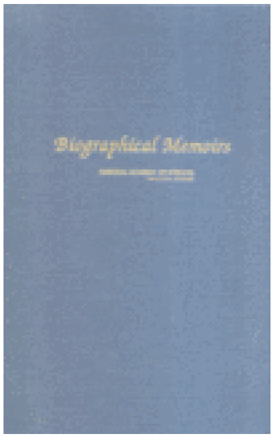

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Biographical Memoirs

VOLUME 86

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CONTENTS

PREFACE

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

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

R. STEPHEN BERRY
Home Secretary

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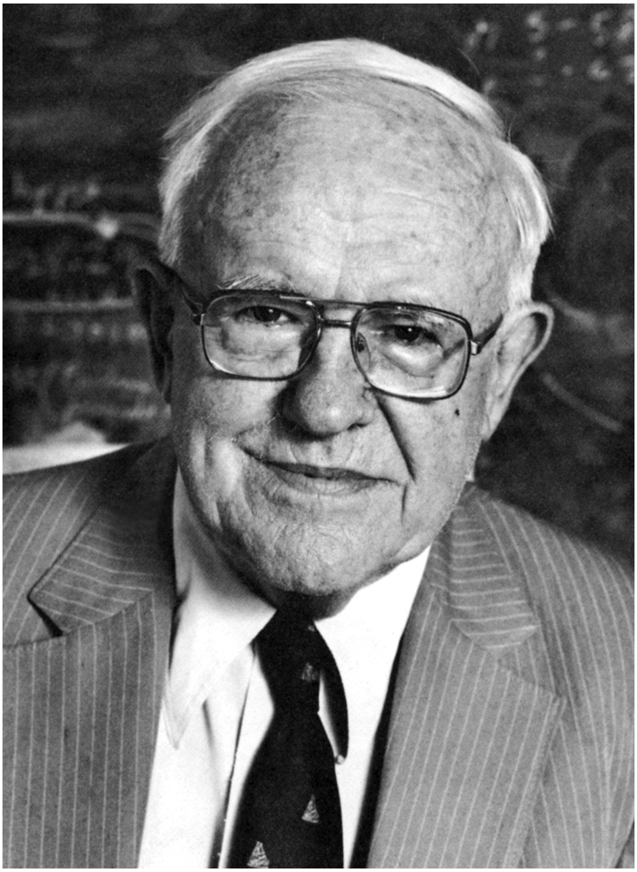

Lawrence H Aller

LAWRENCE HUGH ALLER

September 24, 1913–March 16, 2003

BY MANUEL PEIMBERT

LAWRENCE H. ALLER, after finishing his second year at high school, was dragged away by his father to a primitive mining field in northern California, where he worked helping him in an illusory quest for gold. He literally escaped from this situation to become a brilliant astrophysicist, an expert in the study of planetary nebulae, stellar atmospheres, and the chemical composition of stars and nebulae. Aller was elected to the National Academy of Sciences in 1962.

The comparison between the observed abundances and those predicted by the main astrophysical theories—stellar evolution, galactic evolution, and the evolution of the Universe as a whole—was one of the main drives in the development of astrophysics in the twentieth century. Aller was one of the first astronomers to advocate that differences in the spectra of stars and nebulae were due not only to physical conditions but also to real differences in their chemical composition. A substantial fraction of Aller's research work was devoted to the determination of the chemical composition of stars of different types and of planetary nebulae, which are objects in transition between red giant stars and white dwarfs.

THE FORMATIVE YEARS

Born on September 24, 1913, in Tacoma, Washington, Aller was the son of Leslie E. Aller and Lena Belle, and the youngest of their six children: Leeon, Jane, Paul, Louis, Lee, and Lawrence. His father and grandfather were involved in printing and mining, and his mother had been a schoolteacher before she was married.

His interest in astronomy started early in life. At the age of four he observed the total solar eclipse of 1918, which produced a lasting effect during his life. As a child living in Tacoma, a city by the sea, he was interested in the tides and their causes, and also in world geography. At the age of 10 his mother took him to visit the Lick Observatory, where he bought a small book on astronomy.

During his second year in high school, in Seattle, he borrowed from the public library the book by Russell, Dugan, and Stewart on astrophysics and stellar astronomy. At that time it was not possible to predict that Russell was going to suggest to Aller to write books on astronomy, nor that Aller was going to obtain the Henry Norris Russell award for his life's work on astronomy.

In 1929, at the end of his second year in high school, Aller was taken away by his father to a mining camp in northern California, close to the border with Oregon, not far from a little village called Takilma, where he stayed for two and one-half years, never finishing the high school cycle. With the support of his brother Paul he abandoned the mining camp and went to Oakland to live with his sister Jane and her husband.

Before becoming an undergraduate at the University of California, Aller became a member of the Astronomical Society of the Pacific and engaged in fruitful correspondence with many amateur and professional astronomers, among them Herman Zanstra, Seth B. Nicholson, Adriaan van Maanen, Edison Pettit, and Donald H. Menzel. He read a paper by Menzel on "Hydrogen Abundance and the Constitution of the Giant Planets," which appeared in the August 1930 issue of *Publications of the Astronomical Society of the Pacific.* This paper induced him to write to Menzel, starting a regular correspondence that later on became providential for his astronomical career.

When Aller arrived in Oakland from the mining camp, Menzel was teaching Astronomy I at University of California, Berkeley. Aller met Menzel on November 30, 1931, and took the final exam of Astronomy I and obtained excellent results. Based on the interest shown by Aller and his performance in the exam, Menzel convinced Merton Hill, who was the admissions director at Berkeley, to admit Aller as a special undergraduate student. Menzel left Berkeley for Lick Observatory in December 1931 and later went to Harvard in 1932.

Aller finished his undergraduate work at Berkeley in 1936. He did not go to graduate school immediately because of illness, and in May 1937 he went up to Lick Observatory as a summer assistant assigned to work with Nick Mayall measuring radial velocities of globular star clusters and galaxies using the Crossley telescope. Aller decided to go to Harvard for his graduate education because he wanted to work with Menzel.

Immediately after Aller's arrival at Harvard, in the fall of 1937, Menzel incorporated him into the group of scientists who pioneered in the study of the physical conditions in gaseous nebulae. From 1938 to 1945 Aller collaborated in 12 of the 18 papers of the series "Physical Processes in Gaseous Nebulae." Aller based his Ph.D. thesis on this work and the observations he had obtained at Lick Observatory. Many years later, Menzel compiled a book on the physical processes in ionized plasmas,[1] for which he selected 30 papers on the astrophysical interpretation of spectra of gaseous nebulae, including the 18 papers of the "Physical Processes in Gaseous Nebulae" series and 10 other papers by Aller on the subject.

Aller's graduate preparation on theoretical astrophysics and basic physics was obtained at Harvard, while his observational preparation continued at Lick Observatory, where he went to work in 1938 and 1939 with Nick Mayall. They worked on the rotation curve of the M33 galaxy, and Aller obtained the necessary plates on planetary nebulae for his Ph.D. thesis. For this they used the slitless spectrograph at the Crossley reflector.

Up to 1939 Aller lived on a very tight budget, which along with his relatively humble origins gave him the feeling that he was in the Harvard community but not a part of it. This feeling changed when he was accepted into the Harvard Society of Fellows in 1939. According to Aller, the Society of Fellