proper motions of hundreds of thousands of stars in a short time; in a few years motions for 300,000 stars were found, doubling the number with these data. The catalogues that emerged from this effort are among the most widely used in the field. They include the first round of catalogues of 1955 to 1961, the LFT (Luyten-Five-Tenths) catalogue of 1,849 stars, and the LTT (Luyten Two-Tenths) catalogue of 16,994 stars with proper motions exceeding 0.″5 and 0.″2 arc seconds per year, respectively. Twenty years later, well after his retirement, he published their successors, the LHS (Luyten Half Second) and NLTT (New Luyten Two-Tenths) catalogues with the same limits, but with 3,583 and 58,700 stars.

Honors accrued to Luyten at about the time of his retirement in 1967; he was the Catherine Wolfe Bruce medalist of the Astronomical Society of the Pacific in 1968, and was elected to the National Academy of Sciences in 1970. Also in 1970 he received an honorary doctorate degree from St. Andrew's University, the oldest educational institution in Scotland; only Benjamin Franklin and two others preceded him in the award of this honor. He organized and headed the first conference held specifically on proper motions. The meeting was held at the Control Data Corporation in Minneapolis in April 1970, and the proceedings constituted the International Astronomical Union Colloquium No. 7.

However one obtains a value for the stellar luminosity function, one must calibrate the data for the many thousands of stars covered in the survey against a much smaller group of stars for which the individual luminosities are directly determined from the parallax. Until the present decade, these were few and were biased in one way or another. For his calibration sample Luyten used 610 stars with proper motions in excess of 0.5 arc seconds per year, and for which luminosities were available from trigonometric parallaxes.

In 1964 James Wanner completed a doctoral thesis at Harvard University on the same subject but with a different calibration group. Wanner used a limit in distance instead of proper motion as his major criterion. He used only stars within ten parsecs of the Sun—117 altogether—which also fulfilled secondary criteria in parallax and proper motion. Wanner's technique has the advantage of being far less susceptible to a bias towards stars with a high velocity across the sky. Both Luyten and Wanner used Hertzsprung's approach, but with proper motions being such a fundamental parameter in Luyten's work, a high-velocity bias is apparent in the result. Wanner's function comes closer to recent determinations that can to a large extent bypass proper motion and thus better represent all stars in this part of the galaxy.

This controversy became a matter of great contention, until settled by access to very large stellar samples with distances determined for each star individually. Luyten's function was vitiated only among the very faintest of stars;

unfortunately, these are the ones most critical to the study of certain aspects of stellar evolution and of other planetary systems. They are also among the hardest to model, to assign with confidence the interior domains of radiative and convective energy transfer by which the energy produced at the stellar core rises to the surface and out into space. In any event, the merit of his work is beyond reproach when we consider the data and methods available to him at the time.

Willem Luyten joined the faculty of the University of Minnesota in 1931, his appointment at Harvard having been terminated the previous autumn, apparently without cause. In his autobiography, Luyten contends that Henry Norris Russell, then the putative "dean of American astronomers," was instrumental in the termination. He describes their first encounter: Luyten had compared stellar luminosities from Mount Wilson spectral classifications and from parallaxes and had concluded that, if all M giants were assigned the same luminosity, the mean error in luminosity from parallax would be reduced. Upon seeing this work, Russell, according to Luyten, said, "Even if this were true, I could say it, but you can't." Young Luyten responded, "I thought that in science the only thing that mattered was what was said not who said it." These and further encounters allegedly turned the influential Russell against him.

Over his career, Luyten published some 500 research papers and wrote numerous popular articles for the *New York Times, Minneapolis Star and Tribune,* and other periodicals. His association with the *Times* began in 1925 with his report on the total solar eclipse of that year as seen from the air. He credits its editors with his success in obtaining the Minnesota position after a long job search.

At Minnesota, where a single astronomer was then in fashion, he succeeded the binary star astronomer and observer

Francis P.Leavenworth, who retired and died in 1928. At some time during the three-year interim, the observatory and its 0.25-meter refracting telescope were moved to the top of the then new physics building, a questionable improvement in location. Neither astronomer had a role in this decision. While a student there, I discovered that the coordinates of the old site had continued to be propagated in the literature. Luyten concurred, but he may not have corrected the error in the American Ephemeris and elsewhere, where the old coordinates were listed until at least 1980.

I knew his work habits well. He used a blink machine to align two plates taken years apart to discover the stars that moved noticeably, and were therefore likely to be nearby neighbors of the Sun. This was exhausting work, and none of the rest of us could stand to do it for long. With his one good eye, he could blink for hours at a time; his perseverance seemed limitless. The rest of us measured the locations of each moving star and several of its neighbors for positions and entered them in notebooks. In that computerless era, we needed to combine the two motion components along each of the two orthogonal axes, into a total motion and direction. From repeated use, I came to know the squares of all integers from 1 to 100 from memory. Luyten was a master in teaching students to make offhand estimates, always a difficult point to get across. For example, he encouraged the memorization of the logarithms of 2, 3, and 7. From these, one can quickly derive the logarithms of any integer up to ten and can interpolate larger ones closely.

At the completion of the information on motions in each field, he would assign magnitudes to the stars that had moved. Having none of the photometric equipment of today, he would, with an eyepiece in hand, call out the magnitudes to be recorded. He claimed that a certain image size was set at magnitude 12.7, as I recall, and he went on from there. He was well aware that emulsion and other differences produced a considerable magnitude error of as much as a full magnitude. On this he would cite a rule common to astronomers of his generation and all but forgotten since, that the systematic errors could be assumed to be about one-fifth of the accidental errors from all sources. (I heard this same remark from his contemporaries Bart Bok and Peter van de Kamp as well.)

Until his retirement in 1967, he regularly taught introductory astronomy, as well as some advanced courses, at the university. His enthusiasm extended to every corner of astronomy, as was evident in lectures and in conversation; I for one learned very much from him, inside and outside the classroom. He was a superb teacher, and he regaled the students with stories that revealed a delightful sense of humor. After getting off a bon mot, he retained his typical saturnine facial expression, but the twinkle in his eyes was noted by many.

His strained and sometimes hostile approach to some of his colleagues and the public in general never extended to students, as I well know. Typical of his gruff public manner was an item appearing in a column by “Mr. Fixit” in the *Minneapolis Tribune* in 1956. A woman had written for the identity of a brilliant star appearing in the sky. She cited her neighbor as an authority on astronomy who had never beheld such a spectacle before. “I referred your query to Prof. Willem J.Luyten, chairman of the University of Minnesota Department of Astronomy,” Mr. Fixit replied. “His comment: ‘If you removed the drama and hooey, the planet Venus is left.'”

Yet, it was clear that he knew the place and value of humor in his lectures and other remarks. In response to a student in my introductory course with him, who was hav ing trouble visualizing a galaxy, he remarked that a galaxy looks like a cow pie. Typical of his humorous gruffness was his response to a persistent telephone. He finally interrupted a lecture in my celestial mechanics class to answer it. After a minute, he returned and grumbled, "Some SOB has a piece of shiny steel he thinks is a meteorite." Amidst riotous laughter, he resumed his lecture. His fluency in English was assured if a bit florid. This is evident in his popular book on astronomy *The Pageant of the Stars,* first published in 1929, with a second edition appearing five years later.

Willem Luyten became a factor in my own enthusiasm for astronomy more than once. It was he who, in the spring of 1940, pointed out to me the five naked-eye planets strung along the ecliptic in the western sky at dusk. Later that year he invited me to see Jupiter and Saturn through the refractor. Yet, ten years afterward, when I matriculated at the university, I still had no thought of astronomy as a profession, and I took up engineering instead. After three years of a mediocre record based squarely on a lack of interest, I considered astronomy as a career. When I approached him about a change of careers, he promptly said, "You get the hell out of engineering and into astronomy, where you belong." I have never regretted taking his advice.

Later, after my graduate work was completed, I fell afoul of his wrath more than once. At issue was a group of seven F-type stars near the North Galactic Pole that simple Poisson statistics strongly suggested must be physically associated. From spectroscopic and photometric evidence they appeared to form a small cluster of the old disk population, similar in age to the well-known clusters M 67 and NGC 188. Later known as Upgren 1, this is the fourth or fifth nearest cluster to the Solar System.

The evidence for physical association from proper motions was marginal, with 3 to 5 of the 7 stars showing paral lel motion. Luyten's complaint was that proper motion information should be paramount in the recognition of a group of stars as a cluster. He published a partial refutation centered on this point, and never again discussed it, and soon turned his attention to correct the perceived mistakes of others. Recent radial velocities confirm five stars as members, though no longer gravitationally bound together.

More than once in his writings, Luyten quoted Lord Peter Wimsey, Dorothy Sayers's fictional detective, who remarked in *Gaudy Night* that “the point about it is that the only ethical principle which has made science possible is that the truth shall be told all the time. If we do not penalize false statements made in error, we open up the way for false statements made by intention.” This comment became his touchstone for professional behavior, and in his own way he applied it relentlessly to himself and to his colleagues. Coupled with an intransigent approach towards the proprietary rights of one who first studies a star or group of stars, it led to repeated admonishments on his part of a number of distinguished colleagues in and out of astronomy. Such actions resulted in embittered relations and even total alienation between him and some of them. Most took it in stride or responded in kind. But the potential for harm to the career of a younger astronomer was not always negligible.

Luyten had a talent for alliterative broadsides in his publications. Some of his feistiest papers bore such titles and references to colleagues as “The Messiahs of the Missing Mass,” “More Bedtime Stories from Lick,” and “The Weistrop Watergate.” They made for very amusing reading, but they were too disrespectful and too full of negative allusions to his colleagues and their work to be at all times in the best interest of science, even though much in them was factually correct. In his later years, he referred to himself as a curmudgeon, an epithet bestowed on him at times by others.

In this, too, a certain modicum of humor crept into his otherwise stern bearing. Although we met on several occasions since then, he last spoke to me at the general assembly of the International Astronomical Union in Patras, Greece, in 1982.

While living and working in South Africa, Willem Luyten met and married Willemina Miedema; it was a close marriage and lasted over sixty years until his death on November 21, 1994. The Luytens had three children, all among my neighborhood childhood acquaintances. Mona Coatzee is now on the faculty of the University of Pittsburgh, Ann Dieperink was a Fulbright scholar and is a practicing attorney, and James Luyten earned a Ph.D. degree in physics at Harvard and is now an oceanographer at the Woods Hole Oceanographic Institution in Falmouth, Massachusetts. All three are married to professional people and have families of their own.

In 1939, about when I first knew him, he and Mrs. Luyten built a house only a block from my own, not far from the university campus in Minneapolis. It was the only one of an art deco style—ultramodern for the time—in a neighborhood of gables, dormers, and pitched roofs. Although it appears conventional today, it is almost as conspicuously different from its neighbors as is Frank Lloyd Wright's Guggenheim Museum in New York City. In his home, as in so much of his life, he was a nonconformist among non-conformists. He lived in that house for the remainder of his life and died there over half a century later. In home and family life, he led a remarkably stable existence. He was a man of many interests in addition to astronomy. His well-known knowledge of wines, especially those of Burgundy, was occasioned by many annual visits to that region of France for tasting and other celebration.

Willem Luyten maintained his research activity during the years after his retirement. He remained steadfast to his principles, but principle is best tempered at times with compassion and forgiveness. This he too seldom realized in the course of his relations with other astronomers. Yet, however he may come to be judged by those who knew him, he remains almost universally respected as the great imaginative and dedicated scholar and scientist he was. They are likely to agree with Shakespeare that “he was a man, take him for all in all, I shall not look upon his like again.”

MY PRIMARY SOURCE for this memoir was Willem Luyten's own autobiography (1987). Secondary sources were a paper by Helmut Abt in *Publ. Astron. Soc. Pac.* (80[1968]:247–251) written upon Luyten's award of the Bruce Medal, and obituaries by Dorrit Hoffleit in *J. Am. Assoc. Variable Star Obs.* (24 [1996]:43–49) and by myself in *Publ. Astron. Soc. Pac.* (107[1995]:603–605) and *Q. J. Roy. Astron. Soc.* (37[1996]: 453–456). In addition, I relied on many memories I have of Willem and his family over nearly five decades and some correspondence with him. I have included only the anecdotes that I witnessed or verified from independent evidence.

SELECTED BIBLIOGRAPHY

1922 Observations of variable stars. *Ann. Obs. Leiden* 13:1–64.

On the relation of mean parallax to proper motion, apparent magnitude, and spectrum. *Lick Obs. Bull.* 336:135–40.

1923 On the form of the distribution law of stellar velocities. *Proc. Natl. Acad. Sci. U.S.A.* 9:181.

A study of the nearby stars. *Harv. Obs. Ann.* 85:73–115.

Note on the possible relation between the intensity of the sodium lines and absolute magnitude. *Publ. Astron. Soc. Pac.* 35:175.

On the mean absolute magnitudes of the K and M giants and the synthetic errors in trogonometric parallaxes. *Proc. Natl. Acad. Sci. U.S.A.* 9:317–23.

1925 With E.B.Wilson. The population of New York City and its environs. *Proc. Natl. Acad. Sci. U.S.A.* 11:137.

1926 The properties of stars in the solar neighborhood. *Sci. Mon.* 32:494.

1930 On the systematic and accidental errors of modern trigonometric parallaxes. *Proc. Natl. Acad. Sci. U.S.A.* 16:464.

1934 Report on the state of the Bruce Proper Motion Survey. *Publ. Astron. Soc. Pac.* 46:194.

1938 On the distribution of absolute magnitudes in the vicinity of the Sun. *Mon. Not. Roy. Astron. Soc.* 98:677.

1942 On the origin of the Solar System. *Astrophys. J.* 96:482.

1945 A proposal for the classification of white dwarf spectra. *Astrophys. J.* 101:131.
1952 The spectra and luminosities of white dwarfs. *Astrophys. J.* 116:283.
1955 *A Catalogue of 1849 Stars With Motions Exceeding 0″.5 Annually.* Minneapolis: Lund Press.
1956 White dwarfs and degenerate stars. *Vistas in Astronomy,* p. 1048.
1957 *A Catalogue of 9867 Stars in the Southern Hemisphere With Motions Larger Than 0″.2.* Minneapolis: Lund Press.
1958 The Hyades: A search for faint blue stars. *Faint Blue Stars* X.
1961 *A Catalogue of 7127 Stars in the Northern Hemisphere With Motions Larger Than 0″.2.* Minneapolis: Lund Press.
1963 Bruce Proper Motion Survey General Catalogue: The Motions of 94,000 Stars.
Proper Motion Survey With the 48-Inch Schmidt Telescope. I. Organization and Purpose. *Proper Motion Survey I.*
1965 The luminosities of faint blue stars. In *Proceedings of the First Conference on Faint Blue Stars,* ed. pp. 66–72. Saint Paul: Hill Foundation.
1967 A comparison between the Bruce, Palomar Schmidt, and Lowell proper motions. *Pub. Minn.* 3:20.

1971 Performance of an automated computerized plate scanner. *Proc. Natl. Acad. Sci. U.S.A.* 68:513.
1974 The Weistrop Watergate. *Proper Motion Survey* XXXVIII.
1976 On the alleged plethora of nearby M dwarfs with little or no proper motion. *Proper Motion Survey* XLVI.
LHS catalogue: Proper motions for 3583 stars larger than 0".5 annually. *Univ. Minn. Publ.*
1980 NLTT catalogue: Proper motions larger than 0".18 annually for 58,700 stars.
1981 More bedtime stories from Lick. *Proper Motion Survey* LVI.
1986 Data and proper motions for 250,000 faint stars on magnetic tape.
1987 *My First 72 Years of Astronomical Research: Reminiscences of an Astronomical Curmudgeon, Revealing the Presence of Human Nature in Science.* Minneapolis: W.J.Luyten.

Courtesy of the City College of New York

R. E. Marshak

ROBERT EUGENE MARSHAK

October 11, 1916–December 23, 1992

BY ERNEST M.HENLEY AND HARRY LUSTIG

ROBERT MARSHAK WAS AN extraordinarily imaginative and productive physicist. After making important contributions to astrophysics, he turned to nuclear and elementary particle physics as his primary area of research. His and Hans Bethe's two-meson hypothesis and the proposal with George Sudarshan of the universal V—A weak interaction were milestones in the history of twentieth-century physics. Marshak was one of the great research guides of our time; his students and junior colleagues occupy important positions all over the world. Never a loner or one to limit his horizons, he became a leading statesman of world science and contributed enormously to strengthening communications and cooperation among scientists across borders and consequently to world peace and well-being. Throughout his life Marshak was driven not only by intellectual curiosity and brilliance, as well as a desire for personal recognition, but also by an unquenchable quest for social justice. This led him to make many contributions to the public good, most notably as president of the City College of New York during a period of wrenching change and renewal for that institution. He was a born leader and a practical dreamer whose work will live on for many years to come.

THE EARLY YEARS

Robert E.Marshak was born in 1916 in the Bronx, a borough of New York City, to poor immigrants from Minsk, Russia (now Belorus). In America his mother Rose became a seamstress and his father Harry worked as a garment cutter and seller of fruits and vegetables from a horse-drawn cart. Marshak's ability and ambition were recognized early and were strongly supported by his parents. (In later years Bob often was moved to tell the story of his father getting up at four in the morning to shine Bob's shoes, advising the son that his time was better spent in study than in cleaning shoes.)

He graduated from James Monroe High School at age fifteen, having won virtually every prize offered by the New York school system and having captained the school's math team to citywide victory. Like so many talented but poor New Yorkers, Marshak enrolled in the academically rigorous, tuition-free College of the City of New York (CCNY). After one semester he received a Pulitzer scholarship that provided full tuition and a stipend for study at Columbia University. Initially he majored in philosophy and mathematics and served as dance critic for the school newspaper. His first published article, in *Columbia Magazine,* was a critique of the dancer Martha Graham. (Bob Marshak maintained a love for and a commitment to the arts and humanities throughout his life. Almost five decades after Columbia, one of us [H.L.] attended a concert with him at CCNY's newly inaugurated Davis Center for the Performing Arts, in whose creation Bob had played a leading role. He told of his discovery of Schubert fairly late in life and he appeared moved to tears during the performance of that composer's Octet.) In his senior year Marshak switched to physics and came into contact with I.I.Rabi. Although

Rabi originally was skeptical of Marshak's commitment to physics, he later became a friend.

Marshak graduated from Columbia in 1936 and went on to graduate school at Cornell. There he studied with Hans Bethe, who at the time was working on problems pertaining to energy production in stars, work that later won him a Nobel Prize. Marshak wrote his dissertation on energy production in white dwarf stars, completing his Ph.D. degree in 1939 at the age of twenty-two. He concluded that white dwarfs could not contain hydrogen in their interior because it would immediately burn up at the high temperature. This conclusion is considered basic by astrophysicists who are expert on white dwarfs, and was confirmed by observation over the following half century. Never one to miss an opportunity, he persuaded Bethe to submit Bethe's paper on the carbon cycle as a source of stellar energy to the New York Academy of Sciences for the A.Cressy Morrison Prize. Bethe won that prize and gave Marshak a 10% finder's fee. When Marshak finished his thesis, it was also submitted to the Academy, and Marshak won the Morrison prize. With the money from it, he was able to buy his first car.

Jobs were hard to come by in the 1930s, especially for Jewish scientists. Marshak was nevertheless able to get a one-year position at the University of Rochester. It was “definitely for only one year,” because it had been promised to another man who had gone off for advanced study with a famous physicist. But that other man did not return; so this tenure track position wisely was given to Marshak. At Rochester he met and worked with Victor Weisskopf. He remained at Rochester, with time off for the war effort and during later leaves, until 1970. In 1943 Marshak married Ruth Gup, a schoolteacher in Rochester. In 1950 they had a daughter, Ann, who is now professor of immunology in the Department of Microbiology at Boston University and five years later a son, Stephen, who is professor of geology at the University of Illinois in Urbana-Champaign.

THE WAR YEARS

When the United States joined World War II in 1941, Marshak, like many other scientists, enlisted in the war effort. At first, he worked on developing radar at the MIT Radiation Laboratory. In 1943–44 he was at the Montreal Atomic Energy Laboratory (which later became the Chalk River Laboratory), where he worked for the British atomic bomb project on problems of neutron diffusion. In 1944 he joined the Manhattan Project, which was developing the American atomic bomb at Los Alamos, New Mexico. His position as deputy leader of a group in theoretical physics allowed him to be privy to the overall strategy of the creation of the atomic bomb. One of his contributions was an explanation of how shock waves work under conditions of extremely high temperatures during a nuclear explosion, when most of the energy is in radiation. These waves are now called Marshak waves. His explanation became the subject of renewed interest many years later when it helped to describe the consequences of a supernova explosion.

Both Robert and Ruth Marshak felt that Los Alamos was the most influential event in their lives. He worked among the most select group of physicists in the world, men like Bethe, Fermi, Bohr, Oppenheimer, and Feynman. With them he witnessed the explosion of the first atomic bomb, an event that affected him profoundly. The shock of the destruction of Hiroshima and Nagasaki led him to join in organizing the Federation of American Scientists, a group seeking to limit the proliferation of nuclear weapons and to ban the bomb. Marshak became chairman of the federation in 1947. In later years he was active in other organizations with similar goals, including the Pugwash Conference and the Union of Concerned Scientists. Driven by a desire to help bring about world peace and prosperity and with an understanding of the unique role that science should play in achieving these goals, he was an effective world leader in the internationalization of science.

THREE DECADES AT ROCHESTER

After the war Marshak returned to the University of Rochester, where he moved quickly through the ranks to become a chaired professor and in 1950 the head of the physics department. During his fourteen-year chairmanship it became one of the top departments in the country and a recognized center for research. Many of the world's leading physicists passed through Rochester during those years and Ruth Marshak played an indispensable role as their hostess, as she did later as “First Lady” of City College. In spite of the growing prestige of the physics department, Rochester was not considered to be in the same league with institutions such as Princeton, Harvard, MIT, Caltech, or the University of California, Berkeley. To have students of high caliber in the department, Marshak sought out the best graduate students from overseas, notably from India, Pakistan, and Japan, and brought them to Rochester, a strategy that was soon copied by other departments on the move. Many of these students later became leaders in their countries' scientific communities.

During the Rochester years, Marshak's output was prodigious; it is recorded in 4 authored and 2 edited books, some 120 articles in refereed scientific journals and in more than 20 contributions to magazines. (Over the rest of his busy life, these numbers increased to 8 books, 180 scientific articles and close to 50 general articles.) He continued his work in astrophysics and published papers on solar models, on the internal temperature and opacities of white dwarfs, and on the internal temperature-density distribution of main sequence stars. During this early period Bob was also interested in nuclear forces, nuclear binding energies, and betadecay theory. The discovery of the muon (then thought to be the Yukawa meson) led to papers on the scattering of spin 1/2 mesons by nuclei. In 1947 Bethe and Marshak were among the first to realize that the weakly interacting muon could not be the Yukawa meson, and they proposed the two-meson hypothesis, thus suggesting that a second, strongly interacting, meson (now called the pion) remained to be found. Marshak continued to study the pion and muon and in particular the interaction of the former—its production, scattering, and absorption—with nuclei. With several colleagues he worked on charge independence in multiple pion production, X rays from pi-mesic atoms, and the meson theory of nuclear forces. With his students Peter Signell and Ronald Bryan he produced the Signell-Marshak potential, which, by virtue of including the spin-orbit contribution in the nuclear force, was one of the first to give quantitative agreement with experiment. In 1952 his book *Meson Physics* was the first to be published on that subject.

Marshak was the driving force for the construction of the 240-MeV Rochester cyclotron, built by Sidney Barnes. It was the first meson-producing cyclotron after the 184-inch cyclotron at Berkeley, and in 1948 it produced pions on nuclear targets that allowed researchers to determine the pion's spin and parity. Unfortunately, its energy was too low except for threshold pion production; the accelerator could not reach the energy of the delta resonance (1232 MeV) and the Rochester cyclotron was soon eclipsed by accelerators at the University of Chicago and Columbia University. In 1951 Marshak suggested that one could determine the spin of the positive pion experimentally by comparing the cross sections for the reactions $pp \rightarrow \pi^+ d$ and $\pi^+ d \rightarrow pp$ and invoking the principle of detailed balance, which in turn is a result of the time reversal invariance of strong interactions. Immediately after Marshak made this proposal, experiments at Columbia and Rochester confirmed the spin to be zero. The Rochester cyclotron also was important in showing that protons could be polarized easily by scattering from a nucleus of zero spin, such as carbon.

Bob Marshak turned his attention to the strange particles when they were discovered in the late 1950s. He studied their expected properties: spins, magnetic moments, production, interactions, and decays. Together with S.Okubo and with his student Sudarshan, he showed for the first time that broken symmetries could account for the magnetic moments and masses of the sigma hyperons. As early as 1958, Marshak and Sudarshan studied chirality invariance and its effect on weak interactions. With Okubo, Sudarshan, W.Teutsch, and S.Weinberg, he investigated conserved currents and K-meson decays. During all these years he continued his interest in the nucleon-nucleon interaction, but after 1956, when parity violation was discovered in the weak interactions, his primary attention shifted to symmetries and the weak interaction. Two books recount the achievements of that period: *Elementary Particle Physics* (1961) by Marshak and Sudarshan and *Theory of Weak Interactions in Particle Physics* (1969), co-authored by Marshak, Riazzudin and C.P.Ryan.

Marshak's most significant scientific contribution arguably was the proposal in 1957 of the V—A theory of weak interactions in collaboration with George Sudarshan. The theory, which emphasized the importance of chiral invariance, was a starting point for the standard unified electroweak theory of Glashow, Salam, and Weinberg. Marshak and Sudarshan at that time published their theory only in the proceedings of a conference (the Padua-Venice International

Conference on Mesons and Recently Discovered Particles). Six months later a different derivation was published by Feynman and Gell-Mann in *Physical Review.* (An account of the rapid-fire developments in the origins of the universal V—A interaction appears in an article by Sudarshan and Marshak in the book *A Gift of Prophecy* mentioned in the penultimate paragraph of this memoir. Although the V—A concept was a seminal contribution to theoretical physics, a Nobel Prize was never awarded for it.

Not content with making his own major research contributions to physics, Marshak became an enthusiastic and indefatigable promoter—some have called him a prophet— of the field, even at an age when he was much too young to figure as an elder statesman. In 1950 Marshak felt that the successful conferences on present problems of physics, which had been held at Shelter Island (1947), the Poconos (1948), and Oldstone (1949), should be continued and that Rochester was the place to do so. The first of what was to become a series of annual Rochester conferences was held in December of 1950. It was attended by fewer than 100 people, who at that time constituted almost all of the U.S. theorists and experimentalists working in the field of high energy; the number also included a few from overseas. The meeting was expanded the following year and evolved later into the International Conference on High Energy Physics. This series of conferences rapidly became (and remain) the preeminent international gathering of high energy physicists. (It also served as a model for the establishment of international conferences in other fields.) Held in Rochester until 1957, the conferences then began to rotate among countries, returning to Rochester in 1960. They are amazingly vital gatherings, where new results are often announced for the first time. It was at one of the early Rochester confer ences that one of us [E.M.H.] first met Bob and was immediately impressed by his vitality.

Marshak made sure that all nations could be represented at the Rochester conference and worked very hard with the U.S. Department of State and with members of Congress to allow physicists from the Soviet Union and Eastern Europe to attend. In those years no one ever knew quite whom the Soviets would send to conferences and Bob Marshak had to insist that those who had been invited to talk would be among those permitted to come. During those days of the Cold War it was unusual to be able to discuss physics— much less politics—with Soviet scientists. Bob's initiative was not only an immense boon to physics but helped to lead the way to a rapprochement between the United States and the Soviet Union.

Marshak's intense interest in promoting international scientific cooperation and world peace manifested itself in many other activities. In 1956, after the death of Stalin, he was a member of the first delegation of six American scientists to visit the Soviet Union, where he met the leaders of the Soviet physics community, including Lev Landau. He made more trips to the Soviet Union in the late 1950s and became an acknowledged expert on Soviet science. As a result, he published articles about the subject in several magazines and was frequently interviewed by the news media. His outspoken views may have led to his being subjected to an interrogation during the McCarthy era. He was found to be a loyal American and allowed to retain his Q-clearance.

Over the years Marshak also made a large number of trips to other countries in Europe and to the Middle East, India, Pakistan, and Japan. In the 1960s he headed delegations of the National Academy of Sciences to negotiate exchange agreements with Poland and Yugoslavia. His trips provided him with an opportunity to meet the scientific and occasionally the political leaders of many countries, including Prime Minister Jawaharlal Nehru of India. He became friends with physicists such as Hideki Yukawa, Abdus Salam, and others not as well known in the United States but who played major roles in the development of science in their countries. He was a founder of the International Centre for Theoretical Physics at Trieste and a member of its Science Council from 1965 to 1975 and again from 1984 until his death. He served as secretary of the Commission on High Energy Physics of the International Union of Pure and Applied Physics.

Not one to slight the promotion of science in the United States, Marshak was involved in lobbying to establish the National Science Foundation and in many issues that came before the U.S. Atomic Energy Commission. In addition to numerous academic visiting professorships he also had connections to industry, acting as a consultant to General Electric, Eastman Kodak, and the RAND Corporation. He served as editor of two major series of physics books, one for McGraw-Hill, the other for Wiley-Interscience.

In the late 1960s, as one of four Distinguished University Professors at the University of Rochester, as well as a distinguished physicist and by then elder statesman of science, Robert Marshak could have finished his career there in a secure and, from a professional viewpoint, an ideal position. However, in this era of the Vietnam War, conflicts between the conservative administration of Rochester President W.Allen Wallis and the more liberal faculty and students surfaced on a number of issues. The faculty elected Marshak as president of the Faculty Senate and what followed was effectively a battle between him and Wallis. After the faculty passed a vote of no confidence against Wallis the two were never again on friendly terms. Rochester's turmoil became City College's opportunity.

PRESIDENT OF CITY COLLEGE (1970–79)

By 1969 the venerable CCNY of Bob Marshak's youth was in the throes of a revolution. For one thing, it had become a unit of a nineteen-campus bureaucratic system, the City University of New York. Its days as a leading and wholly disproportionate producer of undergraduates who would take their place among the country's leading physicists (as well as of a large number of intellectual leaders in most other fields of human endeavor) appeared to be over. Many bright young New Yorkers for whom CCNY would have been the only avenue to higher education and out of poverty now had the means and the opportunity to attend the Columbias and Harvards of the country. At the same time, Black and Hispanic New Yorkers, who could not meet the wholly meritocratic admissions criteria of City College, revolted against their “exclusion” and with their supporters occupied the campus. Their demands for admissions quotas that reflected their numbers in the high schools were answered with an “Open Admissions” policy for the City University. In 1970 this resulted not only in a doubling of freshman but, as public education and high-school graduation requirements in New York further deteriorated, in an influx of unprepared students and an increasing need to provide remedial education and new curricula for them.

Prior to open admissions another revolution had been launched, one that was much less noticed by the public. Led by a few “Young Turks,” mostly from the physics department, which one of us [H.L.] then headed, CCNY was transforming itself from an essentially undergraduate college to a research institution with on-campus graduate programs in the sciences, engineering, and a few other fields.

Into this somewhat schizophrenic reality of CCNY Robert Marshak was recruited to serve. Those who knew of his extraordinary achievements and commitment to academic excellence, combined with his unusual dedication to social programs and equality, believed that he was the right person to replace the dismissed president of the college. As one of the faculty members of the search committee, I [H.L.] traveled to Rochester to recruit Bob to become City College's eighth president. Before speaking with him, I had the privilege of attending one of his legendary seminars. It became easy to convince the search committee and the Board of Trustees that here was an individual who was superior to all other candidates.

What Bob Marshak set out to do as soon as he became president was to take City College into the big leagues of the public and private universities of the seventies. He engaged a public relations firm and used pageantry to make friends and influence people. He launched an unprecedented $25 million fund-raising campaign. To help carry out his programs he liberally added deans, directors, and vice-presidents to the administration, and he summed up his vision in a master plan that he named “The Urban Educational Model.”

To be sure, there were solid academic goals and achievements behind this structure and facade. Bob Marshak wanted to improve every department, whether it wanted to be improved or not, and he succeeded with about a dozen, physics most prominently among them. With the help of a major National Science Foundation development grant, the groundwork for the transformation of the physics department from a prodigiously successful undergraduate teaching operation to a research-active Ph.D. department had been laid before Bob's arrival. However, his presence at the helm and his strong if critical support made it possible to bring in such scientists as Bob Alfano, Joe Birman, Mel Lax, Sam Lindenbaum, Rabi Mohapatra, Bunji Sakita, and Harry Swinney.

To much of the world, Bob's most visible and sometimes controversial achievements were evident in the new programs he created. Most of these were motivated, at least in part, by Bob's unshakable conviction and confidence that he had the obligation to do for the economically still poor (and socially and academically very different) students what the students and faculty previously had done for themselves. The new programs were often made possible and sometimes even shaped by the wishes and ambitions of donors. In rapid succession he created a major Center for the Performing Arts, an Urban Legal Studies Program, the Center for Biomedical Education, and several other new structures and programs.

The Center (later School) for Biomedical Education can serve as a paradigm of Bob's vision and determination, as well as of his occasionally less than completely realistic expectations. As he conceived it, the center was to serve all of the following purposes: 1) to retain and win back gifted students through an accelerated curriculum (they would obtain a medical degree in a total of six years, the first four at City College and the last two at prestigious medical schools with which Bob had negotiated transfers to the third year class); 2) to have 50% of this group composed of minority students; 3) to direct the students into primary health care (rather than into specialties) and practices in underserved areas; and 4) implicitly to show the medical establishment and the country that a medical education could be provided at a much lower cost than was (and is) the practice.

Experience soon showed, unsurprisingly, that these goals were somewhat incompatible. The program experienced difficulties and controversy, including a successful reverse discrimination lawsuit, and had to be modified. It is a tribute to Bob Marshak's vision and determination that the program still exists twenty-five years after its creation and still makes a major contribution to the City College and to society.

During the City College period Marshak also vigorously pursued his lifelong commitment to international cooperation and to developing countries. Among other initiatives, he tried to set up a far-reaching exchange program with the University of Ife in Nigeria and he organized and chaired a workshop at CCNY on “Technological Development of Nigeria.” On the domestic front he organized and co-chaired with Hans Bethe a conference on “American Energy Choices Before the Year 2000.” At City College itself, Bob Marshak, motivated by his social conscience and sympathies, was extraordinarily responsive to all demands. He created not two (as had originally been demanded) but four ethnic studies departments: Black, Puerto Rican, Asian, and Jewish. He worked hard to establish ties with the Harlem community.

All of these acts of creation were initiated in the face of a rapidly deteriorating economic and political situation for CCNY, largely caused by the impending bankruptcy of New York City. This led to the abandonment after 128 years of free tuition and to severe budget cuts (which became even more traumatic in the eighties and nineties). Marshak's acts of creation were also carried out against a background of ethnic strife and agitation that was manifestly much worse than what Bob had expected when he took the job. Indeed it hardly should be called a background, because it consumed so much of Bob's time and effort; unfortunately, it also took a toll on his health. He suffered a stroke during a confrontation with a student group. It affected his physical balance for the remainder of his life, but it did not stop the intensity of his commitments and his work habits.

Not surprisingly, during his extraordinarily demanding presidency, Marshak heroically tried to keep up with physics and, stealing away to his little hideout in the physics department on as many Friday afternoons as he could, he worked with Mohapatra and others to make new contributions. His papers in *Physical Review* and in *Physical Review Letters* were mostly on CP violation and the strange particles. In the end, the deprivation of not being at the center of science, as well as the accumulated frustrations of life at City College, got to him, and at the age of sixty-three he again became a full-time physicist, at the Virginia Polytechnic Institute and State University.

THE BLACKSBURG YEARS

Robert Marshak joined the Virginia Polytechnic Institute and State University in 1979 as a University Distinguished Professor; the announcement of his appointment was made by the governor of Virginia. He continued to work in the area of quark-lepton symmetry and the construction of grand unification schemes with his former student at Rochester and colleague at CCNY, R.N. (Rabi) Mohapatra, who had moved to Virginia Tech, and with others. The failure to detect proton decays predicted by the SU (5) theory had increased interest in experimental tests of alternative options. Bob proposed tests of the SU (10) grand unification theory by studying neutron-antineutron oscillations in the nucleus, and by looking for finite mass Majorana neutrinos. With students and research associates, Marshak worked on models of quarks and leptons. He recognized the importance of anomaly cancellations as a necessary condition in the construction of a new theory. He authored several papers on chiral gauge anomalies and on the relations between the perturbative and non-perturbative ones.

Continuing a lifelong practice of good citizenship and service in his local and the wider national and international community, Bob Marshak organized several memorable physics meetings at VPI and was active in numerous scientific organizations. Among his contributions were service on the National Academy of Sciences' Commission on Human Resources and the Committee on Scientific Exchanges with the People's Republic of China; the vice-chairmanship of the United States National Committee for the International Union of Pure and Applied Physics; membership on the Governing Board of the American Institute of Physics; the chairmanship of an Advisory Committee of the U.S. Agency for International Development; and the organization of the 1984 Trieste conference on physics and development. He was elected president of University Research Associates, but he had to relinquish that responsibility because of a heart bypass operation.

A most important, if not the principal, beneficiary in the early eighties of Bob's intellect and energy was the American Physical Society (APS) and its programs and influence. After serving on its council from 1965 to 1969 and as chairman of its Division of Particles and Fields in 1969–70, he allowed himself to be nominated as vice-president after his retirement from CCNY. This led to the presidency in 1983. The recollections of his colleagues and a perusal of council minutes, as well as newspaper reports, attest that his term was very eventful and effective. Bob Marshak did not leave strong activism and controversy behind when he left City College. An example is his use of the weight of the APS to debate the Reagan Administration on the issue of placing an anti-ballistic missile system in space, a program popularly know as Star Wars. One result was an unprecedented statement on nuclear arms control that the council issued on January 23, 1983, under Bob's energetic leadership, which evoked an extraordinary negative response from George

Keyworth, President Reagan's science advisor. Another perhaps more influential outcome was the production by the APS some years later of an objective scientific study of the feasibility of directed energy weapons. A second major Marshak creation was the approval and initiation of the China Program ("Chinese-American Cooperative Basic Research Program in Atomic, Molecular, and Condensed Matter Physics" of the American Physical Society, 1983–1991.) It is now seen as one of the great contributions of the APS. This program passed the council also on January 23, 1983, by a vote of thirteen members in favor, eleven opposed, and three abstaining. Bob Marshak did not require unanimity to forge ahead; he and his convictions often constituted a strong working majority.

Marshak retired officially from his chair at Virginia Tech in 1992 at the age of seventy-five. During the four years before that and for the remaining months of his life, he worked intensely on his last book *Conceptual Foundations of Modern Particle Physics* (1993). He finished the final corrections on December 22, 1992. When he dropped the manuscript in the mailbox, he turned to his wife and said jokingly: "It's done; now I can die." The last communication I [H.L] had from Bob Marshak was also dated December 22, 1992. It is a note proudly telling us that he had been selected as the first recipient of the American Association for the Advancement of Science's Award for International Scientific Cooperation, for which I had nominated him on behalf of the APS. The next day, December 23, 1992, the Marshak family gathered in Cancun to celebrate Bob and Ruth's fiftieth wedding anniversary. Minutes after their arrival Bob took the grandchildren to the beach. While they played, he stepped into the warm water of the Gulf of Mexico. The undertow was unexpectedly strong, and he apparently lost his balance—the final manifestation of his stroke. He fell into the water, could not stand up, and drowned a few feet from shore.

ENVOI

Beginning early in his life and lasting throughout his career, Robert Marshak received wide recognition and a plenitude of honors. He was elected to the National Academy of Sciences in 1958 and to the American Academy of Arts and Sciences in 1962. He was an Alexander von Humboldt awardee, three times a Guggenheim fellow, a Sigma Xi national lecturer, a Phi Beta Kappa scholar, and a Nobel lecturer. He held distinguished visiting appointments at some twenty foreign and domestic institutions. He received three honorary degrees. On his retirement from CCNY the science building was named in his honor, in defiance of a policy he had established during his presidency of selling the names of buildings to donors. And his students and colleagues honored him with no fewer than three Festschriften, one on his retirement from Rochester in 1970 (“R.E.Marshak: The Rochester Years”); the second, in observation of his sixtieth birthday at City College (“International Symposium on Five Decades of Weak Interactions,” proceedings published in *Ann. N. Y. Acad. Sci.,* vol. 294, ed. N.P.Chang, 1977); and, upon his death, with the book *A Gift of Prophecy-Essays in Celebration of the Life of Robert Eugene Marshak* (ed. E.C.G.Sudarshan, World Scientific, 1994).

Bob Marshak was manifestly not a prophet without honor in his own country or abroad. Scores of colleagues have testified to his seminal contributions to science. The work on the V—A interaction has been described by a disinterested colleague as “a crucial turning point in twentieth century physics.” Others have spoken eloquently about the many other results of Bob's “deep physical intuition” and of their admiration for him as a “deep and creative theoretical physi cist." His leadership in the world scientific community has evoked equally strong expressions of tribute. His successful pursuit of the presidency of City College has been hailed as "an act of great courage and human compassion."

In spite of these tributes and Bob Marshak's immense achievements, his life was sometimes punctuated by disappointment and controversy. Although Marshak was anything but self-effacing or reluctant to claim credit for his accomplishments, he was not always satisfied with himself or with the recognition he received. He was extraordinarily persistent but not always patient in pursuing his ambitious goals in science and society. He did not suffer fools (or for that matter wise men and women who disagreed with him) gladly, and he occasionally exasperated colleagues and persons in high places as much as they must have exasperated him. His interaction with people was anything but weak.

At the same time he was a most generous friend and mentor, particularly to students and junior colleagues. Many have testified about his graciousness, his approachability, and the unexpected amount of time that he took to discuss their problems. A severe workaholic, he had an enormous sense of duty to deliver, fully and promptly, on everything he promised. George Sudarshan reports that "any manuscript or notes handed to him were returned with detailed comments within forty-eight hours, irrespective of how busy he was." Although physics usually took precedence over other duties (and his commitments as a statesman of science over personal concerns), he was involved in everything and he enriched the lives of more people than he or the public ever knew.

FORTUNATELY, THE LIFE and achievements of Robert E.Marshak have been well documented by himself and by others. In addition to items cited in the Selected Bibliography and other material, includ

ing personal reminiscences, we have found the following documents particularly useful: 1) *Robert E.Marshak: A Brief Biography.* Special Collections Department, University Libraries, Virginia Polytechnic Institute and State University, Blacksburg, Va., 1996. This informative and poignant work, authored by Marshak's son, Prof. Stephen Marshak, is so felicitously written that we have with permission incorporated some passages verbatim; 2) Harry Lustig. Two presidencies: The City College of New York and the American Physical Society. In *A Gift of Prophecy. Essays in Celebration of the Life of Robert Eugene Marshak,* ed. E.C.G.Sudarshan, pp. 303–309. Singapore: World Scientific, 1994. Permission to quote from this article has been granted by the publisher; and 3) Harry Lustig, Susumo Okubo, E.C.G.Sudarshan. Robert E.Marshak (obituary). *Phys. Today,* p. 105, Nov. 1993.

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SELECTED BIBLIOGRAPHY

1939 With H.A.Bethe. Physics of stellar interiors and stellar evolution. *Rep. Prog. Phys.* VI:1.

1940 With H.A.Bethe. Generalized Thomas-Fermi method applied to stars. *Astrophys. J.* 91:239.

The internal temperature of white dwarf stars. *Astrophys. J.* 92:321.

1941 With V.F.Weisskopf. On the scattering of mesons of spin 1/2 by atomic nuclei. *Phys. Rev.* 59:130.

1947 Theory of slowing down of neutrons by elastic collision with atomic nuclei. *Rev. Mod. Phys.* 19:185.

With H.A.Bethe. On the two meson hypothesis. *Phys. Rev.* 72:506.

With E.C.Nelson and L.I.Schiff. *Our Atomic World.* Albuquerque: University of New Mexico Press.

1949 On mesons μ and π. *Phys. Rev.* 75:700.

1952 With N.Francis. Elastic photoproduction of π^0 mesons in deuterium. *Phys. Rev.* 85:496.

With L.Van Hove and A.Pais. Charge independence and multiple pion production. *Phys. Rev.* 88:1211.

Meson Physics. New York: McGraw-Hill.

1954 With M.M.Levy. Present status of the meson theory of nuclear forces. In *Proceedings of the Glasgow Conference.* Oxford, U.K.: Pergamon.

1957

With P.Signell. Phenomenological two-nucleon potential up to 150 MeV. *Phys. Rev.* 106:832.

With E.C.G.Sudarshan. Nature of the four-fermion interaction. In *Proceedings of the Padua-Venice Conference on Mesons and Newly Discovered Particles.* V–14.

1958 With E.C.G.Sudarshan. Chirality invariance and the universal Fermi interaction. *Phys. Rev.* 109:1860.

With S.Okubo, E.C.G.Sudarshan, W.B.Teutsch, and S.Weinberg. The interaction current in strangeness-violating decays. *Phys. Rev.* 112:665.

Scientific research in the Soviet Union. *Science* 124:1125.

1959 With S.Okubo and E.C.G.Sudarshan. V—A theory and the decay of the Λ hyperon. *Phys. Rev.* 113:944.

With S.Okubo and E.C.G.Sudarshan. Isotopic spin selection rules and K_2 decay. *Phys. Rev. Lett.* 2:12.

1961 With E.C.G.Sudarshan. *Elementary Particle Physics.* New York: John Wiley.

1969 With Riazuddin and C.Ryan. *Theory of Weak Interactions in Particle Physics.* New York: John Wiley.

1970 My answer to the Sakharov manifesto. Lecture in "Public Understanding of Science" series. University of Texas.

1982 With the assistance of Gladys Wurtenburg. *Academic Renewal in the 1970s: Memoirs of a City College President.* Washington, D.C.: University Press of America.

1988 The pragmatic humanism of Bohr, Einstein, and Sakharov. *Proc. Am. Phil. Soc.* 132:268.
1993 *Conceptual Foundations of Modern Particle Physics.* Singapore: World Scientific.

Courtesy of Scripps Institution of Oceanography

JEROME NAMIAS

March 19, 1910–February 10, 1997

BY JOHN O.ROADS

JEROME NAMIAS WAS one of the world's greatest long-range forecasters of what he liked to call the world's second most complex problem.[1] Hecht (1986) described him as "a man who gives good reasons for any long-range forecast and even better reasons for why it fails…a man who is an infinite source of good ideas…who thinks fast on his feet …is always a scholar…and a gentleman." While lacking formal meteorological training, Namias eventually received the highest awards of the American Meteorological Society and helped to found the long-range forecasting branch of the U.S. National Weather Service and the Climate Research Division and the Experimental Climate Prediction Center at the Scripps Institution of Oceanography. In November 1989 Namias suffered a stroke. Although he was aware of events around him, he was partially paralyzed and was unable to speak or write thereafter. His loving wife Edith, daughter Judith Immenschuh, and grandchildren Dylan and Sionna survive him.

FALL RIVER

Jerome Namias grew up in Fall River, Massachusetts, the son of Joseph and Sadie (Jacobs) Namias. He became en thusiastic about the weather because of a high school physics teacher and the town's amateur meteorologist, who was a cooperative observer for the weather bureau and a wealthy broker. Eventually Namias set up his own weather station, using instruments he bought from his earnings as a door-to-door salesman and a jazz drummer. When Namias heard about the American Meteorological Society, founded by Charles F.Brooks in 1919 and that its requirements for membership were modest ("a sincere interest and annual dues of $2 per year"), he immediately joined. Although his friends thought that Namias was getting into meteorology because of the money,[2] his father initially saw little or no chance of Namias making a living on what he thought was a hobby.[3] Namias began his "hobby" by keeping records and drawing weather maps from reports published in the daily newspaper, but soon he began making forecasts for his friends, a practice he was to carry out to the extreme for the rest of his life.[4]

On graduation from high school, Namias was offered a four-year scholarship to Wesleyan University in Connecticut; however, because of his father's illness and the Great Depression, Namias decided to stay home and try to find a job to help his family out. Namias subsequently became ill with tuberculosis and was confined to his home. Even so, his appetite for self-study soon emerged and he took many correspondence courses, including a course in meteorology given at Clark University by Charles F.Brooks. Although Namias was never to receive an undergraduate degree,[5] he eventually received an M.S. in 1941 from the Massachusetts Institute of Technology. In 1972 the University of Rhode Island awarded him an honorary Ph.D., as did Clark University in 1975.

FIRST METEOROLOGICAL JOB

At the end of his confinement, Namias wrote letters to many meteorologists asking for a job, citing his study of their papers and books. He was unsuccessful until H.H. Clayton at the Blue Hill Observatory, who was working with Charles G.Abbot, secretary of the Smithsonian Institution, finally offered Namias a job. Clayton interviewed Namias by asking him to extract station pressures from the isobars on some random weather maps. Clayton took the same test and, when the results were in, he found that both he and Namias made one mistake. Namias was hired on the spot and sent to the weather bureau in Washington, D.C., where the data was (and still is). This position involved getting data for compilation of world weather records, an internationally known series put out by the Smithsonian, and for solar weather studies. Namias took this opportunity to read and meet the many famous meteorologists whose articles and books he had previously read. In the weather bureau library, Namias soon discovered the many scholarly papers of the Norwegian or Bergen School,[6] and these papers were to influence his later research. Namias also found the initial set of scientific reports issued by Carl G.Rossby's newly founded department of meteorology at the Massachusetts Institute of Technology (MIT) and with colossal nerve wrote a letter to Rossby politely questioning a couple of his statements in one of his papers. Soon Namias received a response from Rossby saying that Namias was partially correct, and would Namias stop by and see him when he got a chance.

MIT

Rossby was to have a major influence on Namias's life. To help out with undergraduate tuition, Rossby arranged a job for Namias. Namias began to take and analyze the recordings of the research aircraft instruments used by the department at the East Boston Airport. Sometimes, Namias's work entailed fourteen-hour days, which included tracking balloon runs with the help of a theodolite to determine wind directions and speeds at various altitudes. This was sometimes dangerous work. According to Namias (1986), "On one occasion, a hometown friend came to visit at the airport, and I encouraged him to watch me send up the pilot balloons. He came into the shed where balloons were inflated. Shortly thereafter I noted that he was smoking, while I was inflating the balloons from the tank of hydrogen!"

Namias became interested especially in the structure of air masses and fronts as determined by the rapidly expanding aerological network of airplane soundings and pilot balloon wind soundings made in Boston, as well as at other places like Detroit and Chicago. These new soundings made it possible for Namias to construct cross-sections through the fronts by combining ascents in time and space (1934). Some of the central ideas for Namias's analysis of the frontal structure stemmed from the work of J.Bjerknes, who had pioneered aerological studies of cyclones over Europe and from research carried on at MIT by Prof. Hurd Willett, an authority on American air masses and fronts. Namias eventually was to write a series of introductory articles on stability and air mass properties (1936).[7]

In 1934, with the advent of the rapidly expanding airline industry and its desire to establish meteorological departments,[8] Namias (on the advice of Rossby) accepted a job at Trans World Airlines, first at Newark and then at Kansas City. In these assignments, Namias got a taste of the real world of meteorology: round-the-clock work shifts and stressful forecasting for early transcontinental flights. Problems in volved icing on aircraft; low ceilings; zero visibility due to fog; blowing dust in the Dust Bowl area; and hazardous winds. There was no time for scientific investigations or for Namias to continue his undergraduate education. He eventually got the impression that airline meteorologists were "second-class citizens around the air terminals, who often served as scapegoats for weather-related accidents." Therefore, when TWA had to downsize temporarily due to the curtailment of government airmail service, Namias was happy to return to part-time work at MIT and Blue Hill Observatory, even though he had to learn to live on student pay once again.

Namias, by this time, was known as an expert forecaster. He gave advice to Piccards in connection with his record-setting high-altitude balloon flights. He helped out at the national gliding and soaring contest in New York, where Dupont made a distance record for the United States by using Namias's forecast of a strong frontal passage to glide all the way to Boston. According to Namias (1986), "I still marvel at his courage and my colossal nerve in proposing such a dangerous flight path!" Namias was not infallible. Asked to forecast for the Harvard tercentennial, Namias forecast a light rain, which turned into a ruinous downpour. Bad forecasts[9] would disappoint, but they would never stop Namias, who felt that forecasts were one of the best teaching tools a meteorologist had. Gilman (1986), who succeeded Namias as chief of the weather bureau, would later stress the probabilistic nature of forecasts, which is especially important when dealing with the growing uncertainty at long ranges.

Namias then decided to finally get his undergraduate degree, enrolled at the University of Michigan, where tuition was more affordable than at MIT. Unfortunately, serious physical problems (pleural effusion) forced him once again to abandon his undergraduate plans and return to Fall River, where he again proceeded with self-study and also published a seminal paper on atmospheric inversions (1936). Again impressed, Rossby offered Namias a graduate assistantship beginning with the 1936 fall term at MIT. Rossby had just begun working on his theory of long waves on the westerlies and was trying to convince people of its validity. One of the main difficulties in applying Rossby's ideas involved the lack of data aloft, particularly over the oceans. At Rossby's suggestion, Namias constructed a trial upper-level map by judicial extrapolations, estimating quantitatively the flow patterns aloft over the North Atlantic, as well as the United States. Namias was later one of the unnamed contributors to Rossby's paper (1939).[10]

At MIT Namias met Edith Paipert, who was to become his wife in the fall of 1938. Harry Wexler, his best friend since grammar school, had married her sister Hannah, and years earlier Namias had introduced Harry to Rossby and the field of meteorology.[11] Edith was an artist[12] and had a feel for symmetry, balance, and aesthetics, and would comment on the aesthetics (or lack thereof) of Namias's weather maps. “It soon became clear that the parts of my analysis that she did not like were incorrect and could be made both more artistically satisfying and scientifically correct by modification. It was then that I realized the close association between art and science. In fact, in a couple of courses I taught at MIT, this philosophy was stressed, much to the chagrin of a few of my contemporaries.”

Namias made a name for himself at MIT with his isentropic analyses (1938), which was the basis for his getting the first Meisinger Award of the American Meteorological Society in 1938. Namias reasoned that isentropic analysis was an exceptionally valuable tool for precipitation forecasting, particularly when the moist and dry tongues were clear and easily identified. Isentropic trajectories often carried the high-level moisture southward into the southern states, even though the surface winds were blowing from the south. Thunderstorms occurred where the deep moist air enhanced convection, which normally is impeded by entrainment of dry air aloft. Summer thunderstorms over the Great Plains of the United States did not occur haphazardly, but they frequently moved in clusters in upper-air moist currents that flowed in great anticyclonic systems in mid-troposphere. Eliassen (1986) noted later that he and his contemporaries, who had studied Namias's aerological papers before the war, were amazed to see that someone their age (twenty-three) had written such extraordinary papers. Eliassen further noted that isentropic analysis has provided the basis for much of modern meteorological instability theory developed later by Charney and others.

EXTENDED-RANGE FORECASTS

It was another aspect of Namias's research, though, that was to capture his attention and eventually become part of his identity. Namias was the junior member (graduate assistant) of the team that was trying to develop extended forecasting on time scales on the order of a week. According to Hecht (1986), this was reported in the *New York Times* as: “The weather bureau has enlisted the aid of experts from several universities in starting a study of long-range forecasting.” According to Namias (1986), “It soon became clear that none of us knew what we were doing, other than coloring charts with red and blue crayons.” In fact, Namias was instrumental in developing the scientific basis for experimental forecasts for times (then) as far as five days into the future.[13]

WORLD WAR II

The military services became especially interested in MIT's extended forecast work (1941), and it was eventually decided to shift the extended forecasting project to Washington, where it would be closer to defense preparations for the looming war. Namias was asked to take a one-year leave of absence from MIT and head up the controversial project. According to Namias (1986), "Our reception by some of the weather bureau personnel was not exactly cordial… attempting forecasts for a period of five days in advance was [thought to be] utterly foolish." Namias was to head the extended-range forecast division for the next thirty years, and he wrote a monograph on extended forecasting techniques, which was promptly stamped confidential. A few years later, this monograph was declassified, brought up to date, and printed for general distribution (1947).

During the war, Namias supervised an historical sea-level map project;[14] lectured Air Force cadets, Navy officers, and civilians at various university training centers; and made extended predictions for many wartime events. Namias received a citation from Navy Secretary Frank Knox for his sea-state forecasts for the North African invasion.[15] Namias also made forecasts for favorable periods for the transfer of disabled vessels to other ports for repair; estimates of the likely course of incendiary balloons from Japan; favorable and unfavorable conditions for the possible invasion of Japan; and certain aspects of the meteorology for bombing raids.[16]

NUMERICAL WEATHER PREDICTION

After the war, meteorology changed to a more computational science. At the Institute of Advanced Study in Princeton, Johnny von Neumann initiated a project in nu merical forecasting with the use of the supercomputer of the day. This project involved Jule Charney, Phil Thompson, Johnny Freeman, Hans Panofsky, Ragnar Fjortoft, Arnt Eliassen, Joe Smagorinsky, Norm Phillips, and many others. Charney developed the first successful numerical forecast using the barotropic model, which Namias had been employing for several years following Rossby's classical 1939 work. Namias (1986) was fond of noting that “at the first meeting to discuss the new Princeton endeavor, to which about 35 of the nation's top meteorologists were invited to give advice, no one suggested, as a starting point, the barotropic model!” Although Namias was photographed with the group that made the first forecast (see Namias, 1986), he was really only peripherally involved. His role was mainly to make sure the computer-generated forecasts resembled the real atmosphere.[17]

CONFLUENCE

Rossby, who returned to Sweden after the war to found the International Institute of Meteorology, subsequently invited Namias to Stockholm.[18] There Namias investigated variations in upper airflow patterns. Particularly noteworthy was his study (1949) of confluence with his long-time colleague Phil Clapp. Confluence and diffluence qualitatively describe asymmetric variations in the upper-level winds. At the upper levels, the strongest climatological winds or jets occur off the coast of Eurasia and North America (and over North Africa). In the entrance region to these jets, a thermodynamically direct circulation occurs. That is, warm air rises in the south and sinks in the north. In the diffluent regions over the ocean, an opposite indirect circulation occurs. As discussed by Newton (1986) confluence theory has been increasingly studied in recent meteorological lit erature (jet streaks) and its popularity will grow as we gain more experience understanding vertical circulations.

INDEX CYCLE

Namias's stay in Sweden resulted in another notable paper, a study of the index cycle (1950), the slow wintertime phenomenon when the westerlies first slowly decline and then recover in a cycle of about four to six weeks. As noted by Lorenz (1986) the zonal index, or blocking variations, appears to happen at about the same time each spring and carry with it alterations in the positions and intensities of the centers of action. Implicit in this and other work was the fact that synoptic scale systems often went through a cycle in about a week, only to return in similar form in the following week or so, suggesting quasi-periodicity, although Lorenz noticed that the motions were actually chaotic. Lorenz further noted that these kinds of studies were forgotten for some time because of the advent of numerical prediction. It was only much later that people began to realize that even these kinds of slowly varying and seemingly predictable phenomena were in fact quite sensitive to initial conditions. Models to date have not been wholly successful in predicting the onset or demise of high and low index conditions.

Because of their potential for prediction, people will continue to search for these and other periodicities. Namias (1986) wrote that Irving Langmuir, the Nobel laureate, had tried to show that his seeding of clouds in New Mexico was responsible for establishing a weekly periodicity in many meteorological elements as far away as the Ohio Valley. Langmuir became greatly interested in Namias's work and invited him to spend a few days with him at General Electric Company's Knolls Laboratories near Schenectady, N.Y. “Although he worked hard to convince me that the period icity found over the Ohio Valley was due to seeding in New Mexico, I was able to demonstrate in this and in other cases that periodicity could be explained in terms of the evolution of the general circulation on the appropriate time scales."

Based on the low frequency variations in the index cycle and other evidence, Namias (1953) decided to expand the five-day predictions, which were by then routine, to thirty days. He also began to issue advisory statements about hurricane probabilities a month in advance. Namias reasoned that changes in the large-scale wind patterns could be used to determine whether or not small-scale hurricanes would be more or less threatening to the U.S. East Coast. A lot of publicity was generated, which resulted in the establishment of the National Hurricane Center (Taba, 1988). Namias's advisories were later stopped, because it was claimed they were harming the tourist trade. Even though Namias (Taba, 1988) considered his advisories a progressive public service (which put him in the limelight during his regular TV appearances) his efforts were always at the hazy frontier and were not popular at the more conservative weather bureau. These efforts, as well as the many previously mentioned efforts, resulted in his being given in 1955 the highest award of the American Meteorological Society, the Award for Extraordinary Scientific Achievement.[19]

LAND INFLUENCES

In 1955 Namias also received the Rockefeller Public Service Award, which made it possible for him to spend a year at a place of his choice, and he once again chose Stockholm. As summarized by Walsh (1986) and Anthes and Kuo (1986), Namias began to write there about the influence of the land and snow on the atmosphere. Namias (1955) suggested that the soil moisture in the Great Plains of the United States played an important role in the Great Plains drought by varying the heat input to the overlying atmosphere. That is, heat could be used for sensible heating of the soil or for producing long period lags in the general circulation. This paper also stressed that the drought-producing upper-level high-pressure cell over the Great Plains is dependent on similar anomalous cells over both the North Pacific and North Atlantic, operating through teleconnections. Once this triple cell pattern was established, soil moisture deficits could feed back to help maintain the continental high cell. These effects later were exploited by Van den Dool and others and are still being explored in modern coupled (air-land) hydrologic experiments started by GEWEX (Global Energy and Water Cycling Experiment).

OCEAN INFLUENCES

A major turning point for Namias occurred at the 1957 Rancho Santa Fe CalCOFI (California Cooperative Fisheries) conference of the Scripps Institution of Oceanography. Invited by John Isaacs to speak about the anomalous period that had sparked the conference, Namias (1959) gave a standard climate diagnostics talk about the anomalous mid-altitude events and then sat back to listen to the other speakers (Bjerknes, Charney, Munk, and Stommel, among others). A remarkable oceanic warming (which we now all know as El Niño) had occurred over the eastern Pacific. Southern fish were being caught in northern waters, unusual typhoons were observed, and, in general, both atmosphere and ocean were far from normal. This sea-surface-temperature abnormality also included anomalies in the marine biota, the California Current, and some marine chemical properties. Namias (1986) wrote, “The inter-associations quickly became clear, and it struck me that some of the secrets of long-range weather forecasting might lie in the coupled air-sea system. It was especially noteworthy that the mismatch of time scales in the two media, air and sea, could account for the frequently observed long-term memory required for long-range problems."

Namias thereafter began to draw on the influence of the ocean surface in other studies; however, it was really to be several years before he could actually begin to work full time on the large-scale air-sea interaction and persistent boundary condition problem, efforts that were to occupy him the rest of his life. His mentor Rossby and his best friend and brother-in-law Harry Wexler passed away from heart problems. Namias also had a heart attack in 1963 and in the summer of 1964 he was involved in a bad automobile accident in Boston. Growing increasingly tired of all the budget battles,[20] he decided to retire from the weather bureau the same year.[21]

SCRIPPS

Actually, Namias never retired; he just changed locations.[22] Namias's earlier work increasingly had drawn the attention of many diverse groups of scientists, including John Isaacs at the Scripps Institution of Oceanography, where he had earlier been stimulated to begin working on ocean atmosphere problems. Namias (1986) wrote, "My new friends were most receptive to fresh ideas about low frequency phenomena in the upper ocean and lower atmosphere. Consequently, I returned each of the succeeding three years for six months at a time before deciding to retire from NOAA [National Oceanic and Atmospheric Administration] and live in La Jolla, a decision I never regretted." In the years following his move to La Jolla, many air-sea problems attracted his interest in the newly formed climate research group, which he founded with the help of Director Bill Nierenberg. That group initially included Tim Barnett, and later became a separate research division under Richard

Somerville. Initial studies were concerned with defining the time and space scales of large-scale air-sea interactions (1970, 1972), and Namias put out several important large-scale air-sea atlases (1975, 1979, 1981) that were used by many other researchers. Namias would use his long experience with climate variations to carefully diagnose low frequency atmospheric and oceanic behavior.[23]

From a number of carefully analyzed case studies (e.g., 1976), it became evident to Namias that, if abnormally warm water were generated at high latitudes during the summer, the Aleutian Low in the subsequent fall would be intense. Similarly, cold water in the summer would lead to abnormally high pressure in the fall. He reasoned that warm anomalies would amplify cyclones by destabilizing cool air masses by the contributions of the surface sensible and latent heat, whereas the opposite situation would occur over the cold anomalies. Namias further suggested that pools of anomalous water might be hidden at depths below the surface thermocline during the warm season. With the onset of increased fall storminess these subsurface anomalies could be mixed vertically again, thus providing for generation of surface anomalies unaccounted for by other factors. In 1981 Namias received the Sverdrup Gold Medal of the American Meteorological Society for his pioneering efforts on air-sea interactions.

Namias's air-sea interaction theories were not universally accepted. As noted by Haney (1986), a number of subsequent papers by Hasselmann, Frankignoul, Davis, and others showed that it was much easier to produce SST anomalies from atmospheric anomalies than it was to produce atmospheric anomalies from SST anomalies. A number of early general circulation model (GCM) sensitivity experiments by Kutzbach, Huang, Chervin, Houghton, and others also showed a remarkable insensitivity[24] to mid-latitude

SST anomalies, whereas these same early GCM experiments at least showed some sensitivity to tropical SSTs.

Namias sometimes has been identified as a strict proponent of only mid-latitude SST effects, in part because he was somewhat skeptical that influences from remote tropical SSTs could overwhelm influences from local SST anomalies. According to Smagorinsky (1986), this discounting of tropical effects was due in part to the standard weather bureau Northern Hemisphere maps, to which Namias had access.[25] Still, when the TOGA (Tropical Ocean, Global Atmosphere) program was launched, Namias was a strong supporter, although he probably would have been a stronger supporter if it had been named GOGA (global ocean, global atmosphere). Namias's many teleconnection and mid-latitude air-sea studies had convinced him that knowledge of the global ocean and global atmosphere ultimately would be required. Despite Namias's tropical skepticism, he did do some seminal ENSO (El Niño Southern Oscillation) work. As noted by van Loon (1986), Namias (1976) was the first person to describe both extremes of the ENSO cycle, as well as its association with temperate latitude wind systems over the North Pacific. Perhaps more characteristically though, Namias and Dan Cayan (1984) demonstrated the lack of uniqueness in middle latitudes for different El Niño years. In much of his work (mid-latitude and equatorial), Namias had the encouragement of J.Bjerknes, the great pioneer in El Niño and Southern Oscillation studies, who was stationed nearby at the University of California, Los Angeles, and who worked with Namias on the NORPAX program.[26]

SEASONAL FORECASTS

Even though Namias officially had retired from government service in 1971, several requests for forecast informa tion from high government sources continued. Among these requests were estimates of the character of the forthcoming winter over the East during the oil embargo of 1974. After several cold winters, Namias predicted that this critical winter would be mild and on the basis of that prediction, the Carter administration decided not to issue gas-rationing cards.

More weather and climate aberrations occurred during the winter of 1976–77, when the Far West suffered a severe drought and the eastern two-thirds of the nation was very cold, with frequent snows. These abnormalities were associated with large anomalies in upper-air wind patterns and in North Pacific sea-surface temperatures. The pressure patterns had a strong ridge in the Far West and a strong trough over the East. Namias wrote, “These patterns were remarkably stable from month to month over a six-month interval from fall to winter, so that a persistence forecast would have been quite successful. Of course, one would have had to know in advance that the period was to be so persistent.” Namias (1978) suggested that several premonitory signs showed up in the fall of 1976, including the forcing Pacific SST patterns, atmospheric flow patterns with strong teleconnections, an El Niño in the tropics, and some early snows, providing enhanced baroclinicity along the eastern seaboard. All of these factors and the suggested enhancement by the normal general circulation led to an excellent forecast for the 1976–77 winter.

Many numerical studies of this abnormal winter have since been conducted. At a large NATO-sponsored workshop in Italy both Joe Smagorinsky of GFDL and Namias were invited to speak. Namias discussed the synoptic and statistical characteristics of the meteorological situation, as well as his intuitive forecast. Smagorinsky then described the results obtained by Miyakoda, a member of Smagorinsky's staff at Princeton, which employed a sophisticated model to pre dict the weather for the entire month of January 1977, using the data of January 1, 1977, as initial data. Impressed by Miyakoda's predictions, Namias stated that he felt privileged to be present at this public unveiling, much as he had been present on the occasion of the first numerical forecast made a few decades earlier in Princeton. Later, less skillful numerical forecasts reinforced his often-stated opinion that machines would never replace human forecasters; rather, they would be only a tool forecasters used at long ranges.

To conclude, one of Namias's major accomplishments at Scripps was to help develop an experimental climate forecast center where novel techniques could be developed and tested before being put into operation by the weather services. Namias was among several meteorologists who testified before congressional committees about the desirability of passage of this act, and he was especially impressed by the interest and questions of Senator Hubert Humphrey and Representative Charles Mosher, as well as those of Senators Alan Cranston, Adlai Stevenson, Jr., among others. Eventually the National Climate Act was passed, and in the ensuring competition among peers, Namias and Scripps colleagues obtained the first such center for Scripps beginning in 1981. This center has continued to thrive, as has the Climate Research Division, which he founded earlier.

Namias's work thus had come full circle. From the earliest beginnings at MIT, where he was a junior member of a project devoted to making experimental five-day forecasts, to the Scripps Experimental Climate Prediction Center, where he started experimental seasonal forecasting efforts, Namias was the extreme forecaster.

Namias was so much more than his modest description of himself (Namias, 1986): “A good synoptic meteorologist who was fortunate enough to have been on the scene when great advances were being made—and one who…participated in some of the advances." Namias was an inspiration to several generations[27] of meteorologists and climatologists, not only from the podium of a large lecture hall, but also in one-on-one conversations. He, therefore, rightly gathered a number of honors over the years. The most gratifying of all these honors was his election to the National Academy of Sciences. Namias wrote, "Something I thought would never happen because of the fuzzy nature of my field of research and my poor formal background…. It is an honor that strengthens my belief in our system, where a person is judged solely on the basis of his contributions."

EDUCATION

Durfee High School, Fall River, Massachusetts
Massachusetts Institute of Technology, 1932–34, 1940–41, M.S. degree
University of Michigan, 1934–35
University of Rhode Island, honorary D.Sc., 1972
Clark University, honorary D.Sc., 1984

AWARDS

1938	Meisinger Award, American Meteorological Society
1943	Citation from Navy Secretary Frank Knox for weather forecasts in connection with the invasion of North Africa
1950	Meritorious Service Award, U.S. Department of Commerce
1955	Award for Extraordinary Scientific Accomplishment, American Meteorological Society Rockefeller Public Service Award
1965	Gold Medal Award, U.S. Department of Commerce
1972	Rossby fellow, Woods Hole Oceanographic Institution
1977	Visiting scholar, Rockefeller Study and Conference Center, Bellagio, Italy
1978	Headliner Award (Science), San Diego Press Club
1981	Sverdrup Gold Medal, American Meteorological Society
1984	Compass Distinguished Achievement Award, Marine Technology Society

	Associates Award for Research, University of California, San Diego
1985	Department of Commerce Certificate of Appreciation

SOCIETIES

American Academy of Arts and Sciences (fellow)
American Association for the Advancement of Science (fellow)
American Geophysical Union (fellow)
American Meteorological Society (fellow), councilor 1940–42, 1950–53, 1960–63, and 1970–73
Board of Editors, Geofisica Internacional, Mexico
Explorers Club (fellow)
Mexican Geophysical Union
National Academy of Sciences
National Weather Association
Royal Meteorological Society of Great Britain
Sigma Xi
Washington Academy of Sciences (fellow)

NOTES

1. Namias liked to say that predicting human behavior was the most complex problem.

2. The cooperative observer was a millionaire.

3. His father was an optometrist for the immigrant New England Mill Workers and wanted Jerome to follow in his footsteps much like his older brother.

4. Namias would give a detailed exposition to anyone who would listen on where the climate system had been and where it was headed, while pointing out many pertinent features on some of the many synoptic maps covering his office walls.

5. Namias may be the only member of the National Academy of Sciences with no undergraduate degree.

6. C.Rossby and J.Bjerknes were members of this school.

7. As noted by Fultz (1986), Namias was instrumental in introducing air mass concepts to the U.S. community.

8. The aviation industry has always been highly dependent on and a strong supporter of the National Weather Service. Standard products and forecasts are tailored precisely for that industry.

9. A number of other bad forecasts are mentioned in Namias's

(1986) autobiography, including one on his honeymoon.

10. According to Lorenz (1986), the paper by Rossby and collaborators (1939) may be one of the best-known meteorological papers ever published.

11. Harry Wexler would eventually go on to become chief of research at the National Weather Service.

12. Among the many paintings of Edith Namias is the cover of the National Academy of Sciences' “GOALS” document.

13. Extended-range forecasts at five days are standard weather service medium-range forecast products; the weather service, as well as many other groups, are now making even longer range seasonal forecasts.

14. As noted in a Namias memorial talk given by van den Dool at the 22nd Annual Climate Diagnostics Workshop, this was really the first major reanalysis project. All the major numerical weather prediction centers have since carried out major reanalyses, which now involve both model predictions as well as observations.

15. Sverdrup and Munk developed the oceanographic prediction techniques, which depended on estimates of the wind systems over much of the North Atlantic several days in advance.

16. According to Edith Namias, because men's lives were at stake, this was the most stressful period of Namias's life.

17. Many years previously (ca. 1920) the great British scientist Richardson made the world's first numerical forecast and was notably wrong by many orders of magnitude.

18. According to Edith Namias, because of Rossby and all his parties, their stay in Sweden was very pleasant.

19. Later re-named the Rossby Research Medal.

20. Rasmusson (1998) noted that the field of numerical weather prediction then developing eclipsed empirical research for the next several decades, which resulted in a real decline in budget dollars for Namias's empirical efforts.

21. His long-time collaborator Phil Clapp was also retiring at the same time, and Clapp also passed away in 1997, a few weeks after Namias.

22. As noted by Cayan (1998), Namias's collected works (1984) contain the same number of papers before Scripps (73) as the number written after his work began there (72).

23. Namias was among the first to describe interdecadal variabil

ity, which was later amplified by other collaborators (e.g., Dickson and Namias, 1976; Douglas et al., 1982) and is now being reinvestigated with modern coupled models and improved data sets.

24. Some of this insensitivity was probably due to the model used, which was part of the early generation of general circulation models. Other models have since shown greater sensitivity to global SSTs.

25. Not too many years ago, only information north of 20N was included in almost all weather map displays.

26. NORPAX was an outgrowth of the Scripps North Pacific Studies started earlier by John Isaacs.

27. Namias often mentioned the time when someone came up to him after a lecture and congratulated him on following in his father's footsteps.

REFERENCES

Anthes, R.A., and Y.-H.Kuo. 1986. The influence of soil moisture on circulations over North American on short time scales. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Cayan, D. 1998. Tribute to Jerome Namias: The Scripps era. Namias Symposium on the Status and Prospects for Climate Prediction. 78th annual meeting of the American Meteorological Society, Phoenix, Ariz.

d'Ursin, P., and J.O.Roads. 1988. Jerome Namias: The world and I. Time Magazine Publications.

Eliassen, A. 1986. A method pioneered by Jerome Namias: Isentropic analysis and its aftergrowth. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Fultz, D. 1986. Residence times and other time scales associated with Norwegian air mass ideas. Namias Symposium, 1986, ed. J. O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Gilman, D.L. 1986. Expressing uncertainty in long-range forecasts. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Ghil, M. 1988. Namias Symposium, 1986, ed. J.O.Roads. *Bull. Am. Meteorol. Soc.* 69:418–19.

Haney, R.L. 1986. Some SST anomalies I have known, thanks to J. Namias. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Hecht, A.D. 1986. Certificate of achievement. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Lorenz, E.N. 1986. The index cycle is alive and well. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Newton, C. 1986. Global circulation to frontal scale implications of the confluence theory of the high tropospheric jet stream. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Rasmusson, E. 1998. Tribute to Jerome Namias: The pioneering years. Namias Symposium on the Status and Prospects for Climate Prediction. 78th annual meeting of the American Meteorological Society, Phoenix, Ariz.

Smagorinsky, J. 1986. The long-range eye of Jerry Namias. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Taba, H. 1988. The Bulletin interviews Dr. Jerome Namias. *WMO Bull.* 37:156–69.

van Loon, H. 1986. The characteristic of sea level pressure and sea surface temperature during the development of a warm event in the southern oscillations. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

Walsh, J. 1986. Surface-atmosphere interactions over the continents: The Namias influence. Namias Symposium, 1986, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

SELECTED BIBLIOGRAPHY

1934 Structure of a wedge of continental polar air determined from aerological observations. MIT course. Professional notes no. 6.

Subsidence within the atmosphere. Harvard Meteorological Studies. Cambridge, Mass.: Harvard University Press.

1936 An introduction to the study of air mass analysis. *Bull. Am. Meteorol. Soc.* vol. 17:1–84.

Structure and maintenance of dry-type moisture discontinuities not developed by subsidence. *Mon. Weather Rev.* 64:351–58.

1938 Thunderstorm forecasting with the aid of isentropic charts. *Bull. Am. Meteorol. Soc.* 19(1):1–14.

1939 With C.-G.Rossby and others. Relation between variations in the intensity of the zonal circulation of the atmosphere and the displacement of the semi-permanent centers of action. *J. Mar. Res.* 2:38–55.

1941 With H.C.Willett and R.A.Allen. Report of the five-day forecasting procedure, verification and research as conducted between July 1940 and August 1941. Papers in Physical Oceanography and Meteorology. MIT and Woods Hole Oceanographic Institution IX(1):1–88.

1947 Extended forecasting by mean circulation methods. Extended Forecast Section, U.S. Weather Bureau, pp. 1–89.

1949 With P.F.Clapp. Confluence theory of the high tropospheric jet stream. *J. Meteorol.* 6:330–36.

1950 The index cycle and its role in the general circulation. *J. Meteorol.* 7(2):130–39.
1953 Thirty-day forecasting: A review of a ten-year experiment. Meteorological Monograph No. 2. American Meteorological Society.
1955 Some empirical aspects of drought with special reference to the summers of 1952–54 over the United States. *Mon. Weather Rev.* 83:199–205.
1959 Recent seasonal interactions between North Pacific waters and the overlying atmospheric circulation. *J. Geophys. Res.* 64:631–46.
1968 Long-range weather forecasting: History, current status and outlook. *Bull Am. Meteorol. Soc.* 49:438–70.
1970 With B.M.Born. Temporal coherence in North Pacific sea-surface temperature patterns. *J. Geophys. Res.* 75:5952–55.
1972 Space scales of sea-surface temperature patterns and their causes. *Fish. Bull.* 70:611–17.
1975 Northern Hemisphere seasonal sea level pressure and anomaly charts, 1947–1974. In *CalCOFI Atlas 22,* eds. A.Fleminger and J.Wyllie. La Jolla, Calif.: Scripps Institution of Oceanography. 243 pp.
The contributions of J.Bjerknes to air-sea interaction. In *Selected Papers of Jacob Aall Bonnevie Bjerknes,* ed. M.G.Wurtele, pp. 16–18. North Hollywood, Calif.: Western Periodical Co.
1976 With R.R.Dickson. North American influences on the circulation

and climate of the North Atlantic sector. *Mon. Weather Rev.* 104:1255– 65.

Negative ocean feedback systems over the North Pacific in the transition from warm to cold seasons. *Mon. Weather Rev.* 104:1107–21.

Some statistical synoptic characteristics associated with El Niño. *J. Phys. Oceanogr.* 6:130–38.

1978 Multiple causes of the North American abnormal winter 1976–1977. *Mon. Weather Rev.* 106:279–95.

1979 Northern Hemisphere seasonal 700-mb height and anomaly charts, 1947–79, and associated North Pacific sea surface temperature anomalies. In *CalCOFI Atlas 27,* ed. A.Fleminger. La Jolla, Calif.: Scripps Institution of Oceanography. 275 pp.

1981 Teleconnections of 700-mb height anomalies for the Northern Hemisphere. In *CalCOFI Atlas 29,* ed. A.Fleminger. La Jolla, Calif.: Scripps Institution of Oceanography. 265 pp.

1982 With A.V.Douglas and D.Cayan. Large-scale changes in North Pacific and North American weather patterns in recent decades. *Mon. Weather Rev.* 110:1851–62.

1983 The history of polar front and air mass concepts in the United States—An eyewitness account. *Bull. Am. Meteorol. Soc.* 64:734–55.

1984 With D.Cayan. El Niño: Implications for forecasting. *Oceanus* 27:41– 47.

Short Period Climatic Variations. Collected Works of J. Namias, vols. I–IV. San Diego: UCSD Press.

1986 Autobiography. Namias Symposium, ed. J.O.Roads. Scripps Institution of Oceanography Reference Series 86–17.

EDWARD PURDY NEY

October 28, 1920–July 9, 1996

BY ROBERT D.GEHRZ, FRANK B.MCDONALD, AND JOHN E.NAUGLE

UNIVERSITY OF MINNESOTA regents professor emeritus Edward Purdy Ney was a gifted, dedicated scientist and teacher whose research career spanned the period from the onset of World War II through the early 1990s. He made important contributions to nuclear physics, cosmic-rays astrophysics, heliospheric studies, atmospheric sciences, and infrared astronomy. Throughout his career, he chose to be at the frontier and to work in emerging fields of science, accompanied by a small, devoted group that included colleagues, technicians, engineers, and graduate students. As a field developed and became more crowded, he sought a new and often very different research frontier, while remaining securely anchored to the University of Minnesota from 1947 onward. Ney pioneered the development of sophisticated particle detector systems, including cloud chambers and scintillation counters, for studies at the top of the atmosphere. He flew the first space science experiment on NASA manned flight, *Gemini 5,* and founded the O'Brien Observatory in Minnesota, where he developed new Dewar and bolometer technology to make some of the early observations in infrared astronomy. He participated in the semi nal discovery of heavy nuclei in the galactic cosmic radiation.

Together with Nick Woolf, he showed that silicate and carbon grains, the building blocks of the planets, form in circumstellar shells around aging stars. He was an excellent teacher, particularly at the undergraduate level. Ney lived his life and conducted his research with an unconventional flair and frankness. Ney relentlessly sought truth and took delight in challenging authority and the conventional wisdom when he believed they were wrong. He spoke out forcefully, not only on scientific issues but also on his fellow scientists, the space program, his university, the nation's nuclear policy, and governance of the National Academy of Sciences.

THE EARLY YEARS

Ney was born on October 28, 1920, in Minneapolis, Minnesota, the son of Otto Fred and Jessamine Purdy Ney and was raised in Waukon, Iowa, a small farm town in the north-east corner of Iowa, 18 miles west of the Mississippi River. Ed's father was a stern disciplinarian who traveled frequently selling farm supplies. His mother was partially disabled by an attack of polio in her youth. She finished two years of junior college and taught kindergarten in Waukon. Their mother read to Ed and his younger sister Nancy, nurturing their curiosity and love of learning. In the eighth grade, Ed and a friend started and produced a school newspaper. By the time he reached high school, according to his sister Nancy, Ed had developed a strong interest in science, math, and girls. The local high school had a well-rounded science curriculum, which provided Ed with courses in general science, zoology, chemistry, and physics. He was especially influenced by Howard Moffitt, who taught several of his courses and later became an administrator at the University of Iowa.

Ed was disdainful of other courses and confined his reading mainly to science and to his special hobby, photography. Nevertheless, he developed a mastery of the English language that was reflected in his writing, in his ability to spot grammatical and spelling errors in his students' theses, and especially, in the edicts and pronouncements that came down from Minnesota's deans and higher authorities.

One of Ney's early clashes with authority and conventional wisdom occurred in high school. His focus on physics, math, and chemistry to the apparent detriment of his other more routine studies did not endear him to the principal, who informed Ney that “nobody who ever graduated from Waukon High School has ever done anything in science and neither will you.” Ney vowed to prove him wrong.

In 1938, at the age of eighteen, Ney entered the University of Minnesota. In 1940 he took a class from Alfred O.C. Nier. During the course, he asked if he could play around with one of Nier's oscilloscopes. Ney's ability and enthusiasm so impressed Nier that he hired the twenty-year-old Ney to make mass spectrometer measurements of carbon dioxide samples, in which the ratio of $^{13}C/^{12}C$ had been increased by passage of the CO_2 through a thermal diffusion column set up in an abandoned elevator shaft. Nier, ever careful of the use of his research funds, paid Ney thirty-five cents per hour. At a time when the use of radioactive tracers was just beginning, chemists and biologists used the ^{13}C-rich CO_2 for metabolism studies and as a tracer for photosynthesis.

John Bardeen, then a young assistant professor at Minnesota, theoretically calculated the expected enrichment from the thermal diffusion column. As Ed described it in his notes:

Bardeen's calculation and my measurement disagreed by a factor of two. We were getting too much ^{13}C. Bardeen shook his head and went back to the office for a week to try again. This calculation was not elegant physics like the BCS theory of superconductivity. It involved convection and messy assumptions. However, John was convinced there was a problem and finally Al Nier asked me to describe the power measurement. The columns were fed by an autotransformer and, when I measured the power to one, I had disconnected the other. Bardeen was right, and I took an electrical engineering course. The primary lesson was that John Bardeen was one smart guy. After World War II he went to Bell Labs and won his first Nobel Prize.

At the very beginning of the U.S. program to develop the atomic bomb, Al Nier had produced a 0.1-microgram separated sample of ^{235}U and ^{238}U, which had been used to show that ^{235}U was the isotope that underwent slow neutron fission as predicted by Bohr and Wheeler. In mid-1940, Nier was asked by the Uranium Committee to separate a 5-microgram sample of ^{235}U for the determination of nuclear cross-sections and neutron production rates. Nier designed a new mass spectrometer, which Ney and another undergraduate, Robert Thompson, kept going twenty-four hours a day for three months to produce the required sample. Nier then undertook the design and development of a special mass spectrometer for the analysis of processed UF_6. With the assistance of Ney, Mark Inghram, and the department's machine shop they produced three instruments in six months. These were to become the key assay instruments used by the Manhattan Project to measure the enrichment of uranium produced by the different separation methods then under development at Oak Ridge, Columbia, the Naval Research Laboratory, and the University of Virginia. Out of this collaboration, Alfred O.C.Nier and Edward P.Ney formed a very close personal and professional friendship that lasted until Nier's death in 1995.

On June 20, 1942, just after graduating from the University of Minnesota with a bachelor of science degree in physics, Ney married June Felsing. June had caught Ed's attention at a dance during their undergraduate years at the university. When she mentioned that she was taking a physics course taught by the department chairman, Ed asked her what her grade was. Hearing “A,” Ed was impressed, but skeptical. Ever the careful scientist, he checked her record, found she had indeed gotten an “A,” and promptly began courting. Their union lasted until his death and produced daughter Judy and sons John, Arthur, and William.

Shortly after their marriage, the Neys moved from Minneapolis to Charlottesville, Virginia. Ney took along one of Nier's new mass spectrometers and became the mass spectrometer specialist assigned by the Manhattan Project to work with Jesse Beams at the University of Virginia. He also enrolled as a graduate student in the Physics Department. Beam's group, a part of the Manhattan Project, was investigating the feasibility of using centrifuges to enrich uranium. Ney used the assay instrument developed at Minnesota to analyze the uranium samples produced by centrifuging at Virginia and also those produced by thermal diffusion at the Naval Research Laboratory. The Virginia centrifuges were very promising as pilot models, and Standard Oil of New Jersey began developing a production facility. Because this effort did not go well, General Groves came to Charlottesville in 1944 and closed down the Virginia program. (Many years later centrifuging became the most energy-efficient way to produce moderate amounts of enriched uranium). Ed then worked with his Virginia colleagues developing circuits and systems for gun control on naval ships and the guidance of small missiles. He maintained a lifelong interest in and concern about nuclear weapons.

For his Ph.D. thesis, which was classified, Ney measured the self-diffusion coefficient of UF_6. This constant was an important number, because knowledge of it, as well as the self-diffusion coefficient and the viscosity, determines the molecular force law and predicts the thermal diffusion co efficient. It turned out that UF_6 molecules have an inverse fifth power law of force for which the thermal diffusion coefficient is zero.

In 1944 their daughter Judy was born and two years later their son John. In 1946 the University of Virginia awarded Ney a Ph.D. Four years earlier, Ney had arrived in Virginia, a self-assured new college graduate who appeared much younger than his twenty-one years but whose research experience already extended beyond that of many fresh Ph.D.s. In only four years, he had worked more than full-time on the Manhattan Project and other defense-related efforts, juggled graduate courses, written a Ph.D. thesis, and won the admiration and respect of his colleagues at Virginia and the Naval Research Laboratory. The University of Virginia asked him to join their faculty as an assistant professor.

THE COSMIC RAY AND SKYHOOK BALLOON ERA

In 1946, with the end of the war, it was time to seek new research frontiers. Ney taught a course based on Heisenberg's book *Cosmic Radiation* and decided to shift his field of research from mass spectroscopy to cosmic-ray studies. Ney, Jesse Beams, and Leland Swoddy began an underground experiment in the Endless Caverns near New Market, Virginia. While waiting to get substantial results, Ney wrote a theoretical paper on the cascade component of cosmic radiation. According to Ney, the paper, while not very profound, caught the eye of John T.Tate, editor of *Physical Review* and professor of physics at the University of Minnesota. Tate was looking for bright young physicists to start a project to study cosmic rays with the aid of large plastic balloons invented by Jean Piccard and manufactured by the General Mills Research Laboratories in Minneapolis. Tate offered Ney an assistant professorship at the University of

Minnesota, which Ney promptly accepted. Although there is no documentary evidence, Ed's mentor Alfred O.C.Nier probably played a role in the return of his young prodigy. In 1947 the Neys and their two children returned to Minneapolis. Except for a sabbatical in Australia and two one-quarter leaves of absence, Ney spent the rest of his life at the University of Minnesota.

In 1947 Ney, together with Ed Lofgren and Frank Oppenheimer, formed the Minnesota cosmic-ray group and began to use nuclear emulsions and cloud chambers for studies of cosmic rays. Soon, Phyllis Freier joined the team as a graduate student. Together they pioneered the use of balloon-borne cloud chambers and nuclear emulsions. For the first time it became possible to study the nature of the primary cosmic rays at the top of the atmosphere. This effort paid off in 1948, when, in a joint balloon flight with Bernard Peters and Helmut L.Bradt of the University of Rochester, they discovered “heavy” particles in the cosmic radiation. Their data showed that cosmic rays are not electromagnetic radiation at all. Instead, they are high-energy nuclei of the elements stripped of their electrons. When astrophysicists found that the primary cosmic radiation consisted of elements from hydrogen through iron and that their relative abundances were similar to those deduced from astrophysical studies, they realized the studies of cosmic radiation could play a major role in astrophysics, as well as in understanding the origin and transport of energetic particles in the galaxy. Fifty years later it remains a very active research field.

Shortly after this major discovery, there were significant changes of personnel in the original cosmic-ray group. First, Lofgren left to supervise the construction of the Berkeley bevatron. Next, the university forced Oppenheimer to resign, because he had concealed his pre-war membership in the Communist Party. From 1949 through 1962, Ney led the cosmic-ray group. In 1949 John R.Winckler joined the Physics Department. Previously he had been at Princeton, where he had been carrying out cosmic-ray studies at balloon altitudes. The university appointed Ney associate professor in 1950. A second son, Arthur, was born on September 10, 1951, and a third, William, on August 9, 1952. In 1955 Ney became a full professor.

Meanwhile, Charles Critchfield, a theoretical physicist at Minnesota, became concerned about the lack of electrons in the cosmic radiation. He noted that if all the particles were positively charged then the Sun itself should charge up in about a year and repel the positive cosmic rays. As we know today, this idea is incorrect, but it stimulated Ney and Sophia Oleksa, a graduate student, to conduct a series of cloud chamber flights using both horizontal and vertical lead plates to try to measure the flux of electrons. Although they did observe electron showers, the number of events they observed could be explained as the result of the decay of pi mesons produced in the material above the chamber and their subsequent decay into gamma rays. Although Ney and Oleksa did not detect electrons in the primary radiation, they did set an upper limit on the electron flux of about 1% of the primary particles with energies above 1 GeV. Ten years later, when James Earl and Peter Meyer independently measured the flux of primary electrons they found the flux to be only slightly below the limit set by Critchfield, Ney, and Oleksa.

In 1950 Ney shifted from cloud chambers to scintillation counters and made one of the first measurements of the abundance of the elements using a scintillation counter. Shortly thereafter Ney and other cosmic-ray physicists became frustrated with a number of unexplained failures of large plastic balloons. In one celebrated case, a graduate student's payload separated from its parachute, free fell from high altitude, and crashed through the roof of an Iowa farmhouse. As a result, Ney, Winckler, and Critchfield undertook a high-priority, classified military project, supported jointly by the Army, Navy, and Air Force, to improve performance of high-altitude balloons and to develop a system that could photograph military installations in the Soviet Union. Ultimately, this became a multimillion-dollar project involving some thirty-five people. In late 1955, after the development of the U2 aircraft, the Air Force and subsequently the Army abruptly withdrew their support, since they no longer needed balloon-borne surveillance. In August 1956 the project closed down. A number of techniques developed in this research program, such as the duct appendix, super-pressure tetroon, and the natural shape balloon, continue to be used for cosmic-ray and atmospheric research, both here and abroad. Funding from the Office of Naval Research continued, making it possible for Ney and his graduate students to conduct an extensive atmospheric research program that resulted in eight Ph.D. theses. As Ney observed in his research notes, this return to science was a blessing that led to many significant developments:

John Kroening studied atmospheric small ions, invented a chemiluminescent ozone detector, and did a seminal study of atmospheric ozone. John Gergen designed the “black ball” and studied atmospheric radiation balance, culminating in a national series of radiation soundings in which a majority of the weather bureau stations took part. Jim Rosen studied aerosols with an optical coincidence counter, which was so good it still has not been improved; he was the first to discover thin laminar layers of dust in the stratosphere and to identify the source as volcanic eruptions. Ted Pepin participated in photographic observations from balloon platforms, and has subsequently carried this interest further with optical observations of the Earth's limb from satellites.

As the balloon project wound down, Ney also began to work more closely with the emulsion group, which at the time consisted of Phyllis Freier and several graduate students. Under Ney's leadership, Peter Fowler of the Bristol emulsion group spent the 1956–57 academic year at Minnesota. In a joint effort, the two groups systematically measured the flux of alpha particles as a function of energy and found that it reached a maximum at about 300 MeV/ nucleon. Later, during the International Geophysical Year, Ney, Winckler, and Freier applied techniques developed in the balloon program to keep a balloon in the air continuously during a period of intense solar activity. They observed protons from the Sun during several solar flares. In November 1960, during a giant solar flare, the Minnesota group measured a flux of solar protons that exceeded the normal cosmic flux by a factor of 10,000. An astronaut in space beyond the magnetosphere would have received an exposure of about 60 roentgens, or about a tenth of the lethal dose. Observing that the flux of galactic cosmic rays increased by a factor of three during the period of minimum solar activity, Ney proposed that this variation would lead to a variation in the ion density in the atmosphere and that this might prove to be a connection between solar activity and the weather.

Still in search of the elusive electrons in cosmic radiation, Ney and Paul Kellogg, a theoretical physicist at Minnesota, proposed that an appreciable fraction of the visible light in the solar corona came from synchrotron radiation of high-energy electrons spiraling about solar magnetic lines of force. Their theory predicted a non-radial component of polarization in the light of the corona. They set out to check their theory during the 1959 eclipse of the Sun. First there were formidable logistics problems to solve. In a little over two years, they prepared a proposal, obtained funding of $60,000, built three instruments, and flew from Minne apolis to French West Africa in an ancient DC4. There they set up instruments at three sites along the path of the eclipse. During the eclipse, good data were obtained at two sites, but the third was clouded over. The measurements disproved Ney and Kellogg's theory, for they showed that the light of the corona came from Thomson scattering as postulated by solar physicists, not from synchrotron radiation. Although this result was a disappointment, the work led to the development of cameras and polarimeters that Ney and his students later used to study dim, diffuse sources of light. Two decades later, Ney participated in another eclipse expedition to observe the solar corona in the infrared. Later he commented on the differences between the two expeditions:

Although the overall support for science was less, then it was possible for a university group to conceive and carry out an expedition. In 1980 we participated in the National Science Foundation's expedition to observe the eclipse in India. It was like a Boy Scout outing with administrators and managers, and even a doctor. But it wasn't much fun, and it cost a lot more.

The coronal experiment stimulated Ney's interest in dim, diffuse sources of light in astrophysics. He undertook to understand the origin and nature of the zodiacal light. He and his students flew cameras and polarimeters, developed for the coronal experiment, on balloons, *Mercury* and *Gemini* flights, and two orbiting solar observatories. These flights showed that the zodiacal light was highly polarized, of constant or slowly varying intensity, and that it was produced by the scattering of sunlight from dust grains. As the first scientist to fly an experiment on a NASA manned space flight program, Ney spent a good deal of time briefing the astronauts in the Moorhead Planetarium in Chapel Hill. Ney found it fun to get to know the astronauts, but he thought conducting research on a manned spacecraft a hard way to do science.

Although Ney designed instruments for an Orbiting Solar Observatory to study the zodiacal light during the portion of the orbit when the satellite was in the dark, the instruments could also be used to study light sources on Earth. Ney obtained a completely unexpected result when he observed thousands of terrestrial lightning flashes and found that there were ten times as many flashes over the land as over the ocean. As yet, no satisfactory explanation of this observation exists.

In 1963 Ney decided to change from physics to astronomy. He presented his final paper on cosmic rays at the Pontifical Academy in Rome while on his way to Australia to study astronomy with Hanbury Brown and Richard Twiss. Upon his return to Minnesota, in collaboration with two graduate students, Fred Gillette and Wayne Stein, Ney entered the emerging field of infrared astronomy, a field suitable to his pioneering instinct. At that time, there were only two infrared astronomers, Frank Low, then at Rice University, and Gerry Neugebauer at Caltech. With good students, a highly qualified support group, and his own exceptional physical insight and great experimental skills, Ney soon had Minnesota at the frontier of this new research field. To make infrared observations, he founded the O'Brien Observatory and equipped it with a 30-inch infrared telescope. Later, he helped to design the 60-inch infrared telescope for the Mount Lemmon Observing Facility in Arizona. As a result of their infrared observations, Ney and Nick Woolf showed that silicate and carbon grains form circumstellar shells around aging stars. As Ney noted at the time, in a cosmology dominated by hydrogen and helium, it was a relief to find the source of the material that forms the terrestrial planets.

After his retirement, Ney took up yet another field of research: the effect of radioactivity from radon gas on the atmosphere. He thought that the ionization from radon would produce a higher level of ionization in the atmosphere over land that could account for the higher levels of lightning over land, compared to those over the ocean. Unfortunately, Ney's death prevented the completion of this work.

Ney loved to teach. He had a special gift for using novel demonstration equipment to illustrate physics. His award-winning, animated lectures, liberally laced with hilarious wisecracks and anecdotes, gave thousands of students in his introductory courses the opportunity to experience the excitement of working at the cutting edge of science. Beneath the wisecracks and the jokes, students found a man with an absolute, steely insistence on honesty in academic and research work.

In 1961 he lectured in the department's first honors course in modern physics. These lectures were turned into “Ney's Notes on Relativity.” The next year he contracted hepatitis on a trip abroad. Instead of quietly recuperating, he used the time at home to turn these notes into a book, *Electromagnetism and Relativity* (New York: Harper and Rowe, 1962). He received the University of Minnesota's outstanding teaching award in 1964.

Ney's enthusiasm and charisma attracted good graduate students to his program. He encouraged them to select their own thesis topic and to conduct their research with a minimum of direction from him. He believed this produced a better and more mature Ph.D. Sixteen students received their Ph.D. under Ney. His methods produced high-quality students. Twice, the position of NASA chief scientist was filled by former students of Ney. Another student helped establish the Stratoscope Program at Princeton, and two students constructed one of the world's largest infrared telescopes at Jelm Mountain, Wyoming. One former student is a member of the National Academy of Sciences. Ney's career demonstrates that great research scientists can, and do, like to teach.

A major reason for Ney's success lay in his ability to attract, stimulate, and direct superb engineers and technicians. He made them full partners in his research and when they contributed in a substantial way to a project, he included them as co-authors of the resulting papers.

Ney's interest and concern extended beyond research. He took an activist role in campus politics. He believed that the students, staff, and faculty were the heart of a strong university and that the administration should serve their interests. He also believed in rigorous academic standards. Once, when invited to serve as the “outside professor” on a Ph.D. final exam, Ney considered the thesis topic to be trivial, not worthy of a Ph.D., and he refused to approve the thesis. Ney then severely criticized the professor who had approved the topic and supervised the work.

Ney did not limit his contributions to the academic arena. As a citizen he maintained a lifelong concern about the impact of science on public policy. He frequently contributed letters and articles to local editorial pages on atomic energy, nuclear weapons, the space program, and the environment. In later years he became deeply concerned about the proliferation of nuclear weapons and the possibility of their use by terrorists.

Edward P.Ney was elected to the National Academy of Sciences in 1971 and the American Academy of Arts and Sciences in 1979. In 1975 NASA awarded him its Exceptional Scientific Achievement Medal. In 1964 the University of Minnesota awarded him the university's Outstanding Teaching Award. Subsequently, in 1974 the university bestowed on him its highest honor, a Regent's Professorship.

As unconventional in his dress as in his work, Ney's red, high-top tennis shoes graced many a formal function, including a National Academy of Sciences garden party and black-tie dinner. Ney liked good cars and he liked to drive them fast. In the late 1940s he chased balloons in a low, black, streamlined Hudson that cruised at 90 miles per hour. He next bought a convertible that traveled even faster. Later, with proceeds from the sale of his book *Relativity and Electromagnetism,* Ney bought his ultimate automobile, a powder-blue Jaguar XKE. Returning from a night's observing at the O'Brien Observatory, Ney conducted an experiment to see how large a fine he would get if he exceeded the 65-miles-per-hour speed limit by a factor of two. Unfortunately, the experiment provided a null result; the Minnesota State Highway Patrol failed to appear.

In his later years, Ney suffered from ventricular tachycardia, a condition in which the ventricles of the heart contract at a high frequency and which can cause death in a short time. Frustrated with his doctor's inability to control the arrhythmia, Ney began to study cardiology. He turned the full force of his research talent on himself and his disease. He searched the literature, and became convinced that the best way to control the disease required a pacemaker that could be commanded to send pulses to the heart at a higher rate than the tachycardia. This action enabled the pacemaker to capture the rhythm of the heart so that, when the doctor slowed the frequency of the pacemaker's pulses, it brought the heart back to its normal rhythm. Ney's last (unpublished) paper, “A Physicist's History of Pacing and Shocking in the Treatment of Recurrent Sustained Monomorphic Ventricular Tachycardia, 1975–1995,” gave the history of his illness and documented the results of his research. Ney's last battle with authority was with his cardiologist. Ney wanted to carry the “black box” that controlled the defibrillator with him so that if he had an attack of ven tricular tachycardia, he could send the defibrillation command. Conventional wisdom held that the patient must be brought to a hospital for treatment by a registered cardiologist. Unfortunately, authority and conventional wisdom finally won a battle with Edward P.Ney. He died at his home in Minneapolis on July 9, 1996.

He is survived by June, his wife of fifty-four years, and their four children Judy, John, Arthur, and William, a sister Nancy Braum of Atlanta, and nine grandchildren. On July 16, 1996, several hundred people, including friends, family, his cardiologist, colleagues, and former students attended a joyful memorial celebration in his honor at the University of Minnesota.

SELECTED BIBLIOGRAPHY

1945 The power spectrum of the cosmic-ray cascade component. *Phys. Rev.* 70:221–21.

1947 With P.C.Armistead. A self-diffusion coefficient of uranium hexaflouride. *Phys. Rev.* 71:14–19.

1948 With others. Evidence for heavy nuclei in the primary cosmic radiation. *Phys. Rev.* 74:213–17.

1949 With E.J.Lofgren and F.Oppenheimer. Apparatus for cloud-chamber investigations with free balloons. *Rev. Sci. Instrum.* 20:48–51.

With F.Oppenheimer. Wide angle sprays of minimum ionization particles. *Phys. Rev.* 76:1418–19.

1950 With P.Freier. Multiple production of mesons. *Phys. Rev.* 70:337–41.

With C.L.Critchfield and S.Oleksa. The electrons in cosmic rays. *Phys. Rev.* 79:402–403.

1951 With D.M.Thon. A scintillation counter measurement of heavy nuclei. *Phys. Rev.* 81:1069–70.

1956 With W.Elsasser and J.R.Winckler. Cosmic-ray intensity and geomagnetism. *Nature* 178:1226–27.

1957 With others. The low energy end of the cosmic-ray spectrum of alpha-particles. *Philos. Mag.* 2:157–75.

1958 With P.Freier and P.H.Fowler. Cosmic rays and the sun-spot cycle: The primary alpha-particle intensity at sunspot maximum. *Nature* 181:1317–21.
1959 Cosmic radiation and the weather. *Nature* 183:451–52.
With P.J.Kellogg and J.R.Winckler. Geophysical effects associated with high-altitude explosions. *Nature* 183:358–61.
With P.S.Freier and J.R.Winckler. Balloon Observations of solar cosmic rays on March 26, 1958. *J. Geophys. Res.* 64:685–88.
1962 With W.A.Stein. Solar protons, alpha particles and heavy nuclei in November 1960. *J. Geophys. Res.* 67:2087–2105.
1964 With F.C.Gillett and W.A.Stein. Observations of the solar corona from the limb of the Sun to the zodiacal light, July 20, 1963. *Ap. J.* 140:292–305.
1969 With N.J.Woolf. Circumstellar infrared emission from cool stars. *Ap. J. Lett.* 155:L181–84.
With D.A.Allen. The infrared sources in the trapezium region of M42. *Ap. J. Lett.* 155:L193–95.
1970 With R.W.Maas and N.J.Woolf. The 10-micron peak of comet Bennett 1969i. *Ap. J. Lett.* 160:L101–104.
With J.A.Vorpahl and J.G.Sparrow. Satellite observations of lightning. *Science* 169:860–62.
1972 With J.G.Sparrow. Zodiacal light observations from the ecliptic to the poles. *Ap. J.* 174:705–16.
1974 Infrared observations of comet Kohoutek near perihelion. *Ap. J. Lett.* 189:L141–43.

1975 With others. Studies of the infrared source CRL 2688. *Ap. J. Lett.* 198:L129–34.
1978 With B.F.Hatfield. The isothermal dust condensation of Nova Vulpeculae 1976. *Ap. J. Lett.* 219:L111–15.
1982 Optical and infrared observations of bright comets. In *Comets,* ed. L.L.Wilkening, pp. 323–39. Tucson: University of Arizona Press.
1987 With R.S.Lively. Surface radioactivity resulting from the deposition of Rn daughter products. *Health Phys.* 52(4):411–15.
1990 With R.D.Gehrz. Confirmation of dust condensation in the ejecta of supernova 1987a. *Proc. Natl. Acad. Sci. U.S.A.* 87:4354–57.
1992 With R.D.Gehrz. 0.7 to 2.3 micron photometric measurements of P/Halley 1986 III and six recent bright comets. *Icarus* 100:162– 86.
1995 With R.D.Gehrz, C.H.Johnson, and S.D.Magnuson. Infrared observations of an outburst of small dust grains from the nucleus of comet P/Halley 1986 III at perihelion. *Icarus* 113:129–33.

CHRISTIAN HEINRICH FRIEDRICH PETERS

September 19, 1813–July 18, 1890

BY WILLIAM SHEEHAN

IN THE MID-NINETEENTH century the discovery of new asteroids was still far from routine. These objects had not yet grown so numerous as to earn for themselves the contemptuous label later applied, “vermin of the skies,” and those who excelled in claiming the starlike wanderers from the camouflage of background stars were honored with renown. Hind, de Gasparis, Goldschmidt, Chacornac, Pogson, and Peters were foremost among the early discoverers. Even on this short list C.H.F.Peters stood out.

On May 29, 1861—just weeks after the American Civil War began at Fort Sumter—Peters discovered his first asteroid (72 Feronia). It was the fifth asteroid discovered in North America (others had been found by Ferguson and Searle). Feronia was the first of forty-eight such discoveries that made Peters the most prolific finder of minor planets of his generation, and even today he remains second only to Johann Palisa among visual discoverers of asteroids. During his colorful career, he also compiled meticulous star charts of the zodiac, collated observations from manuscripts of Ptolemy, and embroiled himself in a series of often bitter controversies with other astronomers, notably over the existence of an intra-Mercurial planet.

EARLY CAREER

The son of a clergyman, Peters was born on September 19, 1813, at Coldenbüttel in Schleswig (then a duchy of the Danish crown, now part of Schleswig-Holstein, Germany). He studied mathematics and astronomy under J.F.Encke at the University of Berlin, and received his doctorate at twenty-three. After unsuccessfully applying for work at the Copenhagen Observatory, he went to Göttingen, famous for its association with the mathematician Carl Friedrich Gauss. As a very young man, Gauss had devised methods for calculating the orbits of asteroids from observations covering only short arcs of their apparent motion, methods first applied to the recovery of the asteroid Ceres serendipitously discovered by a Sicilian priest, Guiseppe Piazzi, at Palermo on January 1, 1801. Piazzi's discovery would prove to be one of the great achievements of the century: Ceres was the first of the horde of small planets discovered between Mars and Jupiter.

Young Peters pursued his studies under Gauss, but his chief association at Göttingen was with a young geologist, Sartorius von Walterhausen, with whom he traveled to Sicily. There he and Walterhausen commenced a detailed exploration of Etna, the famous Sicilian volcano. They also laid out a meridian line in the great church of St. Nicolò l'Arena—it is very artistic, with mythological figures of the zodiacal constellations depicted in red stone.

As a result of these efforts, Peters was asked to take charge of a new observatory then being planned in Sicily. The observatory, however, received no support from the Bourbon government—in the end, it was not actually established until 1879, when the observatory on Etna was built. Instead, Peters went to work for the Geodetic Survey of Sicily. At the same time he became a regular observer at the observatory of Capodimonte, Naples, and used its 3 1/2-inch refractor for a careful series of sunspot observations. Also, on June 26, 1846, he picked up a faint comet (1846 VI). Unfortunately, the orbit he worked out for this object was widely in error, and with the exception of a single independent sighting by Francesco de Vico at Rome, it was not observed again until 1982, when it was recovered by Malcolm Hartley with the 122-cm Schmidt telescope at Siding Spring, Australia.

Sicily in the 1840s was a seething place, a cauldron of popular discontent and on the verge of revolt. Since 1821, when Piazzi's patron Ferdinand I, with the aid of foreign troops, had scrapped the constitution he had reluctantly agreed to a year earlier, it had been a state governed by the police—“the most brutal and reckless set of individuals,” according to the Conservative Member of Parliament and future Prime Minister of England William Gladstone. The police were empowered to imprison a man without affording means of defense, to detain him year after year without trial, and even “to supervise all the actions and control of all the movements of those…who came under suspicion of being opposed to the regime.”

In 1848 the fall of the Orléans monarchy in France and the declaration of the Second Republic stirred the spirit of liberation all over Italy; there were revolutions in Florence and Milan, the latter led by a guerrilla leader who had made a name for himself in South America, Guiseppe Garibaldi. In Sicily, where Ferdinand II proved to be no less illiberal than Ferdinand I had been, there were also uprisings, sporadic attempts to wrest the island from the Kingdom of Naples. One of Peters's colleagues, Ernesto Capocci, the director of the Capodimonte Observatory, was enthusiastic about the revolution and, according to Peters, was “joyful that his four oldest sons” had been willing to accept the dangers of the cause by taking arms for Garibaldi. Pe ters also sided with the rebels; however, in the end the protest was thoroughly crushed, bombed into submission by Ferdinand's gunners. Peters was abruptly relieved of his post at the Geodetic Survey and escaped by English ship to Malta, but later claimed he returned to Sicily to help General Ladislaw Mieroslawski, a Polish soldier of fortune who had led rebellions in Poland and Germany, to fortify the towns of Catania and Messina.

Peters's tumultuous Sicilian adventure came to an end in May 1849, when the Bourbon troops of General Filangieri occupied the island. Peters fled to France. After briefly recouping, he made his way to Constantinople (now Istanbul). On his arrival he had only enough money in his pocket to buy breakfast or a cigar—he chose the cigar!

Peters was a remarkable linguist, fluent in modern European languages and also in Greek, Latin, Hebrew, Arabic, Persian, and Turkish (he once published a scientific paper in Turkish, an achievement few European scientists could boast). In Constantinople he became scientific adviser to Reshid Pasha, Grand Vizier of Sultan Abdul-Mejid II. The sultan had recently acquired a fine 11-inch refractor, and Reshid Pasha was inclined to place it at Peters's disposal. However, according to a newspaper clipping from the time, “Reshid Pasha's power and protection were not sufficient to overcome the antagonistic influences within the palace, nor could astronomical science, which would not stoop to rule the planets, prevail against the astrologers.” The sultan also discussed with Peters the possibility of his leading a scientific expedition to Syria and Palestine; but in 1854 the Crimean War broke out, and the plan was abandoned.

TO AMERICA

Acting on a suggestion by George Marsh, the American ambassador to Turkey, and armed with a letter of recom mendation from Alexander von Humboldt, Peters set sail for America in 1854. He immediately paid a visit to the Harvard College Observatory, where he met W.C. and G. P.Bond, and made the acquaintance of other leading American astronomers at the 1855 meeting of the American Association for the Advancement of Science at Providence, Rhode Island. He spoke on the sunspot observations he had made at Naples. His remarks formed the basis of a paper, “Contributions to the Atmospherology of the Sun,” which was published in the *Proceedings of the American Association for the Advancement of Science* (1855). Peters believed that the Sun was the scene of violent electrical storms, and cited various observations in support of this view. He also had been measuring for years the proper motions of sunspots. Since Galileo's time sunspots had held the key to the Sun's rotation, and Peters was well aware of the fact that sunspots always drifted toward the equator. He also noticed relative motions in longitude, far more considerable than those in latitude. “Whether there be a common motion,” he wrote, “and in what direction, cannot be decided in the present state of our knowledge of the Sun.”

DUDLEY OBSERVATORY

The AAAS meeting made Peters well known in America and won him a position on the staff of the U.S. Coast Survey in Washington, D.C. He became a protégé of the director of longitude determinations, Benjamin Apthorp Gould, Jr., and when Gould became scientific adviser of the Dudley Observatory in Albany, New York, Peters preceded him there as resident observer.

Dudley Observatory had been organized in the early 1850s when several prominent citizens of Albany, headed by Dr. J. H.Armsby and Thomas W.Olcott, approached Cincinnati astronomer Ormsby McKnight Mitchel for advice on found ing an observatory in their city. Mitchel was as well known for his popular lectures and believed strongly in fostering a general interest in the subject among educated laymen—he even founded a short-lived popular journal, the first such journal published in America until the founding of the *Sidereal Messenger* in 1882. Mitchel suggested that a sum of $25,000 would be sufficient for the building and the instruments, in order "to lay the groundwork upon which immediate action and consequent success could be built." His pronouncement persuaded the citizens of Albany that the project was within their means; a subscription, of which the largest portion was donated by the widow of the late Charles E.Dudley, was raised, land was donated, and the actual construction of a turreted dome got underway.

At the AAAS meeting in 1854, Peters argued for the purchase of a heliometer, an instrument with a divided objective used to accurately measure apparent diameters of the Sun. At the time there was no heliometer at the Coast Survey, which was by Act of Congress prevented from establishing an observatory of its own. The superintendent of the Coast Survey, Alexander Dallas Bache, endorsed Peters's recommendation and further proposed that in exchange for the Coast Survey's use of the heliometer, he would place instruments and observers from his own corps of government employees at Dudley's disposal. Thus the Albany concern became inextricably entangled with the Coast Survey; Mitchel withdrew his name from consideration, and Gould became presumptive director of the new observatory.

A scientific council, consisting of Bache, Gould, Smithsonian physicist Joseph Henry, and Harvard mathematician Benjamin Pierce, was appointed to provide advice to the Dudley Board of Trustees. Gould set out for Europe "with full authority to purchase a heliometer, a meridian circle, a transit instrument, a clock, and such other instru ments as he might think proper." He had been trained at Harvard and like Peters received a Göttingen Ph.D. He believed that science in America was in a backward condition, was ambitious to improve the situation, and intended for his observatory to become the leading American research institution of its time. However, the Dudley Observatory Board of Trustees had always envisaged a more public role for its observatory and had hoped for a facility that, in addition to producing results valuable to science, would serve as a means of "attracting, enlisting, and concentrating lovers and patrons of science." Inevitably, Gould and the board began to diverge sharply in their plans. As Simon Newcomb later observed, this "grew into a contest between the director and the trustees, exceeding in bitterness any I have ever known in the world of learning and even of politics."

In marked contrast to Gould, who when he was not in Europe was attempting to run the observatory by bulletins from his office in Cambridge, Peters arrived in Albany eager and ready to go to work, and impressed the trustees at once as a man of action. With one of the small instruments at the observatory he discovered, on July 25, 1857, a new comet, which he proposed to name for Olcott, the most prominent of the trustees. (The name was never officially adopted since by astronomical convention comets are named after their discoverers. Gould, however, at first wrote in support of Peters's initiative; "it is a very pretty idea," he wrote in a letter dated August 4.)

News of the discovery was "snapped up by the papers," and Peters, emerging as a hero who had produced results, immediately became the trustees' clear choice to run the observatory. Lines were drawn with Bache and Gould on one side, Peters and the trustees on the other. Bache, accusing Peters of "untrustworthiness," ordered his imme diate recall. One of the trustees in turn protested this attempt to "decapitate" Peters, and added: "The summary dismissal of such a man from such a position without a shadow of just reason, seems to be unprecedented and unwarrantable. He is a foreigner; but science knows no nationality. He is without social support or governmental patronage, but neither of these will secure the practical service which the observatory just now so much needs… He has slept at the feet of his instruments. In his own expressive language, 'the skies knew him.'" Under pressure from Bache and Gould, Peters resigned his position at the Coast Survey —it had paid only $540 per year, too little to live on. However, at the trustees' behest, he stayed on briefly in an apartment of Dudley Observatory, waiting like Dickens's Micawber for something better to turn up. (He may have still been there when a colleague, George Searle, discovered an asteroid at Dudley; the name, Pandora, was suggested by Mrs. Dudley after the woman in Greek myth who opened the box whence issued the multitude of evils that continue to afflict the human race; at the bottom of the box, only hope remained. Gould later quipped that the "apt significance" of the name would be obvious to all, under the troubled circumstances at the observatory.)

TO HAMILTON COLLEGE

In 1859 Gould gave up his long and bitter fight with the trustees (forced out, he said, by "hired ruffians"). By then, Peters had moved from Albany to Hamilton College, a small men's college in Clinton, New York (near Utica), where he had been named professor of astronomy. The college had just built a new observatory consisting of a two-story building capped with a 20-foot cylindrical dome. It housed a fine instrument, a 13 1/2-inch refractor, one of the largest in America at the time, built by Charles A.Spencer of Canastota, New York. However, financially Peters continued for some time to live on the ragged edge of existence. American astronomy was not well funded at the time. Thus Harvard's director George P. Bond wrote to Peters: “What you say of the financial prospects with which you begin the new year, nearly completes the list of twenty-five observatories started (not founded) within the past twenty years in the United States and left to die of want.” Peters's reply was dated February 1: “Lately for a day I was in Albany to speak with a lawyer about payment of my last year's salary. The trustees here, too, will find that there are ‘fighting' astronomers.” Already Peters had shown a marked attraction to the American propensity for litigiousness; his fighting instincts were aroused, and the rest of his career would be characterized by bitter controversies and legal proceedings.

At Hamilton College, Peters used the 13 1/2-inch refractor to plot sunspots by day and to search for new asteroids by night. His sunspot observations remained unpublished until long after his death (they eventually appeared as *Heliographic Positions of Sun Spots Observed at Hamilton College from 1869 to 1870* (1907). However, his asteroid discoveries won him immediate renown. His first discovery seems to have been inadvertent; he tracked down 72 Feronia while chasing another asteroid, 66 Maja, which had been found by H.P.Tuttle at Harvard. Peters added two more asteroids, 75 Eurydice and 77 Frigga, in 1862 and one each in 1865, 1866, and 1867. Impressed by this record, a Mr. Litchfield, a railroad magnate from nearby Delphi Falls guaranteed all the funds needed to cover the astronomer's modest yearly salary. The observatory was renamed the “Litchfield Observatory,” and Peters enjoyed the title “Litchfield professor of astronomy” and a modicum of financial security.

VULCAN CONTROVERSY

Peters's work as an asteroid discoverer led him to project a series of star charts to be inclusive of all the stars of the zodiac visible with an ocular magnifying 80x on that telescope. (Eventually, he would make some 100,000 zone observations in preparation of these charts.) His work as an asteroid discoverer also brought him into conflict with a younger rival, James Craig Watson, who in 1868 piqued Peters's intense competitiveness by discovering six asteroids— at the time an unprecedented feat.

It is not clear just when Peters began to form his keen dislike of Watson; keen dislike, however, it undoubtedly was. Peters was a lifelong bachelor. He was a man of great learning, a cosmopolitan, a man of the world, and a connoisseur of good cigars. He could be gruff, and was often misunderstood. No doubt he felt isolated at Hamilton College, and complained of his “solitary life.” There was little to distract him from his work. Though he never lost his strong distrust of the entrenched powers, he himself, ironically, became increasingly authoritarian and opinionated with age. He was also litigious in marked degree, intent both in astronomical journals and in the courts on defending his rights. Simon Newcomb, one of a number of astronomers who eventually fell out with Peters, wrote: “Of his personality it may be said that it was extremely agreeable so long as no important differences arose.”

With Watson, suffice it to say, important differences arose. Watson, like Peters, had begun to prepare his own zodiac star maps to assist his asteroid discovery work, and Peters resented an intrusion into realms that he regarded as his prerogative. Probably after so many hard-bitten years, he was also jealous of the junior astronomer's astonishingly rapid progress. Whatever the cause, there came to be something intensely personal in Peters's dislike of his younger rival. Moreover, not only were the two men rivals in asteroid discovery, they ended up vociferously on opposite sides of one of the most noisy scientific issues of the day—the vexed question of the existence of one or more intra-Mercurial planets.

The possibility of such planets had been endorsed by the leading theoretical astronomer, Urbain Jean Joseph Le Verrier of France. Already hailed for the brilliant prediction that had led to the discovery of Neptune at the Berlin Observatory in 1846, Le Verrier a few years later had turned his attention to the errant motion of the innermost planet. Finding a minute discrepancy (i.e., the perihelion of the planet's orbit was advancing slightly faster than predicted by Newtonian law) but unable to discover a strategy within Newtonian dynamics that would eliminate it, he introduced the Trojan horse of an unseen planet (possibly a zone of debris) lying closer to the Sun than Mercury. He announced his conclusion in September 1859; it was enthusiastically greeted as a prophetic utterance pointing the way to another world. Almost immediately he received the curious account of a country doctor, Lescarbault, alleging that the planet had already been observed by him in transit across the Sun's disk the preceding March. Le Verrier was non-plused; nonetheless, he visited Lescarbault's village of Orgères and interviewed the doctor himself. Thus he convinced himself of the truth of Lescarbault's account, and for the rest of his life remained convinced of the existence of the putative planet, which was named Vulcan after the Roman god of fire.

Unfortunately, Vulcan failed to show itself at its next predicted transit in March 1860; nor did it register an appearance at the July 1860 total eclipse in Spain. The astronomical world became sharply divided. Watson, whose work on theoretical astronomy Le Verrier had praised, was a promi nent supporter of his; Peters was a fierce opponent. Soon after he began work at the observatory in Naples, Peters had carried out an investigation of a colleague's claim of having seen a host of corpuscular bodies—they were presumed meteoric, possibly related to the May (Eta Aquarid) meteors—in quick passage across the Sun. After studying the "corpuscles," Peters was convinced that they were nothing more than flocks of migrating birds. Unimpressed by the records of Lescarbault and others who had reported fleeting objects upon the Sun's disk, most of which Peters believed were birds, he insisted on trusting only the records of experienced observers; Schwabe, the discoverer of the sunspot cycle, England's Richard Carrington, and, of course, himself, none of whom had ever seen a planetary object crossing the Sun.

Peters was present at an August 7, 1869, eclipse expedition to Des Moines, Iowa. Simon Newcomb suggested that Peters ought to join in the search for intra-Mercurial planets, but Peters replied he had come to observe the eclipse and added, with an allusion to his migrating-bird thesis, that he would "not go on a wild goose chase after Le Verrier's mythical birds."

THE TRANSIT TO VENUS

Peters again escaped provincial life at Hamilton College in 1874, when he traveled as chief of the U.S. expedition to New Zealand to observe the transit of Venus. The transit was the first since 1769, when Captain James Cook had sailed with the *Endeavour* to the South Seas, had observed the transit of Venus from Tahiti, and had gone on to map the coasts of New Zealand, Australia, and New Guinea. While Peters's ship was being loaded up in San Francisco for its long journey he wrote anxiously to make sure the expedition was being provisioned adequately: "Will you ask Lieu tenant Bass, if it is not too much trouble, to do me the favor to buy on my account some 4 or 500 of your 'Meravillas' [cigars], and to stuff them in the outside boxes of the Equatorial, or Transit, where I think there might be plenty of room for a few cigar boxes? The New Zealand sun will drive out what dampness they may receive on the sea."

Peters's observing station was near Queensland in the mountains of the South Island of New Zealand, an elevated situation that required the transport of telescopes and other supplies (including cigars) through valleys and across rivers. "The English parties sneered a little at us," Peters confessed; but in the end Peters was at least partially vindicated—as he usually was during his long astronomical career—since most of New Zealand lay under heavy cloud cover on transit day, December 8, 1874. Peters, on the high ground, was favored with at least short intervals in which the Sun "shot out from between the clouds," and succeeded in getting a good timing of the first internal contact of the planet with the Sun's disk. Peters returned to the United States by way of Sydney and Brisbane, passed through the Torres Strait and along the coast of Java, then to Batavia, Singapore, Hong King, Yokohama, and finally back to San Francisco. He had spent, in all, a full year "tumbling about in distant countries."

Almost at once on returning to Hamilton College, he opened the dome of the 13 1/2-inch refractor and discovered his twenty-first and twenty-second asteroids—both on the same night, June 3, 1875. He displayed his learning in classical literature in naming them Vibilia and Adeona after the Roman goddesses of journeyings and homecomings. They are not alone among Peters's asteroids in having unusual names; though many of his asteroids have classical names (Eurydice, Io, Iphigenia, Cassandra, Alceste), he also chose many names from Norse mythology, and even one from the Bible: Miriam, the name of one of Moses's sisters, was the name he gave an asteroid he discovered in 1868, apparently for no other reason than to irritate a colleague. At the time it was a strict rule that asteroids were to be named only for mythological, not real, personages; Peters's sole motive in breaching the rule was so he could tell a theological professor, “whom he thought too pious,” that Miriam was also a “mythological personage.” Peters did ever delight in pricking the bubble of pretentious colleagues.

VULCAN AGAIN

Meanwhile, the intra-Mercurial planet question rose again to the fore. Le Verrier died on September 23, 1877—the exact anniversary of the Neptune discovery. To the very end, he had never recanted his belief in Vulcan's existence. Instead he had published new calculations of the planet's orbit and predictions of possible transits, which rekindled the interest of sympathetic astronomers and hardened the skepticism of the unsympathetic. Carefully watched for the world over, the predicted transits were again devoid of result; no Vulcan appeared against the disk of the Sun.

The total eclipse of the Sun of July 29, 1878, was now awaited by astronomers with an almost panicked sense of urgency. It would be, in some ways, the best chance to scour the sky around the Sun for the elusive interloper: Vulcan's last stand. In the United States the path of totality swept from Yellowstone National Park and the Wind River Range in Wyoming Territory, down the front range of the Rockies through Boulder, Denver, and Pikes Peak, then across Oklahoma Indian Territory into Texas and Louisiana. Peters was invited to accompany the party of Edward S.Holden, then of the U.S. Naval Observatory, later of Lick Observatory, who was planning to observe from Virginia City, Montana Territory. “It is a great temptation,” Peters admitted, “…

but I ought not to go, unless the trustees [here] give me an assistant at the observatory—for which probably there is little hope. So, you go to Montana. Take care of not being scalped by the Indians."

Holden did change his plans, and observed the eclipse from Colorado. Simon Newcomb was dispatched to the railroad outpost of Separation, Wyoming, where he was joined by Watson. Peters's rival obtained the most spectacular results at the eclipse-he found a "ruddy star" between the Sun and *theta Cancri* that was not on the star maps, also another, even bright red star, farther to the east. Watson was convinced he had found one, possibly two Vulcans. The announcement electrified the astronomical world. Elsewhere only Lewis Swift, who had made a name for himself as a successful discoverer of comets and observer of nebulae, had seen anything unusual; from his station at Denver he too had made out two strange red stars. At first it seemed that his results agreed perfectly with Watson's. However, he had made a mistake, and on recalculation it turned out that Watson and Swift's positions could not be reconciled. If their reports were both accepted, there must be no less than four planets.

Into this territory of doubt, Peters rushed like an avenging angel. He had always regarded Vulcan as a "mythical bird"; now he was intent on demonstrating, once and for all, the insubstantiality of the ghost planet. (To his impartial interest in defining the truth was added the alluring motive of destroying his hated adversary Watson.) Fired with zeal for the project, he searched the byways of his retentive memory, drew deeply on a lifetime of reading in obscure and forgotten lore. His scholarly interests were wedded to the aggressive skills of a master prosecutor. Vulcan, that notorious fraud, stood in the dock, and must be convicted of imposing itself on the credulity of the astronomical world.

Peters's attack appeared in 1879 in *Astronomische Nachrichten.* It is, as Joseph Ashbrook noted, “a strange blend of sharp insight and utter tactlessness.” Peters quickly disposed of Swift's claim and launched his main attack on Watson. He was convinced that the Ann Arbor astronomer had overestimated his ability to measure the positions of his stars under the necessarily rushed and nerve-wracking conditions of a total eclipse, and his conclusion—which has never been disproved—was that Watson's “Vulcans” were simply the field stars *theta* and *zeta Cancri.*

STAR CATALOGS AND LAWSUITS

By now Peters was in a race against time to complete work to which he had devoted decades of effort. There were his zodiacal star charts, which he had drawn up to aid the detection of his asteroids. He had planned 182 charts in all covering the whole ecliptic. It was a heroic enterprise. The first twenty charts were published as *Celestial Charts Made at the Litchfield Observatory of Hamilton College* in 1882; but he never published the rest, since by then the whole project had been superannuated. The potential of dry-plate photography for star mapping had been realized. In 1887 Peters was among 57 astronomers from 11 countries to meet in Paris to develop a program of cataloging and mapping the entire sky by means of photography. The plan led to the *Carte du Ciel.*

Peters was elected a member of the National Academy of Sciences on April 19, 1876. He was by then planning a revised edition of Ptolemy's star catalog in the *Almagest,* which would involve the collation of existing manuscripts in the libraries of Europe. At the same time, or a little later, he began work on another massive compilation: the gathering together into a single volume all published observations of the comparison stars he used in measuring asteroids.

Naturally, both projects were larger than any man could possibly accomplish alone, especially an increasingly aged and querulous man (Peters was now well into middle age). An assistant, Jermain G.Porter, later director of the Cincinnati Observatory, briefly joined in the comparison-star compilation, but for a number of years the scheme languished. Finally Peters hired a more willing assistant, Charles A.Borst (Hamilton College class of 1881). At first Borst was trusted only with miscellaneous reductions, but from May 1884 he was employed on the compilation itself. By early 1888, Borst, with the aid of his sisters who had helped him carry out many of the calculations at home, had finished and submitted the manuscript to Peters with a title page indicating that it had been performed by Charles A.Borst under the direction of Christian H.F.Peters. According to Borst, Peters immediately became enraged, tore up the title page, threw the fragments into the stove, and shouted, “Bring me the catalog!”

Borst refused to do so, and Peters immediately initiated a suit *in replevin.* Peters hired as his counsel one of the most prominent lawyers in New York, Elihu Root (Hamilton College class of 1867), the son of Peters's close friend, Hamilton mathematician Oren Root. Borst chose for his counsel the law firm of an ex-senator of the United States, the Messrs. Kernan of Utica. Several astronomers, including Newcomb, suggested that the matter would be better submitted to arbitration by astronomers. However, Peters refused to compromise. In 1889 *Peters v. Borst* was heard before the Supreme Court of New York, Oneida County, presided over by Judge Williams. The “Great Star-Catalog Case” became a *cause célèbre,* and received coverage in the local newspapers. The judge—obviously bewildered by many of the technical details—eventually decided for Peters; but the newspapers sided with Borst, and so did many astronomers, including

Newcomb. (Apparently Peters and Newcomb never spoke to one another again.)

Undoubtedly the legal proceedings were an enormous strain on Peters. Up to this time he had remained healthy, active, energetic—his last asteroid discovery, 287 Nephthys, was found on August 25, 1889, when he was almost seventy-six years old. However, when the legal proceedings got underway, he grew preoccupied and depressed. Oren Root recalled that though Peters was still "clear-headed as ever," he was able to accomplish little after his return from Europe in 1887. "The Borst difficulty nearly broke his heart… besides depriving him of an assistant. [It] so preyed upon his mind that he had no wish to do anything…at times his enthusiasm for work showed, but until after the trial and decision his thought was almost entirely upon that." Not only did he fail to finish his great revision of Ptolemy's star catalog, his observing routine suffered; so, perhaps, did his health. Death was around the corner. "It is painful to think," Newcomb wrote, "that his death may have been accelerated by the annoyances growing out of the suit." On the morning of July 19, 1890, Peters was found lying, a half-burned cigar at his fingertips, on the doorstep of the building where he lodged; observing cap on his head, he had fallen in the line of duty, on the way to the observatory the night before.

The mill of legal proceedings ground on after his death (Borst's appeal to the New York Supreme Court was heard in September 1892; by a verdict of two to one, the Supreme Court in *Root v. Borst* upheld the earlier decision in favor of Peters. However, in April 1894, the Court of Appeals of New York reversed the judgment, upon deciding that improper evidence had been admitted, and granted a new trial. It never took place.)

More important was the fate of Peters's miscellaneous observations and compilations, especially his great work, the Ptolemy star catalog. It was finished by the English amateur E.B.Knobel. In this case, death forced collaboration.

Peters's death brought a sudden interruption to the routine of the Litchfield Observatory. His assistant Borst had of course been banished. Someone else would have to succeed Peters as director of the observatory. However, Oren Root noted, “the salary our trustees can offer is too meager to bring any but a younger man here and I've not yet found a young man in whom we can agree.” In the end, Peters's position remained unfilled; the deserted Litchfield Observatory was allowed to crumble and fall into disrepair; the instruments were packed and placed in storage, including the objective of the 13 1/2-inch refractor, and during World War I the building was finally torn down, only the granite pier on which the noble telescope being left to mark the place.

In other respects, Peters's legacy did not long survive him. The *Carte du Ciel* and other photographic surveys superseded his and all other visual observers' maps of the sky. Beginning with Max Wolf's discovery of 323 Brucia in 1891, the application of mass-production photographic methods to the search for minor planets trivialized the labor on which Peters had worn out his middle and late age. His forty-eight asteroids—including eight in one year, 1879-were quickly overwhelmed in the ensuing blizzard of discoveries.

Peters was severe and harsh as a teacher, and fostered no disciples. There is little doubt he possessed a violent temper. He was most in his element when censuring or pointing out the mistakes of other astronomers, who were seldom thankful for the correction. As a result, he made many enemies. By temperament he was an astronomical Jeremiah, “a man of strife and contention.”

He was also an astronomical pack rat, a hoarder of much curious, strange, and forgotten lore. His mind was well stocked with a lifetime of collecting, ransacking, rummaging, until it became an "olde curiositie shoppe," a flea market or astronomical rag-and-bone shop. But it all died with him. Had he been more generous with the knowledge he possessed, he might have contributed much more to astronomy than he did. Certainly he would have been more fondly remembered. Guilty of extreme jealousy and possessiveness that made him deem each fact that passed through his hands, each idea or hint of an idea, his and his alone, he sometimes forgot that facts have little value in themselves but only as they are made available for use and brought into relation with each other. Unfortunately, the data one hoards with diligence may not survive the attic that stores it; and so it may pass into neglect, or be recovered, perhaps, when no longer needed or of interest. There are treasures hidden in the deep blue sea, and flowers that waste their fragrance on the desert air.

For all his faults, Peters was undoubtedly a man of great dedication to his craft. He knew much, and was a rapid and highly accurate mathematical computer and a tireless seeker after the truth as he saw it. He died as he lived, intense, single-minded, engaged in his business, with his observing cap on head, cigar in hand—an enthusiast heading out under the stars.

AFTER PETERS'S DEATH Robert Simpson Woodward, Benjamin Boss, and Curtis L.Hemenway were assigned to his memoir, according to the Academy file forwarded to me by William Press. In finally completing it, I warmly acknowledge the help of Press, Donald E.Osterbrock, and Dorothy Schaumberg of the Shane archives of the Lick Observatory, Richard Baum, and Luigi Prestinenza.

REFERENCES

Ashbrook, J. 1984. *The Astronomical Scrapbook: Skywatchers, Pioneers, and Seekers in Astronomy*. Cambridge, Mass.: Sky Publishing Corp.

Baum, R., and W.Sheehan. 1997. *In Search of Planet Vulcan: The Ghost in Newton's Clockwork Universe*. New York: Plenum.

Hibbert, C. 1965. *Garibaldi and His Enemies.* London: Longmans.

Jones, B.Z., and L.G.Boyd. 1971. *The Harvard College Observatory: The First Four Directorships.* Cambridge, Mass.: Harvard University Press.

Knobel, E.B. Obituary notice. *Mon. Not. R. Astron. Soc.* 51(1890):199– 202.

Newcomb, S. 1903. *Reminiscences of an Astronomer.* Boston: Houghton-Mifflin.

Porter, J.G. Obituary notice. *Sidereal Mess.* 9:(1890):138–39.

Schmadel, L.D. 1992. *Dictionary of Minor Planet Names.* Berlin: Springer-Verlag.

Trustees of the Dudley Observatory. 1858. *The Dudley Observatory and the Scientific Council, Statement of the Trustees.* Albany, N.Y.: Van Benthuysen.

Warner, D.J. 1974. C.H.F.Peters. In *Dictionary of Scientific Biography,* vol. 10, ed. C.C.Gillispie, p. 543. New York: Charles Scribner's.

SELECTED BIBLIOGRAPHY

Most of Peters's publications are orbit calculations, observations, and positions of comets and asteroids, including the forty-eight asteroids he discovered, which appear mainly in the *Astronomische Nachrichten.* A list of his asteroid discoveries appears at the end of this memoir. In addition, his works include the following of more general interest.

1847 *Memoria sopra la nuova cometa periodica di 13 anni.* Napoli: Nel Gabinetto Bibliografico e Tipografico.

1856 Contributions to the atmospherology of the Sun. *Acad. Sci.* 9:85–97.

1869 Beitrag zur Kenntnis gewisser, an der Sonne voruberfligender. Korper. *Astron. Nach.* 74:29.

1877 Uber die Fehler des Ptolemaischen Sternverzeichnisses. *Vierteljahrsschrift Astronomische Gesellschaft.* Berlin: Astronomische Gesellschaft.

1879 Investigation of the evidence of a supposed trans-Neptunian planet in the Washington observations of 1850. *Astron. Nach.* 94:113–16.

Bemerkung zu Oppolzer's "Elemente des Vulcan." *Astron. Nach.* 94:303.

Some critical remarks on so-called intra-Mercurial planet observations. *Astron. Nach.* 94:321–40.

1882 *Celestial Charts Made at the Litchfield Observatory of Hamilton College.* Clinton, N.Y.

1886 Corrigenda in various star catalogues. Memoir XI. In *Memoirs of the National Academy of Sciences,* vol. 3, pp. 87–97. Washington, D.C.: U.S. Government Printing Office.

Flamsteed's stars. Memoir X. In *Memoirs of the National Academy of*

Sciences, vol. 3, pp. 69–83. Washington, D.C.: U.S. Government Printing Office.

1907 *Heliographic Positions of Sun Spots Observed at Hamilton College from 1860 to 1870.* Ed. E.B.Frost. Washington, B.C.: Carnegie Institution of Washington.

1915 With E.B.Knobel. *Ptolemy's Catalogue of Stars: A Revision of the Almagest.* Washington, D.C.: Carnegie Institution of Washington.

ASTEROIDS DISCOVERED BY C.H.F.PETERS

72	Feronia	May 29, 1861
75	Eurydice	September 22, 1862
77	Frigga	November 12, 1862
85	Io	September 19, 1865
88	Thisbe	June 15, 1866
92	Undina	July 7, 1867
98	Ianthe	April 18, 1868
102	Miriam	August 22, 1868
109	Felicitas	October 9, 1869
111	Ate	August 14, 1870
112	Iphigenia	September 9, 1870
114	Cassandra	July 23, 1871
116	Sirona	September 8, 1871
122	Gerda	July 31, 1872
123	Brunhild	July 31, 1872
124	Alceste	August 23, 1872
129	Antigone	February 5, 1873
130	Electra	February 17, 1873
131	Vala	May 24, 1873
135	Hertha	February 18, 1874
144	Vibilia	June 3, 1875
145	Adeona	June 3, 1875

160	Una	February 20, 1876
165	Loreley	August 9, 1876
166	Rhodope	August 15, 1876
167	Urda	August 28, 1876
176	Iduna	October 14, 1877
185	Eunice	March 1, 1878
188	Menippe	June 18, 1878
189	Phthia	September 9, 1878
190	Ismena	September 22, 1878
191	Kolga	September 30, 1878
194	Procne	March 21, 1879
196	Philomena	May 14, 1879
199	Byblis	July 9, 1879
200	Dynamene	July 27, 1879
202	Chryseis	September 11, 1879
203	Pompeia	September 25, 1879
206	Hersilia	October 13, 1879
209	Dido	October 22, 1879
213	Lilaea	February 17, 1880
234	Barbara	August 12, 1880
249	Use	August 16, 1883
259	Aletheia	June 28, 1886
261	Prymno	October 31, 1886
264	Libussa	December 17, 1886
270	Anahita	October 8, 1887
287	Nephthys	August 25, 1889

HOWARD ENSIGN SIMMONS, JR.

June 17, 1929–April 26, 1997

BY JOHN D.ROBERTS AND JOHN W.COLLETTE

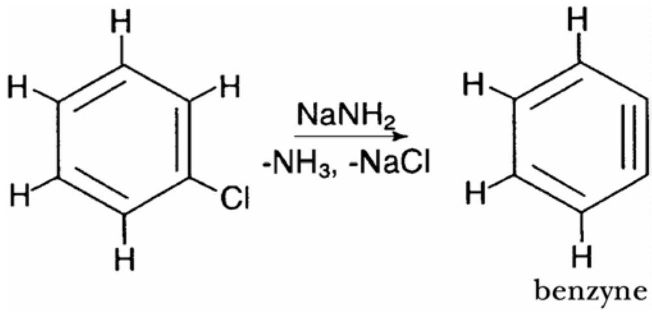
In the current period where almost all large U.S. corporations have downsized their research efforts and traded most of their long-term basic research for short-term gain at the bottom line, it is relevant to consider the scientific and management career, as well as the life, of one of the leading industrial research scientists of the twentieth century, who was deeply involved in the time of changing views of corporations toward basic research.

Howard Ensign Simmons, Jr., was born in Norfolk, Virginia, on June 17, 1929. His father and his uncles were sea captains, but his father did not encourage him to follow that career; instead, he helped with Howard's desire to study chemistry, starting at age twelve, by building him a small but well-used laboratory as an addition to their home. Howard's mother came from a family with a different bent. Her parents emigrated from Bavaria, and her father was an entomologist, possibly inspired by one of his forebears, Jacob Hübner (1761–1826), characterized as the first great lepidopterist, who compiled the first catalog of North American butterflies.

By his own account, Howard did not do too well in his early schooling until he encountered Latin and other languages, the study of which he found very interesting, and indeed became a lifelong avocation. He did so well at French in high school that he was offered a scholarship at the University of Virginia, but with his interest in science he decided to go to MIT. Howard saw MIT as a true Mecca of science with its aura of scientific and engineering prowess greatly enhanced by its success in war research. Howard entered MIT in 1947, joined and lived in a social fraternity, and enrolled in the ROTC. He started somewhat slowly academically, but was at the top of the chemistry class by the time he graduated.

In this period, MIT had a senior-thesis requirement and Howard did exemplary research in organic chemistry with J.D.Roberts on the mechanism of the reaction of silver salts of carboxylic acids with iodine. The head of the Chemistry Department, Arthur C.Cope,[1] was endeavoring to improve the quality and diversity of the organic chemistry graduate students and believed that the best way to do that was to require MIT undergraduates to go elsewhere for graduate work. On occasion, he was willing to relax this requirement and, in one particular period, three outstanding exceptions were E.J.Corey (Nobel Prize in chemistry, 1990), Robert H.Mazur (later the discoverer of NutraSweet), and Howard E.Simmons, Jr.

Simmons made a very successful start on his graduate thesis work in which he continued with J.D.Roberts on the chemistry of cyclobutenones. But that work was suspended so that Howard could investigate the intermediacy of benzyne (benzene minus two hydrogens, C_6H_4) by the removal of HCl from chlorobenzene with the strongly basic reagent, sodium amide. Howard played a crucial rule in this achievement, which is immortalized in almost every elementary textbook, not only as evidence for benzyne but also as a prime example of the use of isotopic labeling to establish a reaction mechanism.



When his thesis supervisor decided to move to a professorship at the California Institute of Technology, Howard elected to remain at MIT and complete his doctorate work with Arthur Cope on cyclooctane chemistry. In all, Howard's Ph.D. research resulted in six publications in quite diverse fields of organic chemistry.

Although he would have been a prime candidate for a postdoctoral fellowship or a junior academic position when he finished his Ph.D., Howard accepted a position in the Chemical Department (subsequently the Central Research Department) of the DuPont Company in 1954. This was during a period later referred to as the golden age of basic research at DuPont. At that time, the company had well-established businesses in fibers (nylon, Dacron®, Orlon®), films (cellophane, Mylar®), plastics (polyethylene, Teflon®), neoprene elastomer, tetraethyllead, automotive finishes, refrigerants, and many basic chemicals, as well as owning more than 20% of General Motors Corporation. The success of the research of Wallace H.Carothers[2] before World War II with nylon, along with the demonstrated utility of what had been considered purely academic research to the war effort, spawned a euphoria of what basic research could achieve at DuPont's industrial laboratories in the way of “Better things for better living through Chemistry.” In this period, well before the magnificent array of DuPont products became commonplace, the company was clearly ensconced in the public's mind as a provider of technological miracles.

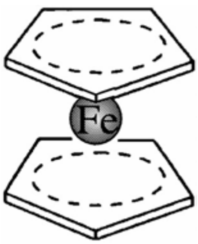
DuPont's research in the early 1950s when Simmons came aboard was by no means concentrated in the Central Research Department. Absolutely outstanding exploratory research was being carried out in the Organic Chemicals, Polychemicals, Explosives, Photo Products, Pigments, Textile Fibers, Agricultural Chemicals, and Electrochemicals Departments that were the mainstays of DuPont's businesses. And much of that research was published or presented at professional meetings.

The success of the research in the manufacturing departments in support of their businesses, along with an upper management decision to make Central Research independent of the operating departments, provided Central Research the opportunity to engage in many basic research programs that became the envy of the university chemical community. This is not to say that Central Research programs failed to be directed toward new businesses for DuPont. The objectives were in place, but a principal focus was taken on the development of new chemistry, rather than incremental improvement of existing processes.

The emphasis on entirely new chemistry was surely inspired by a number of extraordinary discoveries that shook up conventional ideas of what was possible in organic chemistry. One was the discovery in Germany of cyclic polymerization of acetylene to provide any desired quantity of cyclooctatetraene, a substance made earlier in very small amounts that turned out later to undergo a plethora of quite unexpected chemical transformations.

cyclooctatetraene

Another was the unusual properties found for highly fluorinated substances. Still another was the discovery in England of ferrocene and its wholly unanticipated properties. Ferrocene was later shown to have a structure with an iron atom sandwiched symmetrically between two five-membered carbon rings.



ferrocene

This latter finding led to an entirely new field of organometallic chemistry that has produced many new catalysts for diverse reactions of prime industrial utility. In this arena, DuPont played a very significant role.

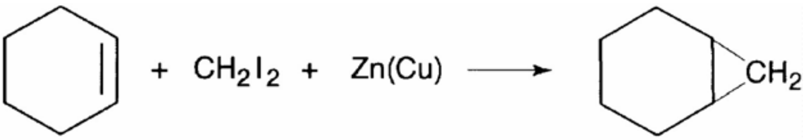
As we have said, the goal of DuPont's Central Research in this yeasty period was to produce new chemistry and new chemicals. Particular emphasis was to be placed on organic chemicals and, in one way, this was particularly appropriate, because before and until a few years after World War II, research in organic chemistry did not require substantial capital investments. This was true despite enormous growth in the knowledge and utility of organic chemistry, as well as the development of new reactions, because little change in the techniques available for characterization or handling of organic chemistry had occurred over the previous fifty years.

But, unforeseen and enormous changes were in the wind. One was the ongoing development of highly sophisticated techniques applicable to the analysis of organic compounds, such as visible and ultraviolet spectroscopy, gas- and liquid-phase chromatography, mass spectroscopy, routine crystallographic structural analyses, infrared and Raman spectroscopy, and, perhaps more than any other, nuclear magnetic resonance (NMR) spectroscopy.

Central Research, by virtue of its charter, sought to be in the forefront in investigating these techniques and demonstrating their value for industrial research. With NMR, DuPont played a major role in the development of its uses for analysis and structural characterization, primarily through the work of William D.Phillips.[3] Instruments of this character, while enormously increasing the efficiency of organic research, require investment of millions of dollars in technologies utilizing equipment that becomes essentially obsolete and needs to be upgraded or replaced in three-to five-year cycles, much in the same way as many of us continually encounter on a smaller scale with personal computers.

Additional research costs also resulted from greater emphasis on laboratory safety, an area in which DuPont has always maintained a leadership position, as well as changes in laboratory practices designed to meet societal concerns for emissions and disposal of laboratory wastes.

These and other problems were to confront Howard Simmons in his steadily upward career at the Central Research Department. However, early on, the focus was on synthesis and study of entirely new compositions of matter, although perhaps with some degree of overconfidence that profitable uses could be found for these materials as they became available. Right from the outset, Simmons developed new chemistry. His early work featured a keen interest in using highly reactive intermediates in synthesis. He had found a reference to the formation of a gas in the reaction of CH_2I_2 with zinc and investigated the possibility that the gas could be ethylene, $CH_2=CH_2$, formed by dimerization of unstable carbene, CH_2. A detailed study of the reaction led to a very general synthesis of cyclopropanes, derivatives of the three-membered carbon ring, $(CH_2)_3$, now universally known as the Simmons-Smith reaction. One of Howard's collaborators on studies of the mechanism of this reaction was E. P. ("Doc") Blanchard, who later became a vice-chairman of the company.



Simmons-Smith reaction

A later project involved further elucidation of the structure of benzyne and, to this end, he conducted trapping experiments of this very unstable intermediate that led him to conclude that benzyne is truly aromatic in the same sense as benzene, but has a very reactive multiple bond. This conclusion remains the prevailing view to this day, although much more is now known about benzyne, and even its NMR spectrum has been taken.[4]

A discussion of some elements of the culture of DuPont's Central Research Department in Howard's time is perhaps worthy of mention here. One still adhered to is that research chemists (often designed as "bench" chemists), with very few exceptions, carry on their work with the assistance of technicians possessing different levels of skill and experi ence. Normally, there is no way for chemists at this level to multiply their research output by having more than one technician. Promotion to research supervisor is necessary, and then the chemist acquires an office and a research group that works on the supervisor's ideas to quite varying degrees. Much depends on the research credentials of the supervisor, and in Howard's case they were very high. Many of those who came to CRD often decided that their chances for promotion and influence in the company were better if they transferred to an operating department, and this meant that the turnover was relatively high, which of course gave more opportunities for the department to search for and hire outstanding new Ph.D. recipients.[5] In recent years, transfers have become less common and, as a result, with the slowdown of hiring generally, the average age of the CRD personnel has risen considerably. At the same time, more technical responsibility for the direction of research has shifted to the chemist as the result of having to carry out short-term projects in support of the company's business units.

An open-door policy was also a feature of the Central Research Department culture when Simmons came. This meant that, even up to the research director, everyone's door was kept open at all times unless confidential matters were being discussed. It was a great policy for ready communication and good feelings, but not so satisfactory for contemplative study of difficult problems. Simmons made a conscious and successful decision on his own to change the culture. It was not altogether popular with the management, but he was so good that he made it stick. Despite this, all of his colleagues regarded him as being very accessible and helpful with their problems.

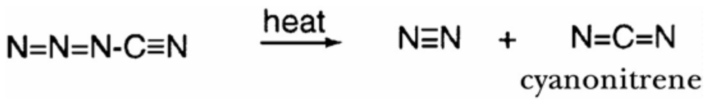
Another consideration for Central Research was publication policy. It is customary for industrial research laborato ries to be wary of publication in the open literature, and many in DuPont opposed CRD's quite vigorous policy of displaying its technical achievements at scientific meetings and in the scientific literature. Some take the view that, even if publication does not actually aid a competitor's development or improvement of a commercial process, open publication could give valuable indications to a competitor as to where DuPont's research was heading. Further, there is the question of compromising patent protection, and, in Howard's case, the opportunities to patent the Simmons-Smith reactions were in fact limited by early publication of the results.[5]

To reconcile the concerns of the operating departments with regard to the importance given to publication by CRD management, a system was established of circulating proposed publications to the operating departments for non-objection to, if not outright approval of, before sending the manuscripts to journals.[5] There can be no doubt that the Central Research publication policy was a fabulous success for recruiting talented young chemists. During the period when Simmons was active, many graduate students believed that the best research positions anywhere that one could aspire to were at the very few top universities and DuPont's Central Research.

Another sticky issue that Simmons helped to resolve regarded whose names should appear as co-authors on CRD publications. Some of the supervisors who did not actually participate in particular research projects but had administrative responsibility over them, felt that they should be included among the authors. This did not sit too well with the chemists directly involved and a practice was established to include only those who actually participated. It was true, however, that if the author(s) wished, others could be afforded courtesy authorship.[5]

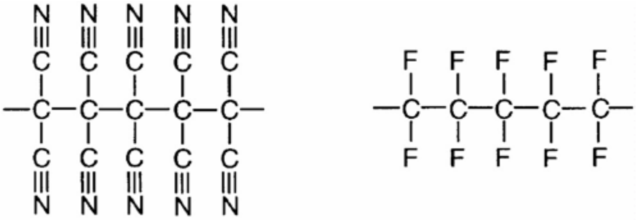
A wonderful international relation-building policy carried out by CRD during the Simmons era was to invite foreign chemists to come to the United States, not only to visit CRD but also to spend some time with university chemistry departments giving lectures and getting acquainted. Simmons made visits to Europe to search for the rising research stars to implement this policy, which developed many long-lasting relationships.[5]

As research supervisor, Simmons continued the study of unusual intermediates—now of cyanonitrene, a substance formed by heating cyanogen azide.



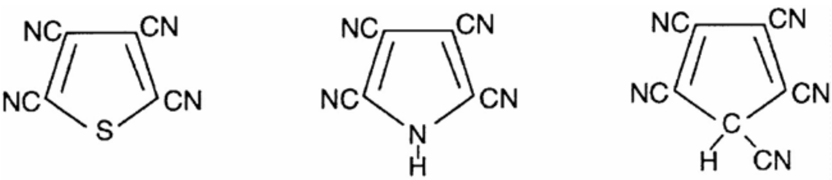
Cyanonitrene was found to provide access to a variety of cyano-substituted products by addition to aromatic hydrocarbons, alkanes, and alkenes. Simmons used isotopic labeling to show that cyanogen azide reacts with alkenes before losing N_2, but with aromatics and alkanes, cyanonitrene with two chemically equivalent nitrogens is the intermediate. Further, he demonstrated that cyanonitrene is initially formed in a singlet state and then decays with a measurable half-life to a more stable triplet state.

During the 1960s and 1970s, the Central Research Department had an extensive program under the direction of T.L.Cairns to synthesize long-chain cyanocarbons, substances analogous to long-chain fluorocarbons, such as Teflon®.



a cyanocarbon a fluorocarbon

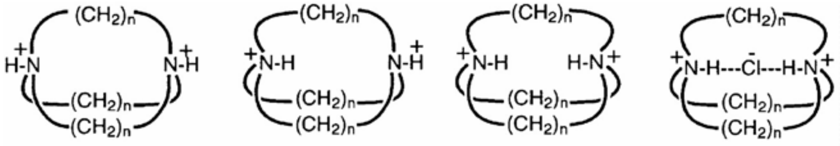
In a variation on this theme, Simmons recognized the potential of disodium dimercaptomaleonitrile for preparation of polycyano compounds, and he and his research group made many novel substances of this type, including tetracyanothiophene, tetracyanopyrrole, and pentacyanocyclopentadiene.



tetracyanothiophene tetracyanopyrrole pentacyanocyclo-pentadiene

Simmons also made a major contribution to the mechanism of cycloaddition reactions by providing the first example of a nonstereo-specific polar [2+2] cycloaddition by way of an intermediate zwitterionic intermediate.

Some of his most interesting experimental work was in his study of flexible bicyclic diamines. This research was stimulated by the classic study of DuPont's Charles Pedersen[6] on crown-ether complexes. The bicyclic amines made by Simmons and Park had the unique property of *in-out* isomerism, where a chloride ion can be encapsulated or not in the hydrocarbon cavity, depending on pH and the numbers of carbons connecting the nitrogens.

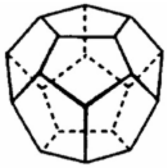


macrobicyclic amines (n = 6-10)

Along with the foregoing, Simmons made important contributions to theoretical chemistry. Early on, with the aid of DuPont's Rudolph Pariser, Simmons became involved in quantum mechanics, especially molecular orbital theory applied to the spectral properties of conjugated π-election systems. He used his theoretical techniques to gain fundamental understanding of the stereoisomers (substances with the same general connections between the atoms but with different geometrical arrangements) of planar, linear, and cross-conjugated polyenes, as well as the cyclic radilenes. He gave much attention to the molecular-orbital interactions in three-dimensional space, and this work led to the concepts of spiro-conjugation and trefoil aromatic compounds. His interest and skills in mathematics were evident in his work on the D_{9h} symmetry point group and the diamond-lattice analysis.

The chemical community has been fascinated in recent years by the discovery and chemistry of the caged allotropic forms of carbon known as “buckyballs.” It is interesting as further evidence of the Simmons imagination (if any is needed by this point) that there exists a memorandum submitted to T.L.Cairns by Howard Simmons and Edward L.

Jenner on September 7, 1956, entitled "Dodecahedrane. An Unprecedented Structural Type." This memorandum lays out in exquisite detail the anticipated geometry, the interesting question of expectations of the properties of compounds capable of encapsulating a void, as well as an imaginative projected synthesis of the $C_{20}H_{20}$ dodecahedrane by way of dimerization of a bowl-shaped tricyclodecatriene (later dubbed "triquinocene")—a route that was conceived independently by Harvard's Robert B.Woodward, but which turned out in both DuPont's and Harvard's hands to be unsuccessful despite the efforts of enormously talented Tada Fukanaga, who worked with Woodward and then moved to CRD to continue research on the synthesis. Much later, dodecahedrane was made by a wonderful but far more elaborate synthesis by Paquette and co-workers.[7]



dodecahedrane

It was in the character of Howard Simmons that, as his responsibility for research management increased, his interest in theoretical chemistry did not diminish and he developed novel concepts for applying finite topology to problems of molecular structure. With the collaboration of R.E. Merrifield, this work probed direct relationships between mathematics and chemistry. The key elements were the development of combinatorial analysis of the structures of finite spaces and the associated graph-theoretical analysis. He found the first-known method of describing the structure of a molecule by means of a topological space in a well-defined manner. It was further shown that combinatorial analysis of topological spaces provides a quantitative description of molecular complexity, as well as a detailed description of bond-strength variations with molecules that agrees well with quantum-mechanical calculations. The result of his collaboration with Merrifield was an extraordinary monograph, *Topological Methods in Chemistry,* published in 1989 just two years before he retired.

The account of the scientific work of Howard Simmons is given in greater detail than might be interesting to the general reader, because it shows the character and scientific culture of the Central Research Department in the forty-year period starting about 1950. Simmons was not an aberration in Central Research; other path-breaking scientific work was accomplished by T.L.Cairns, William D. Phillips, Earl Muetterties[8] and George Parshall, to mention just those of Central Research who were elected to the National Academy of Sciences.

Let us turn now to the important contribution made by Howard Simmons in the management of the Central Research Department. Howard had a prime role in enlarging the already significant impact of the Central Research Department. He became research director in 1974 and vice-president of Central Research in 1979. Under his leadership, the scientific effort of the laboratory was broadened and expanded with new thrusts in life and materials sciences. The thrust into life sciences was initiated by the then chairman and chief executive office of the company, Edward G.Jefferson, and resulted in a doubling of the research personnel in the department. A powerful basic research group in molecular biology was assembled that later was a major attraction for Merck and Company to participate in the formation of the joint pharmaceutical venture DuPont-Merck Pharmaceuticals, which for a few years had many DuPont and DuPont-Merck personnel working side by side in the Central Research laboratories.

The thrust into materials science was characterized by research on new polymers, optical and electronic materials, high-temperature superconductors, and ceramics. These new efforts were carried on while maintaining strong core efforts in exploratory organic chemistry, physical chemistry, analytical techniques, and catalysis. The expansion is evidenced by the scientific publications of the department, which increased from an average of 100 per year during 1975–79 to 200 per year during 1985–87, with major growth in biological and material sciences.

During the Simmons period as research director and vice-president, the department played a critical role in developing practical catalytic processes to make hydrochlorofluorocarbons (HCFCs) to replace the chlorofluorocarbons (CFCs) implicated in atmospheric-ozone depletion, an important societal need. Central Research was able to respond because of its scientific depth in fluorocarbon chemistry and in catalysis.

In the polymer field, group-transfer polymerization was discovered by O.Webster[9] and was the first new polymerization process developed since living anionic polymerization. Not only was the mechanism of the reaction determined, but the process was converted to commercial application in a relatively short time. The basic process of group transfer also has application to general organic synthesis, including natural products. Other studies have involved extension of theoretical modeling to predict tensile properties of flexible- and rigid-chain polymers.

The department developed new electronic and magnetic materials, especially potassium titanylphosphate, a most versatile nonlinear optical material. A major program was also carried on for the synthesis, characterization, and application of high-temperature superconductors.

Molecular biology and agricultural biotechnology also received major attention, one output being advances in DNA-sequencing technology based on synthesis of novel fluorescent labels. This effort also resulted in Qualicon, a DuPont venture that identifies bacteria by examination of their DNA using PCR. Substantial success was also achieved in the synthesis of unnatural peptides and proteins to accomplish specific functions and prediction of their tertiary structures.

DuPont was the first chemical company to obtain a Cray supercomputer and was in the forefront of use of supercomputer applications in quantum chemistry in support of basic research, atmospheric models relevant to ozone depletion, and theoretical models of material properties in complex multiphase systems.

Over the forty years of the golden age of DuPont's basic research programs in the Central Research Department, the world was changing. New generations of the general public came to regard nylon, Teflon®, Dacron®, Mylar®, and so on, not as technological wonders but as commodities just as essential to modern life as food, water, and power, but desired to be inexpensive, completely benign to human health no matter how used, and produced by environmentally benign processes. However, as patents expired and expertise in polymer technology became more widespread, global competition increased. Many more research-based companies entered the polymer field and new polymers aimed at niche markets proliferated. Still other companies entered the field by purchasing turnkey plants to manufacture nylon and polyester. They could then compete with DuPont on a cost basis without making DuPont's large investment in research and market development. To be sure, many such operations lacked the technical savvy to provide the quality of DuPont's sales-service support, but many customers are willing to sacrifice quality of support for lower prices.

With the rise of rampant and global competition along with a squeeze on profits and higher energy and environmental costs, it was clear that DuPont upper management was sure to ask, “What has all of this investment in Central Research done for us?” Phrases like “research is a black hole into which you pour money” became common. One should not infer that the value of the work of Central Research to the company's bottom line was not questioned earlier. Indeed it was, and often. Notions that DuPont has had during the last half of this century an unwavering corporate strategy for the utilization of its research in supporting existing businesses and starting new ones do not square with the facts.[10] The company, driven by rapidly changing business climates in the last thirty years, has often been in internal upheavals, with multiple periods of reorganization swinging between concentration, on one hand, on profit centers, strategic business units, and the like and, on the other hand, to selling off existing businesses, new acquisitions (the largest, Conoco), and new ventures.[10,11] Research has always felt the swells from these disturbances, but for a long period these were moderated, because upper management very largely had technical rather than business backgrounds and recognized the need for patience when the lead time between a laboratory discovery and a plant turning out a product is often ten to fourteen years.

It is certainly true that many new DuPont products were brought to the market between 1950 and 1980. To mention just a few, there were Nordel®, an oxidation-resistant elastomer; Lycra®, the popular spandex fiber; Delrin®, polyacetal polymer; Viton®, a fluoropolymer elastomer; Kevlar® and Nomex® aramid fibers; Kapton® polyimide films; and titanium dioxide paper and paint whiteners. Although of high quality and successful, these did not instantly capture the public imagination like nylon.[2,12] Instead, these products were born into an already crowded arena of polymers, made worldwide and, to a substantial degree, they had to compete primarily in niche applications.

The failure of the Central Research Department's overabundance of extraordinary new chemistry to provide a cornucopia of stunning new products that could be commercialized by the operating departments at acceptable costs and in acceptable time periods, along with the need to make the manufacture of the company's existing line of products more efficient and more environmentally benign, as well as perceived needs for better returns on stockholders investment, led to many changes. Initially, a virtual hold occurred on new hires in Central Research, then substantial downsizing and a refocusing of the research effort on immediate, rather than long-term, concerns.

Simmons was already involved in management when Irving Shapiro, a lawyer, became the head of the company and, when faced with the Arab oil crisis and escalating raw material costs, made substantial cuts in research. Business conditions were better when Shapiro was succeeded by Edward G.Jefferson, a Ph.D. chemist, who was a strong supporter of research and, as mentioned above, brought about a very substantial expansion of CRD in life sciences. But this was not of itself a panacea for those chemists and supervisors who felt threatened by expansion into areas in which they were unfamiliar. Simmons presided over CRD through several of the ups and downs of changing views of the value of basic research and did his very best to defend the work of the department, as well as to try to accommodate to the new situations. However, after his retirement, he often expressed private dissatisfaction with the extent to which the mission of the department changed to focus on short-term gains, and this exacerbated his concern for DuPont's long-term future.

Howard Simmons was an extraordinarily interesting person with a quickness and intellectual power that was truly impressive. He had more than enough requisites to be successful in the academic world, but he refused a number of offers of university professorships, including MIT. He did participate in visiting professorships at Harvard and the University of Chicago, and he accepted an adjunct professorship at the University of Delaware. He took professional responsibilities seriously, and served on advisory or executive committees for the American Chemical Society, MIT, Harvard, Rensselaer, Franklin Institute, Los Alamos, Maryland, University of California at San Diego, and Chicago. He was on the Advisory Board and later president of the Board of Trustees of the Chemical Heritage Foundation. Late in life, he was appointed to the National Science Board, but ill health reduced his participation in the last part of his term.

Simmons's scientific and management work was recognized by honorary degrees from Rensselaer and the University of Delaware. He received the Chandler Medal from Columbia, the National Medal of Science in 1992, and in 1994 both the Priestley Medal, the highest award of the American Chemical Society, and the Lavoisier Medal of the DuPont Company, its highest award for technical achievement. He was elected to the National Academy of Sciences in 1975 and the American Philosophical Society in 1990.

Howard was a staunch political conservative with a high level of disdain for most contemporary liberal thought. One could have almost bitter arguments with him on matters of political principles and outcomes, but, because these were on an intellectual rather than personal plane, one could remain a warm and fast friend. An omnivorous reader.

Howard was extraordinarily well informed on a wide range of subjects.

From age twelve onward, Howard Simmons and Elizabeth Warren grew up together in Norfolk. After the not-unusual teenage ups and downs, they were married on September 1, 1951. Liz, as she is widely known, was a wonderful partner in the Simmons enterprise. The couple had two sons, John W. and Howard E.Simmons III, both of whom earned Ph.D. degrees in organic chemistry, John at Yale, Howard at Harvard. They are employed at DuPont, John in DuPont Nylon and Howard at Central Research.

Howard Simmons' views on physical exercise were apparently not far from those of Alexander Woollcot's: “If I think about exercise, I know if I wait long enough, the thought will go away.” But he did have a good-sized powerboat and found great happiness in overnight cruising on the Chesapeake Bay. A heavy smoker for much of his life, Howard finally succumbed after a long struggle to lung cancer and heart disease on April 26, 1997.

Howard Ensign Simmons, Jr., was a titan among chemists and his personal and scientific achievements will not soon be forgotten.

WE WISH TO THANK Elizabeth Simmons and Howard E.Simmons III for their help in providing valuable information and insights for this memoir, as well as the Chemical Heritage Foundation for access to, and permission to use material from, their H.E.Simmons, Jr., oral history.[5] Very helpful additional comments and suggestions were received from Edward G.Jefferson, E.P.Blanchard, Joseph H. Miller, Blaine C.McKusick, and Suzanne Grandel. We especially thank the Central Research Department of Du Pont for supplying and making possible publication of the accompanying color portrait of Howard Simmons.

NOTES

1. J.D.Roberts and J.C.Sheehan. Arthur Clay Cope, 1909–1966. In *Biographical Memoirs,* vol. 60, pp. 16–30. Washington, D.C.: National Academy Press, 1991.

2. M.E.Hermes. *Enough for One Lifetime. Wallace Carothers, Inventor of Nylon.* Washington, D.C.: ACS Books, 1996.

3. R.C.Ferguson. William D.Phillips and nuclear magnetic resonance at DuPont. In *Encyclopedia of Nuclear Magnetic Resonance,* vol. 1, eds. D.M.Grant and R.K.Harris, pp. 309–13. Chichester, England: John Wiley & Sons, 1996.

4. R.Warmuth. NMR of *o*-benzyne stabilized inside a molecular container compound: Is the species a strained alkyne or a cumulene? *Angew. Chem. Int. Ed.* 36(1997):1347–50.

5. J.J.Bohning. *Howard E.Simmons, Jr., Oral History.* Philadelphia: Chemical Heritage Foundation, 1993.

6. C.G.Pedersen. The discovery of crown ethers. In *Nobel Lectures in Chemistry (1981–1990),* ed. B.G.Malmstrom, pp. 495–511. Singapore: World Science, 1992.

7. L.A.Paquette. Dodecahedrane—the chemical transliteration of Plato's Universe (a review). *J. Am. Chem. Soc.* 79(1982):4495–4500.

8. R.G.Bergman, G.W.Parshall, and K.N.Raymond. Earl L. Muetterties, 1927–1984. In *Biographical Memoirs,* vol. 63, pp. 383–93. Washington, D.C.: National Academy Press, 1994.

9. O.W.Webster and coworkers. Group-transfer polymerization. 1. A new concept for addition polymerization with silicon initiators. *J. Am. Chem. Soc.* 105(1983):5706–5708.

10. D.A.Hounshell and J.J.K.Smith. *Science and Corporate Strategy. DuPont R&D, 1902–1980.* New York: Cambridge University Press, 1988.

11. Ibid. Part V, pp. 503–601, for an account of early struggles between research managers and non-research oriented managers over the importance of research to DuPont's long-term success.

12. For vivid descriptions of the public reaction to the initial sales of nylon stockings, see preface in Note 2 and pp. 268–71 in Notes 10.

SELECTED BIBLIOGRAPHY

1953 With J.D.Roberts, L.A.Carlsmith, and C.W.Vaughan. Rearrangement in the reaction of chlorobenzene-1-^{14}C with potassium amide. *J. Am. Chem. Soc.* 75:3290–91.

1956 With J.D.Roberts, D.A.Semenow, and L.A.Carlsmith. The mechanism of aminations of halobenzenes. *J. Am. Chem. Soc.* 78:601–11.

1958 With R.D.Smith. A new synthesis of cyclopropanes from olefins. *J. Am. Chem. Soc.* 80:5323–24.

1960 With D.E.Wiley. Fluoroketones. I. *J. Am. Chem. Soc.* 82:2288–96.

1961 A cycloaddition reaction of benzyne. *J. Am. Chem. Soc.* 83:1657–64.

1964 Pariser-Parr theory: Quantum mechanical integrals from the benzene spectrum. *J. Chem. Phys.* 40:3554–62.

With J.K.Williams. An empirical model for nonbonded H-H repulsion energies in hydrocarbons. *J. Am. Chem. Soc.* 86:3222–6.

1966 With S.Proskow and T.L.Cairns. Stereochemistry of the cycloaddition reaction of 1,2-bis (trifluoromethyl)-1,2-dicyanoethylene and electron-rich alkenes. *J. Am. Chem. Soc.* 88:5254–66.

1967 With R.A.Carboni, J.C.Kauer, and J.E.Castle. Aromatic azapentalenes. IV. *J. Am. Chem. Soc.* 89:2618–25.

With A.G.Anastassiou. Cyanonitrene. A reaction with saturated hydrocarbons. *J. Am. Chem. Soc.* 89:3177–84.

With T.Fukunaga. Spiroconjugation. *J. Am. Chem. Soc.* 89:5208–15.

1968 With C.H.Park. Macrobicyclic amines. I. *Out-in* isomerism of (1,k+2)-diazabicyclo[k.l.m] alkanes. *J. Am. Chem. Soc.* 90:2428–29.

With C.H.Park. Macrobicyclic amines. II. *Out-out in-in* prototropy in (1,k+2)-diazabicyclo[k.l.m] alkane ammonium ions. *J. Am. Chem. Soc.* 90:2429–31.

With C.H.Park. Macrobicyclic amines. III. Encapsulation of halide ions by *in-in*-(1,k+2)-diazabicyclo[k,l,m]alkane ammonium ions. *J. Am. Chem. Soc.* 90:2431–32.

With J.C.Kauer. The tetramers of acetylenedicarboxylic esters. *J. Org. Chem.* 33:2720–26.

1970 With C.H.Park, R.T.Uyeda, and M.Habibi. Macrobicyclic molecules. *Trans. N. Y. Acad. Sci.* 32(5):521.

With A.G.Anastassiou, J.N.Shepelavy, and F.D.Marsh. *Cyanonitrene.* New York: Interscience Publishers.

1972 With C.H.Park. Bicyclo[8.8.8]hexacosane. *Out-in* isomerism. *J. Am. Chem. Soc.* 94:7184–86.

1973 With T.L.Cairns, S.A.Vladuchick, and C.M.Hoiness. Cyclopropanes from unsaturated compounds and methylene iodide and zinc-copper couple. *Org. React.* 20:1–131.

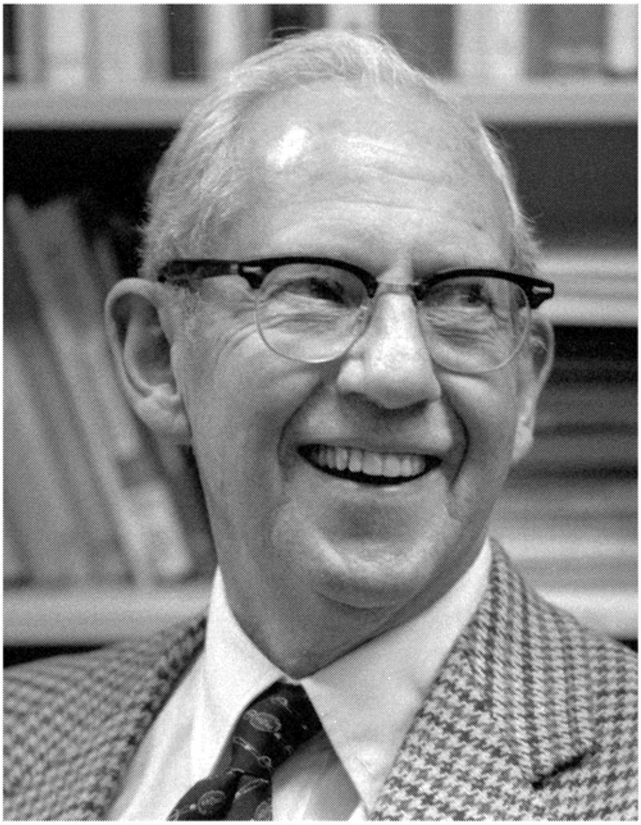
1974 With J.F.Bunnett. *Orbital Symmetry Papers.* Washington, D.C.: American Chemical Society Publishers.

1976 With M.D.Gordon and T.Fukunaga. A quantitative treatment of spiroconjugation. Long-range ‘through-space' interactions and chemical reactivity of spirenes. *J. Am. Chem. Soc.* 98:8401.

1980 With S.A.Vladuchick, T.Fukunaga, and O.W.Webster. Thiacyanocarbons. 6. 1,4-dithiimo(2,3-c; 6,5-c')-diisothiazole 3,7-dicarbonitrile, isothiazole(3,4-f)-(1,2,3,4,5-pentathiepine-8-carbonitrile, and disodium 5-cyanoisothiazoledithiolate. *J. Org. Chem.* 45:5122–30.

1983 With T.Fukunaga, J.J.Wendeloski, and M.D.Gordon. Trefoil aromatics: A potentially new class of aromatic molecules. *J. Am. Chem. Soc.* 105:2729–34.

1989 With R.E.Merrifield. *Topological Methods in Chemistry*. New York: Wiley Interscience.



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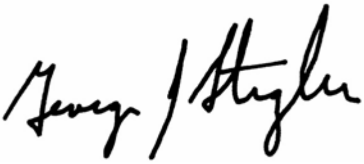

GEORGE JOSEPH STIGLER

January 17, 1911–December 1, 1991

BY MILTON FRIEDMAN

I CANNOT PRETEND TO objectivity in writing about George Stigler. For nearly sixty years he was either my closest friend or one of my closest friends. My debt to him, both personal and professional, is beyond measure. Despite deep sadness at his death, like so many others who knew him, I cannot think of him without an inadvertent smile rising to my lips. He was as quick of wit as of mind, and his wit always had a point. His occasional humorous articles—such as "A Sketch of the History of Truth in Teaching" (Stigler, 1973)—have become classics and demonstrate that had he, like an earlier Chicago Ph.D. in economics, Stephen Leacock, chosen to become a professional humorist as well as an economist, he would have achieved no less fame in the one field than in the other.

George Stigler was one of the great economists of the twentieth—or any other—century, with a gift for writing matched among modern economists only by John Maynard Keynes. Intellectual history was his first field of specialization. It remained a lasting love and provided a rich seedbed for his scientific work. A deep understanding of the ideas of the great economists of the past gave him a strong foundation on which to build an analysis of contemporary issues. Few economists have so consistently and successfully combined economic theory with empirical analysis, or ranged so widely. Stigler regarded economic theory, in the words of Alfred Marshall, as "an engine for the discovery of concrete truth," not as a subject of interest in its own right, a branch of mathematics.

PERSONAL HISTORY

George Stigler was born January 17, 1911, in Renton, Washington, a suburb of Seattle. He was the only child of Joseph and Elizabeth Hungler Stigler, who had separately migrated to the United States at the end of the nineteenth century, his father from Bavaria, his mother from what was then Austria-Hungary. George writes that his "father had been a brewer until prohibition drove that activity underground. Thereafter, he tried a variety of jobs," finally entering the real estate market. "My parents bought rundown places, fixed them up, and sold them. By the time I was sixteen, I had lived in sixteen different places in Seattle. But my parents had a comfortable if nomadic existence" (Stigler, 1988, pp. 9–10).

George went to public schools and then to the University of Washington, all in Seattle, receiving a B.A. in 1931. "An insatiable and utterly indiscriminate reader," he "got lots of good grades" at the University of Washington. He said that, when he graduated from college, he had "no thought of an academic career"; it was the depression and jobs in business were scarce, so he applied for and was awarded a fellowship at Northwestern University for graduate study in the business school, receiving an M.B.A. in 1932 (Stigler, 1988, p. 15). At Northwestern he developed an interest in economics and decided on an academic career. He returned to the University of Washington for one further year of graduate study, and then received a tuition scholarship to study economics at the University of Chicago. There he found an intense intellectual atmosphere that captivated him. Chicago became his intellectual home for the rest of his life, as a student from 1933 to 1936, a faculty member from 1958 to his death in 1991, and a leading member of and contributor to the “Chicago School” throughout. He received his Ph.D. in 1938.

At Chicago, Stigler was particularly influenced by Frank H.Knight, under whom he wrote his dissertation—a noteworthy feat, since only three or four students ever managed to complete a dissertation under Knight in his twenty-eight years on the Chicago faculty. Stimulating and influential in both economic analysis and social philosophy, Knight was a perfectionist and tended to inhibit students who came under his influence. It is a mark of Stigler's character and drive that he never succumbed to that aspect of Knight's influence; rather, he imbibed what he described as Knight's “devotion to the pursuit of knowledge… a sense of unreserved commitment to ‘truth'” (Stigler, 1988, pp. 17–18).

The other faculty members whose influence George stressed were Jacob Viner, who taught economic theory and international economics; John U.Nef, economic historian; and their younger colleague Henry Simons, who became a close personal friend and whose *A Positive Program for Laissez Faire* greatly influenced Stigler and many of his contemporaries.

“At least as important to me,” wrote George, “as the faculty were the remarkable students I met at Chicago,” and he goes on to list W.Allen Wallis; the author of this memoir; Kenneth Boulding and Robert Shone from Great Britain; Sune Carlson from Sweden; Paul Samuelson; and Albert G.Hart—all of whom subsequently had distinguished careers (Stigler, 1988, pp. 23–25).

I overlapped George at Chicago for one year, 1934–35, during which he, W.Allen Wallis, and I formed what proved to be a lifelong friendship. As it happened, all three of our future spouses were also students at Chicago. George was to marry Margaret Mack, always known as Chick, who was majoring in social science. Allen would marry Anne Armstrong, an art history major, and I married Rose Director, whose major was economics. We soon formed a sextuple whose lives were intertwined from then on.

In 1936 George accepted an appointment as an assistant professor at Iowa State College (now University), and shortly thereafter was married to Margaret “Chick” Mack. George and Chick had three sons: Stephen, a professor of statistics at the University of Chicago; David, a corporate lawyer; and Joseph, a businessman. The family suffered a tragic loss in 1970, when Chick died unexpectedly, without any advance warning. George never remarried.

George accepted an appointment at the University of Minnesota in 1938 and then went on leave in 1942 to work first at the National Bureau of Economic Research and later at the Statistical Research Group of Columbia University, a group directed by Allen Wallis that was engaged in war research on behalf of the armed services. When the war ended in 1945, George returned to the University of Minnesota, but he remained only one year, leaving in 1946 to accept a professorship at Brown University. That simple statement conceals a traumatic experience. In George's words: “In the spring of 1946 I received the offer of a professorship from the University of Chicago and, of course, was delighted at the prospect. The offer was contingent upon approval by the central administration after a personal interview. I went to Chicago, met with the president, Ernest Colwell—because Robert Hutchins was ill that day—and I was vetoed! I was too empirical, Colwell said, and no doubt that day I was. So the professorship was offered to Milton Friedman, and President Colwell and I had launched the

Chicago School" (Stigler, 1988, p. 40). It speaks volumes for George's character that the incident never cast the slightest shadow on our friendship.

In 1946 George and I were two of the thirty-six participants at a conference in Switzerland convened by Friedrich A.Hayek to discuss the dangers to a free society. The Mont Pelerin Society was founded at that conference and has since grown and flourished, providing a forum for members from all over the world to discuss the issues involved in achieving and maintaining political and economic freedom. An active member of the society until his death, George served as its president from 1976 to 1978.

After a year at Brown, George moved to Columbia, where he remained until 1958, despite several attempts by Theodore Schultz, chairman of the Chicago Department of Economics, to bring him to Chicago. In 1958 Allen Wallis, then dean of the University of Chicago business school, persuaded him to accept the Charles R.Walgreen professorship of American institutions. George remained at Chicago for the rest of his life. At Chicago he became an editor of the *Journal of Political Economy;* established the Industrial Organization Workshop, which achieved recognition as the key testing ground for contributions to the field of industrial organization; and in 1977 founded the Center for the Study of the Economy and the State, serving as its director until his death.

In the academic year 1957–58, George was a fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford. From 1971 to his death, George was a fellow at the Hoover Institution at Stanford, and spent part of almost every year at Hoover.

George was president of the American Economic Association in 1964, and of the History of Economics Society in 1977. He was elected to the National Academy of Sciences in 1975. He received the Alfred Nobel Memorial Prize in Economic Science in 1982 “for his seminal studies of industrial structures, functioning of markets and causes and effects of public regulation.” He received the National Medal of Science from Ronald Reagan in 1987.

George's governmental activities included service as a member of the attorney general's National Committee to Study the Antitrust Laws, 1954–55; chairman, Federal Price Statistics Review Committee, 1960–61; member, Blue Ribbon Panel of the Department of Defense, 1969–70; vice-chairman, Securities Investor Protection Corporation, 1970– 73; co-chairman, Blue Ribbon Telecommunications Task Force, Illinois Commerce Commission, 1990–91.

A word about George as a person: In the nearly six decades of our friendship, I never knew him to do a mean or hurtful or unworthy thing to anyone. An ideal friend in time of trouble, he would go to any lengths to be helpful.

He always appeared casual and unhurried, seeming to have ample time for golf (his favorite sport), tennis, bridge, carpentry, photography (his favorite hobby), casual talk with friends, consultations with students, and constructive and detailed criticisms of the writings of his students and academic friends. Yet, he also was incredibly productive, turning out a steady stream of fundamental contributions. Truly, as his son Stephen said at a memorial service, “My father had phenomenal energy.”

One feature of George's personality that he did his best to conceal was his extreme personal sensitivity. His smart cracks were in part a way of covering that sensitivity, as was his half-embarrassed laugh. He was as sensitive to others as to himself. The stiletto concealed in his humor was always meant for ideas or policies, never ad hominem—unless “An Economist Plays with Blocs” (1954), his brilliant title for an article on Galbraith's theory of countervailing power, can be so interpreted.

George was a delightful correspondent. Serious and profound discussion never came without an interlarding of amusing comments. In a letter from London in 1948 when he was giving *Five Lectures on Economic Problems* (1949), after remarking on the inconvertibility of the pound and the inedible, still-rationed food, he concluded, “So here I am losing weight and gaining pounds.”

George was an extremely valuable colleague. He provided much of the energy and drive to the interaction among members of the Chicago economics department, business school, and law school that came to be known at the Chicago School. His workshop on industrial organization was an outgrowth of a law school seminar started by Aaron Director, which George cooperated in running when he came to Chicago. His relations were especially close with Aaron, Gary Becker, Richard Posner, Harold Demsetz, and myself, enhancing significantly the scientific productivity of all of us.

STIGLER AS SCIENTIST

HISTORY OF THOUGHT

Stigler's doctoral dissertation, published as *Production and Distribution Theories* (1941), was a historical survey of neo-classical theories that remains the definitive study of its subject. That book was followed by a steady flow of perceptive, thoughtful, and beautifully written articles and books interpreting the contributions of his predecessors, some of which were collected in *Essays in the History of Economics* (1965).

Throughout, Stigler's interest was in “the essential structure of the… analytical system” of the authors whose work he examined (Stigler, 1969, p. 220). In judging that analyti cal system, he placed great stress on its implications for observable phenomena. "Surprising as it may sound, no previous scholar had ever examined the development of the discipline with anything like the same insistence that intellectual progress had to be measured in terms of its ability to generate empirically refutable propositions" (Rosenberg, 1993, p. 836). Stigler tried not only to identify such propositions but to put them to the test, often with data that would have been available to the author whose work he was examining.

During most of Stigler's professional career, the history of economic thought was in the doldrums as a field of study. His writing played a major role in keeping the field alive and enhancing its attractiveness. By the end of his career, the field was flourishing, thanks in part to the example he set and to the new directions for research that he pioneered.

PRICE THEORY

George's first important publication after his doctoral thesis was a textbook, *The Theory of Competitive Price* (1942), which was followed by revised versions under the title *The Theory of Price* in 1946, 1952, 1966, and 1987. Its systematic linking of highly abstract theory to observable phenomena is unique among intermediate textbooks in price theory, as is its concise yet rigorous exposition. That feature, according to Thomas Sowell, one of his students, "made it probably the least readable thing Stigler ever wrote. It was not a matter of convoluted writing or confused thought—Stigler was never guilty of either of these common academic sins—but of excessive condensation that required painstakingly slow pondering over every concentrated thought. If the book had been three times as long, it could have been read in half the time. Still, it remained something of a classic, though Stigler himself made many a wry joke about its supposedly meager sales. It was the kind of book that teachers of price theory courses read themselves, while they assigned some other text to the class" (Sowell, 1993, pp. 785–86).

The linkage of fact and theory in his textbook foreshadowed his subsequent scientific work. His many contributions to economic theory were all a byproduct of seeking to understand the real world, and nearly all led to an attempt to provide some quantitative evidence to test the theory or to provide empirical counterparts to theoretical concepts.

An early example of the latter is an article on "The Cost of Subsistence" (1945), which starts, "Elaborate investigations have been made of the adequacy of diets at various income levels, and a considerable number of 'low-cost,' 'moderate,' and 'expensive' diets have been recommended to consumers. Yet, so far as I know, no one has determined the minimum cost of obtaining the amounts of calories, proteins, minerals, and vitamins which these studies accept as adequate or optimum." George then set himself to determine the minimum cost diet, in the process producing one of the earliest formulations of a linear programming problem in economics, for which he found an approximate solution, explaining that "there does not appear to be any direct method of finding the minimum of a linear function subject to linear constraints." Two years later George Dantzig provided such a direct method, the simplex method, now widely used in many economic and industrial applications.

George's approximate solution—very close to the best possible one—cost very little, far less than the standard low-cost adequate diet, demonstrating that those diets could not be defended as "scientific" but reflected mainly allowance for taste and variety rather than simply for nutritive adequacy. The estimated cost of such low-cost diets has subsequently become the basis for the widely used poverty lev els of income, assuring the continued significance of this finding.

History of thought apart, George's impact was greatest and most lasting in the three fields that were singled out in the Nobel citation, those he labeled the economics of information, the theory of economic regulation, and the organization of industry.

"The Economics of Information" is the title of a seminal article (Stigler, 1961) that gave birth to an essentially new area of study for economists. In his intellectual autobiography, George termed it, "My most important contribution to economic theory" (Stigler, 1988, pp. 79–80). The article begins, "One should hardly have to tell academicians that information is a valuable resource: knowledge is power. And yet it occupies a slum dwelling in the town of economics. Mostly it is ignored." Stigler then proceeded to illustrate the importance of subjecting information to economic analysis with two examples: the dispersion of prices and the role of advertising (Stigler, 1961, pp. 213–25).

This article is a splendid illustration of several of Stigler's signal virtues: creativity (which he defined as consisting "of looking at familiar things or ideas in a new way"), the capacity to extract new insights about those seemingly familiar things, and the ability to state his main points in a provocative and eminently readable way.

As he wrote in his Nobel memorial lecture,

The proposal to study the economics of information was promptly and widely accepted. Within a decade and a half, the literature had become so extensive and the theorists working in the field so prominent, that the subject was given a separate classification in the *Index of Economic Articles,* and more than a hundred articles a year are now devoted to the subject.

The absence of controversy was certainly no tribute to the definitiveness of my exposition…. The absence of controversy was due instead to the fact that no established scientific theory was being challenged by this work; in fact, all I was challenging was the neglect of a promising subject (Stigler, 1983, p. 539).

The historian of economic thought practicing his craft on himself.

ECONOMIC REGULATION

Starting from the traditional view that government regulation was instituted for the protection of the public, Stigler was struck by the absence of any quantitative studies of the actual effect of regulation. His first effort to remedy this was directed at the regulation of the prices of public utilities. The result was a 1962 article written jointly with Claire Friedland, his long-time associate, entitled “What Can Regulators Regulate? The Case of Electricity,” which concluded that regulation of electric utilities had produced no significant effect on rates charged. This was followed two years later by “Public Regulation of the Securities Market,” which concluded that purchasers of new stock issues fared no better (or worse) after the creation of the Securities and Exchange Commission than before.[1] These articles, like “The Economics of Information,” opened a floodgate of empirical studies of the effects of economic regulation. Economists could no longer simply take it for granted that the effects of regulation corresponded to the stated intentions.[2]

These essays “also posed a basic problem: If regulation does not generally achieve its stated objectives, why have so many agencies been established and kept in existence?” (Schmalensee, 1987, p. 499). “The Theory of Economic Regulation” (Stigler, 1971) presents Stigler's answer to that question. The “central thesis of the article,” Stigler wrote, “is that, as a rule, regulation is acquired by the industry and is designed and operated primarily for its benefit.” He notes that two “alternative views of the regulation of indus try are widely held. The first is that regulation is instituted primarily for the protection and benefit of the public at large or some large subdivision of the public… The second view is essentially that the political process defies rational explanation." He then gives example after example to support his own thesis, which by now has become the orthodox view in the profession, concluding, "The idealistic view of public regulation is deeply imbedded in professional economic thought… The fundamental vice of such a [view] is that it misdirects attention"—to preaching to the regulators rather than changing their incentives.

Stigler's analysis fed the emerging field that has since come to be called "public choice" economics: the shift from viewing the political market as not susceptible to economic analysis, as one in which disinterested politicians and bureaucrats pursue the "public interest," to viewing it as one in which the participants are seeking, as in the economic market, to pursue their own interest, and hence subject to analysis with the usual tools of economics. The seminal work that deserves much of the credit for launching public choice, *The Calculus of Consent,* by James Buchanan and Gordon Tullock, appeared in the same year as the Stigler-Friedland article.

"Smith's Travels on the Ship of State," published in the same year as "The Theory of Economic Regulation," raises the same question on a broader scale. Smith gives self-interest pride of place in analyzing the economic market, but he does not give it the same role in analyzing the political market. Smith's failure to do so constitutes Stigler's main— indeed, nearly only—criticism of the *Wealth of Nations,* that "stupendous palace erected upon the granite of self interest" (Rosenberg, 1993, p. 835). The same theme pervades many of Stigler's later publications.

The Organization of Industry (1968) is the title of a book whose "main content," as Stigler says in the preface, "is a reprinting of 17 articles I have written over the past two decades [including "Economics of Information"] in the area of industrial organization… Although the main topics in industrial organization are touched upon, the touch is often light. The ratio of hypotheses to reasonably persuasive confirmation is distressingly high in all of economic literature, and it must be my chief and meager defense that I am not the worst sinner in the congregation." Stigler's main contribution to the field, both in this book and later writing, was the use of empirical evidence to test hypotheses designed to explain features of industrial organization. Article after article combines subtle theoretical analysis with substantial nuggets of empirical evidence, presented so casually as to conceal the care with which the data were compiled and the effort that was expended to determine what data were both relevant and accessible. These articles record the shift in Stigler's views on antitrust—from initial support of an activist antitrust policy to skepticism about even a minimalist policy—that led up to his path-breaking article on "The Theory of Economic Regulation" (Stigler, 1971).

Two other facets of Stigler's contributions deserve mention. First, his essays written for the general public, collected in three volumes, *The Intellectuals and the Marketplace* (1963), *The Citizen and the State* (1975), and *The Economist as Preacher* (1982). "There he [the intelligent layman] will find a potpourri of wit and seriousness blended with a high writing style" (Demsetz, 1982, p. 656). Second, his role as editor and reviewer. "For 19 years Stigler was a very successful editor of the *Journal of Political Economy.* Under his leadership this journal solidified its high reputation among economists" (Becker, 1993, p. 765). His complete bibliography lists 73 reviews in 24 publications ranging from strictly professional, like the *Journal of Political Economy* (22) and the

American Economic Review (10), to the popular, like the *Wall Street Journal* (5), and the *New York Times* (3), and dating from 1939 to 1989.

Stigler's last book, his intellectual autobiography, *Memoirs of an Unregulated Economist* (1988), is a delight to read. As I described it at the time: "Stigler's memoirs are a gem: in style, in wit, and above all, in substance, they reflect accurately his own engaging personality and his extraordinarily diverse contributions to our science."

STIGLER AS TEACHER

Stigler was also a great teacher. Many who knew him only casually, especially in his younger years, were offended by his wit, which could be biting, and his unerring ability to find just the right response to deflate pomposity and pretentiousness. His students never had that reaction. He was uniformly available, tolerant of their lack of understanding of subtle points, and willing to go to any length to help them. He inspired them by his own high standards and instilled a respect for economics as a serious subject concerned with real problems.

As John Lothian, one of my students who took several courses from Stigler, wrote me after Stigler's death: "His lectures taught me how to think about economics… His public persona was one of not suffering fools gladly, but that certainly did not come across in the classroom or in his individual meetings with us to talk over what we were doing in our papers for the course… He seemed quite willing to put up with foolishness from us as long as it seemed like we might ultimately get somewhere with what we were doing."[3] Another student of Stigler's, Thomas Sowell, wrote: "What Stigler really taught, whether the course was industrial organization or the history of economic thought, was intellectual integrity, analytical rigor, respect for evi dence—and skepticism toward the fashions and enthusiasms that come and go" (Sowell, 1993, p. 788).

Stigler supervised many doctoral dissertations at both Columbia and the University of Chicago, a sharp contrast with the record of Frank Knight, under whom Stigler wrote his thesis. His students come close to dominating the field of industrial organization.[4]

FINAL WORD

I give final word on Stigler to his colleague and fellow recipient of the Nobel Memorial Prize in Economic Science, Ronald Coase:

He is equally at home in the history of ideas, economic theory, and the study of politics. Even more remarkable is the variety of ways in which he handles a problem; he moves from the marshaling of high theory to aphorism to detailed statistical analysis, a mingling of treatments…. It is by a magic of his own that Stigler arrives at conclusions which are both unexpected and important. Even those who have reservations about his conclusions will find that a study of his argument has enlarged their understanding of the problem being discussed and that aspects are revealed which were previously hidden. Stigler never deals with a subject which he does not illuminate. And he expresses his views in a style uniquely Stiglerian, penetrating, lively, and spiced with wit. His writings are easy to admire, a joy to read, and impossible to imitate (Coase, 1991, p. 472).

NOTES

1. Both essays are reprinted in *The Citizen and the State: Essays on Regulation,* pp. 61–77, 78–100. Chicago: University of Chicago Press, 1975.

2. Sam Peltzman recalculated the empirical results in the Stigler-Friedland article to correct a mistake in the original. His thoughtful and sophisticated article brings the story up to date (Peltzman, 1993).

3. Personal letter dated Dec. 3, 1991.

4. According to Claire Friedland, Stigler's associate for many years, he served on more than forty thesis committees at Chicago,

perhaps forty more at Columbia, and chaired a considerable fraction of those committees.

REFERENCES

Becker, G.S. 1993. George Joseph Stigler. *J. Polit. Econ.* 101:761–67.

Coase, R. 1991. George J. Stigler. In *Remembering the University of Chicago,* ed. E.Shils, pp. 469–78. Chicago: University of Chicago Press.

Demsetz, H. 1982. The 1982 Nobel Prize in economics. *Science* 218:655– 57.

Peltzman, S. 1993. George Stigler's contribution to the economic analysis of regulation. *J. Polit. Econ.* 101:818–32.

Rosenberg, N. 1993. George Stigler: Adam Smith's best friend. *J. Polit. Econ.* 101:833–48.

Schmalensee, R. 1987. *The New Palgrave: A Dictionary of Economics,* vol. 4, eds. J.Eatwell, M.Milgate, and P.Newman, pp. 499–500. New York: Stockton Press.

Sowell, T. 1993. A student's eye view of George Stigler. *J. Polit. Econ.* 101:784–92.

Stigler, G.J. 1961. The economics of information. *J. Polit. Econ.* 69:213–25.

——. 1969. Does economics have a useful past? *Hist. Polit. Econ.* 1:217–30.

——. 1971. The theory of economic regulation. *Bell J. Econ. Man. Sci.* 2:3–21.

—— 1973. A sketch of the history of truth in teaching. *J. Polit. Econ.* 81:491–95.

—— 1975. *The Citizen and the State: Essays on Regulation.* Chicago: University of Chicago Press.

—— 1983. Nobel lecture: The process and progress of economics. *J. Polit. Econ.* 91:529–45.

—— 1988. *Memoirs of an Unregulated Economist.* New York: Basic Books.

SELECTED BIBLIOGRAPHY

1941 *Production and Distribution Theories: 1870–1895*. New York: Macmillan.

1945 The cost of subsistence. *J. Farm Econ.* 27:303–14.

1946 *The Theory of Price*. New York: Macmillan.

1947 *Domestic Servants in the United States, 1900–1940*. New York: National Bureau of Economic Research.

The kinky oligopoly demand curve and rigid prices. *J. Polit. Econ.* 55:432–49.

Trends in Output and Employment. New York: National Bureau of Economic Research.

1949 *Five Lectures on Economic Problems*. New York: Longmans, Green Co.

1950 *Employment and Compensation in Education*. New York: National Bureau of Economic Research.

1954 The economist plays with blocs. *Am. Econ. Rev. Pap. Proc.* 44:7–14.

1956 *Trends in Employment in the Service Industries*. Princeton, N.J.: Princeton University Press.

1957 With D.M.Blank. *Supply and Demand for Scientific Personnel*. Princeton, N.J.: Princeton University Press.

1958 The goals of economic policy. *J. Bus.* 5:169–76.
1963 *Capital and Rates of Return in Manufacturing Industries.* Princeton, N.J.: Princeton University Press.
The Intellectual and the Market Place, and Other Essays. New York: Free Press of Glencoe.
1965 *Essays in the History of Economics*. Chicago: University of Chicago Press.
1967 Imperfections in the capital market. *J. Polit. Econ.* 75:287–92.
1968 *The Organization of Industry*. Homewood, Ill.: Irwin.
1970 Director's law of public income redistribution. *J. Law Econ.* 13:1–10.
1975 *The Citizen and the State: Essays on Regulation.* Chicago: University of Chicago Press.
1977 With G.S Becker. De gustibus non est disputandum. *Am. Econ. Rev.* 67:76–90.
1982 *The Economist as Preacher, and Other Essays*. Chicago: University of Chicago Press.
1986 The Essence of Stigler, *eds. K.R.Leube and T.G.Moore. Stanford, Calif.: Hoover Institution Press.*

1988 *Memoirs of an Unregulated Economist.* New York: Basic Books.
1990 The place of Marshall's *Principles* in the development of economics. In *Centenary Essays on Alfred Marshall,* ed. J.K.Whitaker, pp. 1–13. Cambridge: Cambridge University Press.

CLINTON NATHAN WOOLSEY

November 30, 1904–January 14, 1993

BY RICHARD F.THOMPSON

ONE OF THE GREAT achievements of neuroscience in this century has been characterization of the organization of sensory and motor representations in the cerebral cortex. Rough facts of cortical localization were known from the nineteenth century and earlier, based on studies from brain-damaged humans and animals and by application of electrical stimulation. The most important contemporary scientist to pioneer the fine-grained analysis of sensory and motor representations in the cortex was Clinton N.Woolsey. In the course of his remarkable career he made many important and fundamental discoveries.

Clinton N.Woolsey was born on November 30, 1904, in Brooklyn, New York, the son of Joseph Woodhull and Mathilda Louise Aicholz Woolsey. He left Brooklyn at the age of nine months ("before developing a Brooklyn accent"). He spent his youth in Orange County, New York, and attended a one-room country school from grades 1 through 6. He described it as an interesting experience, because he "could listen to the lessons given to all the pupils." He attended grades 7 through 10 in Montgomery, New York, and moved to Schenevus, New York, for his third year of high school. There he was awarded the H.Bernard Gold Medal as the best student of the year.

In the audience during his award ceremony was Dr. Walter Allan Cowell of Olean, New York, who suggested that young Woolsey move to Olean for his senior year, which he did. Cowell was a physician interested both in the practice of medicine and in research and was studying the effects on diabetes of insulin, which had just been discovered. As a result of his association with Cowell, Woolsey became greatly interested in medicine and in research. He graduated near the top of his class and spent another year at Olean High School taking extra work in Latin, French, and other subjects.

In 1924 Woolsey entered Union College in Schenectady, New York, where Cowell had graduated. Woolsey continued his study of Latin and French and took two years of Greek and the courses needed to enter medical school. Among his most impressionable experiences at Union College was a remarkable psychology professor, Johnny March, who "described the experiments of Pavlov and Sherrington so vividly that one felt in the presence of these investigators and their experimental animals." As a result of this, Woolsey considered going to Columbia University for training in psychology. Instead, he decided to go to medical school, and was accepted by the Johns Hopkins University School of Medicine in 1928.

During his first year at Hopkins, Woolsey took courses in histology and neuroanatomy from Dr. Marion Hines. At that time Woolsey intended to become a brain surgeon, so Hines sent him to work with Dr. Sarah Tower, who was an accomplished animal surgeon. Following a special course on localization of function taught jointly by Hines and Tower, Hines invited Woolsey to work with her on the dog brain, which led to his first publication: "On the Postural Relations of the Frontal and Motor Cortex of the Dog" (1933).

Before finishing his fourth year of medical studies, Woolsey developed pulmonary tuberculosis (a not uncommon con dition for medical students of the day) and had to leave school for six months to recuperate in a sanitarium. He was advised that his goal of an internship in surgery was too physically demanding and might reactivate his pulmonary lesion. Dr. Philip Bard had just been recruited to Hopkins from Harvard, and he invited Woolsey to work in his lab. Woolsey soon realized that his future lay in physiology and not brain surgery. (In later years, those of us who worked with him through all-night experiments and 14-hour brain surgeries on monkeys were amazed at his enormous energy and robust health.)

During his time at Hopkins, Woolsey married Harriet E. Runion (in 1942). They had three children: Thomas Allen Woolsey, M.D., now a leading neuroscientist; John David Woolsey, M.F.A., an artist and medical illustrator; and Edward Alexander Woolsey, Ph.D., a zoologist and teacher. Woolsey remained at Hopkins in physiology until 1948, when he accepted his appointment as Charles Sumner Slichter professor of neurophysiology at the University of Wisconsin medical and graduate schools in Madison. He remained at Wisconsin for the rest of his career and life.

I had the great good fortune to work in Clinton Woolsey's laboratory for four years from 1955 through 1959. I completed by Ph.D. thesis in 1955–56 in his laboratory (my major professor, W.J.Brogden, in the psychology department did not have facilities for my work, and Woolsey kindly allowed us to use his laboratory). I then spent three years in Woolsey's laboratory as an NIH postdoctoral fellow. It was a most exciting environment. Much of the work in the laboratory at the time focused on the organization of the motor cortex in a series of primates (including chimpanzees) using electrical stimulation and on the organization of sensory (and polysensory) cortical areas using surface-evoked potentials. P.W.Davies visited the lab during that time and described the new extracellular microelectrode technique he and Jerzy Rose had developed. Jerzy Rose visited one summer, and using this technique we completed the first single-unit recording study of the tonotopic organization of the auditory cortex (in cat) (1960).

During my time (and, of course, earlier and later, as well) there were extraordinarily talented scientists in the laboratory. Konrad Akert provided solid expertise in neuroanatomy (e.g., 1961); Joseph Hind was expert in the auditory system and all matters acoustic (1960); and W.I.Welker and Robert Benjamin were young scientists at the height of their productivity (e.g., 1957). There were many others as well (e.g., 1957). Woolsey was a very tolerant laboratory chief. If the work we did was to some degree relevant to cortical organization and functions and was carefully done, we were free to follow our own interests. Personally, Woolsey was a gentle man—I never saw him lose his temper. He was an ideal role model in that he was totally focused on the work (and his family), was objective, and never engaged in ad hominem. However, if you took a particular position on cortical organization, you had better be prepared to defend it. He had very high standards and expected the same of everyone. Morale in Woolsey's laboratory was extremely high.

Woolsey was a superb but infrequent lecturer, often teaching by demonstration. At that time textbooks stated that complete removal of the neocortex in monkeys caused virtual paralysis. In the medical student physiology course Woolsey once demonstrated a fully decorticate rhesus monkey, which he held on a stick chain while the monkey chased him around the lectern trying to bite him. This finding was, of course, much more than simply a demonstration. Travis and Woolsey (1956) showed that after bilateral removal of all neocortex in stages, monkeys could show considerable recovery of motor function and become capable of locomotion if given adequate postoperative physical therapy. Recovery of function following brain injury was of deep interest to Woolsey. I assisted Woolsey in preparation of two of the decorticate macaques (we did them in two stages) and in their postoperative care. Woolsey had developed a method of subpial surgical aspiration of cortex that made it possible to remove localized regions without damage to adjacent regions, or of an entire hemisphere of cortex with minimal bleeding. He was a superb experimental neurosurgeon.

Woolsey became the Charles Sumner Slichter professor emeritus at the University of Wisconsin in 1975, but he by no means retired from his work. He published a landmark paper on localization in somatic sensory and motor areas of the human cerebral cortex in 1979, and until shortly before his death he was hard at work bringing to completion his extensive data on cortical localization in the chimpanzee. I participated in these studies, and he called me, I believe in 1990, to check on some details.

In the course of his long and productive career, Clinton Woolsey received many honors and awards, including Phi Beta Kappa (1928); the Franklin P.Mall Award in anatomy, Johns Hopkins University (1933); National Academy of Sciences membership (1960); the Medal of Faculty of Medicine, Free University, Brussels, Belgium (1968); charter membership in the Johns Hopkins Society of Scholars (1968); an Sc.D. (honoris causa) from Union College (1968); honorary membership in the Academy of Neurosurgery (1973); honorary membership in the American Neurological Association (1975); and the Ralph W.Gerard Award from the Society for Neuroscience, with J.F.Rose (1982). He served on numerous NIH committees and was deeply involved in international scientific activities.

PROFESSIONAL HISTORY

Wade Marshall had joined Philip Bard's department at Johns Hopkins in the 1930s and had worked with the cathode ray oscilloscope in Ralph Gerard's laboratory. He and Albert Grass at Harvard built such equipment for Bard's laboratory, and Marshall, Woolsey, and Bard undertook the first detailed mapping of the somatic sensory area of the cerebral cortex of the cat and monkey, using the new evoked potential technique (1937, 1941, 1942). This formed the basis of much of Woolsey's future work. He mapped the cortical sensory areas (and motor areas) in many mammalian species in great detail. Woolsey had a deep and abiding interest in the comparative development of functional areas of the neocortex, an interest he conveyed to his student and colleague W.I.Welker, who has continued this important tradition to the present. These studies were done with great care and attention to detail and led to a general formulation in the late 1940s of the receptotopic organization of cortical receiving areas, the general plan of which is largely unchallenged to this day.

The power of this comparative approach is clear in the following passage:

Of particular significance in the evolution of these [somatic sensory motor] fields is the central position of the hand areas of SI and MI. In the primates the hand achieves a high degree of corticalization in the precentral and the postcentral fields. Because of the central location of the hand areas, the simple basic pattern of organization seen in the rodent, where the parts are represented in relation to one another much as they exist in the actual animal, apparently becomes distorted in evolution as cortical representation for the hand increases, with the result that in chimpanzee and in man the sensory and the motor face areas lose continuity with the centers for occiput and neck, which remain associated with the trunk representations. In macaque this separation of face from occiput has taken place in the postcentral gyrus, but in the precentral field the motor pattern still hangs together as it does in lower forms. Evidence for a transitional status in the postcentral area in the smooth-brained marmoset has been reported and illustrated elsewhere. That this separation of cortical centers for face and occiput is not the result of an *en bloc* reversal of the projections of the cervical segments upon the cortex as was once suggested (1942), but rather is due to expansion of the hand area and disruption thereby of the cortical pattern, is supported by the finding that the trigeminal nerve projects not only to the lower classical face area but also to the "upper" head area, where not only the occiput but other parts of the head and face are represented (1958, pp. 65–66).

In his initial studies Woolsey focused on the somatic sensory cortex, but he quickly extended the work to auditory and visual areas. It was known that regions of the cochlea responded selectively to different tone frequencies, but little was known about the auditory cortex. Woolsey and Walzl (1942) completed a technical tour de force by selectively stimulating localized regions of auditory nerve fibers in the cochlea and mapping the patterns of evoked responses on the auditory cortex of the cat and monkey. This was the first clear demonstration of tonotopic (actually cochleotopic) organization of the auditory cortex. They followed this by examining effects of cochlear lesions on click-evoked responses in the auditory cortex (1946).

Early in the 1940s Woolsey discovered the existence of a second somatic sensory receiving area in the cortex of the cat, dog, and monkey (1943) and subsequently discovered secondary auditory and visual areas. Both E.D.Adrian and Woolsey are credited with independent discovery of the existence of this second somatic sensory area. Actually, it appears that Woolsey was first. The following is a quote from a letter written to me by Clinton's son Thomas:

I believe Dad and his colleagues in Baltimore discovered a second somatic area independently and about the same time as Adrian. My father rarely expressed disappointments in others. However, I think this is one case where he was both very disappointed and surprised. Evidently, early during the Second World War, Adrian was in the United States and visited the laboratory in Baltimore, Maryland. As Father described it, he [Clinton Woolsey] spent a long time very carefully explaining his discovery of a second somatic area. Father said that Adrian nodded and made comments regarding the data that he was being shown, but said nothing about his own work, which Father was greatly surprised to see published several months later. I think Dad felt betrayed in his confidence. In any case, a review of the data suggests that Dad provided the first convincing evidence of an orderly sequence in a second full representation. Adrian's note had only a few points that were outside the region of what was already known to be the somatic area (SI)" (Thomas Woolsey, personal communication, Nov. 22, 1994).

Woolsey and associates carefully mapped the primary visual area of the cortex, demonstrated the detailed retinotopic organization, and mapped a second visual area (1946, 1950). In yet another series of pioneering studies, Woolsey and associates mapped the somatic sensory projections to the cerebellar cortex (1945) and the organization of projections from the cerebral cortex to the cerebellar cortex (1952).

In still yet another series of pioneering studies, Woolsey joined forces with Jerzy Rose to complete a detailed lesion —retrograde degeneration mapping of the projections from the auditory region of the thalamus (medial geniculate body) to the auditory cortex in light of the physiological organization of the auditory cortex Woolsey and Walzl had defined earlier (1949). Rose and Woolsey completed similar studies on the projections of the mediodorsal nucleus to the orbitofrontal cortex (1947) and on the relations between the anterior thalamic nuclei and the limbic cortex (1948). As noted by Clinton's son Thomas in the presentation statement for the Ralph W.Gerard Award to Woolsey and Rose in 1982, these studies demonstrated that (1) there was a direct correspondence between cortical cytoarchitectonic fields and functionally defined regions of the cortex (this structure-function concept was under attack at the time); (2) each functional and cytoarchitectonic region of cortex received a distinctive input from a specific thalamic nucleus (the concept of thalamotelencephalic dependencies); and (3) these connections either could be restricted or be distributed more widely to several functional and cytoarchitectonic areas (the concepts of essential and sustaining projections).

In an ongoing series of exquisitely detailed studies, Woolsey and associates mapped the primary and supplementary motor areas of the cerebral cortex, using electrical stimulation in a wide range of primates and other mammals (1952, 1957, 1958) and compared sensory and motor maps in both pre-and postcentral cortical areas:

It has now been firmly established that the afferent areas are not strictly afferent nor are the motor areas entirely motor. The afferent areas (SI and SII; postcentral and "second" sensory) have well-organized motor outflows which are still functional months after complete removal of the motor areas of the frontal lobe, while at the same time it appears that afferent connections to the frontal motor areas exist independently of the parietal afferent paths (Figure 1). Thus, the concept that the rolandic region is indeed a sensorimotor system, as held by pre-Sheringtonian workers, is reaffirmed, but with the considerable difference that the region is not an undifferentiated entity but one compounded of a number of distinguishable, individually complete, though interrelated, sensory-motor and motor-sensory representations. These facts appear to us to have important consequences for studies of the role of the cortex in neurological and behavioral functions, studies which will require the close cooperation of anatomist, physiologist, and behaviorist, or the mastery of multiple techniques by single individuals (1958, p. 64).

It is perhaps fitting to close this review of Clinton Woolsey's professional history with an example of his work. Figure 1 is reproduced from Woolsey (1958). It shows the detailed maps of a portion of the postcentral gyrus of the *Macaca mulatta,* comparing the representation of the body surface on the left, obtained from evoked potential maps, to the representation of movements from the same cortical tissue, elicited by electrical stimulation, on the right. The figurines (a style of data representation Woolsey invented and perfected) in both cases indicate 2-mm steps along the cortex. The dark region in each figurine on the left is the region of skin surface that yields the maximum amplitude evoked potential at that cortical locus when the skin is lightly tapped. The dark regions on the figurines on the right are the cortical loci where least-intensity electrical stimulation yields the minimal movement shown. Note the exquisite detail. In the animal on the right (motor map), the cortical motor area (precentral gyrus) had been removed bilaterally, hence there is a well-organized postcentral motor system that can function independently of the frontal motor paths (see quotation just above). Woolsey also notes that

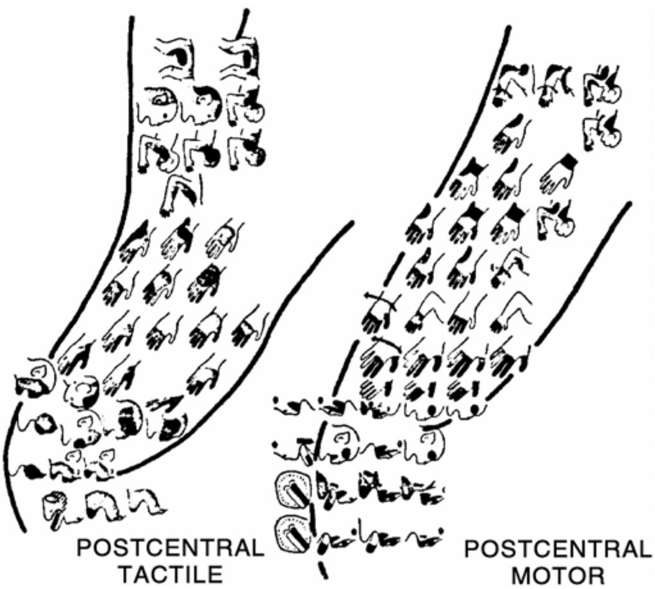

FIGURE 1: Comparison of postcentral tactile localization pattern with the postcentral motor localization pattern of *Macaca, mulatta* (1958).

these two maps are derived from different animals, yet they show remarkable similarities in their patterns of somatotopical organization.

It would be very remiss of me to conclude this biography of Clinton Woolsey without special mention of his wife Harriet. She was totally supportive of his work and career and accepted with profound good nature his incredible work schedule; he would often work all day and night and sometimes longer. Clinton was truly most fortunate. We who worked with him remember with great fondness the evenings at their home. Every summer, when the first sweet corn was ripe in southern Wisconsin, Clinton and Harriet held a corn roast for the laboratory at a local park. Perhaps it was the influence of Harriet and Clinton, but somehow corn has never tasted quite as good since.

I ACKNOWLEDGE MOST gratefully the following documents that provided information, particularly on the early phases of Clinton Woolsey's life: the autobiographical document dated April 10, 1989, that Clinton Woolsey wrote to the National Academy of Sciences; the original nomination (circa 1959) to the National Academy of Sciences; the Ralph W.Gerard Award presentation statement dated November 1, 1982, that Clinton's son Thomas A.Woolsey wrote for the Society for Neuroscience; and a letter dated November 22, 1994, that Thomas A.Woolsey wrote to me. Finally, I have innumerable personal experiences from the time I worked in Clinton Woolsey's laboratory from 1955 to 1959, the high point of my professional career.

SELECTED BIBLIOGRAPHY

1937 With W.H.Marshall and P.Bard. Cortical representation of tactile sensibility as indicated by cortical potentials. *Science* 85:388–90.

1941 With W.H.Marshall and P.Bard. Observations on cortical somatic sensory mechanisms of cat and monkey. *J. Neurophysiol.* 4:1–43.

1942 With W.H.Marshall and P.Bard. Representation of cutaneous tactile sensibility in the cerebral cortex of the monkey as indicated by evoked potentials. *Bull. Johns Hopkins Hosp.* 70:399–441.

With E.M.Walzl. Topical projection of nerve fibers from local regions of the cochlea to the cerebral cortex of the cat. *Bull. Johns Hopkins Hosp.* 71:315–44.

1943 “Second” somatic receiving areas in the cerebral cortex of cat, dog, and monkey. *Fed. Proc.* 2:55–56.

1945 With J.L.Hampson and C.R.Harrison. Somatotopic localization in the anterior lobe and lobulus simplex of the cerebellum of the cat and dog. *Fed. Proc.* 4:31.

1946 With S.A.Talbot and J.M.Thompson. Visual areas I and II of the cerebral cortex of the rabbit. *Fed. Proc.* 5:103.

With E.M.Walzl. Effects of cochlear lesions on click responses in the auditory cortex of the cat. *Bull. Johns Hopkins Hosp.* 79:309– 19.

1947 With J.E.Rose. The orbitofrontal cortex and its connections with the mediodorsal nucleus in rabbit, sheep, and cat. *Res. Publ. Assoc. Nerv. Ment. Dis.* 27:210–32.

1948 With J.E.Rose. Structure and relations of limbic cortex and anterior thalamic nuclei in rabbit and cat. *J. Comp. Neurol.* 89:279– 348.

1949 With J.E.Rose. The relations of thalamic connections, cellular structure and evocable electrical activity in the auditory region of the cat. *J. Comp. Neurol.* 91:441–46.

1950 With J.M.Thompson and S.A.Talbot. Visual areas I and II of the cerebral cortex of the rabbit. *J. Neurophysiol.* 13:277–88.

1952 Patterns of localization in sensory and motor areas of the cerebral cortex. In *The Biology of Mental Health and Disease,* pp. 192–206. New York: P.Hoeber.

With P.H.Settlage, D.R.Meyer, W.Sencer, T.P.Hamuy, and A. M.Travis. Patterns of localization in precentral and “supplementary” motor areas and their relation to the concept of the premotor area. *Res. Publ. Assoc. Nerv. Ment. Dis.* 30:238–64.

With J.L.Hampson and C.R.Harrison. Cerebro-cerebellar projections and somatotopic localization of motor function in the cerebellum. *Res. Publ. Assoc. Nerv. Ment. Dis.* 30:299–316.

1956 With A.M.Travis. Motor performance of monkeys after bilateral partial and total cerebral decortications. *Am. J. Phys. Med.* 35:273– 310.

1957 With W.S.Coxe, J.F.Hirsch, R.M.Benjamin. W.I.Welker, and R. F.Thompson. Precentral and supplementary motor areas in *Ateles. Physiologist* 1:19.

With W.I.Welker, R.M.Benjamin, and R.C.Miles. Motor effects of cortical stimulation in squirrel monkey. *J. Neurophysiol.* 20:347– 64.

1958 With H.F.Harlow. Eds. *Biological and Biochemical Bases of Behavior*. Madison: University of Wisconsin Press.

With J.E.Rose. Cortical connections and functional organization of the thalamic auditory system of the cat. In *Biological and Biochemical Bases of Behavior,* eds. H.F.Harlow and C.N.Woolsey, pp. 127–50. Madison: University of Wisconsin Press.

Organization of somatic sensory and motor areas of the cerebral cortex. In *Biological and Biochemical Bases of Behavior,* eds. H.F. Harlow and C.N.Woolsey, pp. 63–81. Madison: University of Wisconsin Press.

1960 With J.E.Hind, J.E.Rose, P.W.Davies, R.M.Benjamin, W.I. Welker, and R.F.Thompson. Unit activity in the auditory cortex. In *Neural Mechanisms of the Auditory and Vestibular Systems,* eds. G. L.Rasmussen and W.F.Windle, pp. 201–10. Springfield, Ill.: Charles C.Thomas.

Organization of the cortical auditory system: A review and a synthesis. In *Neural Mechanisms of the Auditory and Vestibular Systems,* eds. G.L.Rasmussen and W.F.Windle, pp. 165–80. Springfield, Ill.: Charles C.Thomas.

1961 With K.Akert, R.A.Grusen, and D.R.Meyer. Klüver-Bucy syndrome in monkeys with neocortical ablations of temporal lobe. *Brain* 84:480–98.

1979 With T.C.Erickson and W.E.Gilson. Localization in somatic sensory and motor areas of human cerebral cortex as determined by direct recording of evoked potentials and electrical stimulation. *J. Neurosurg.* 51:476–506.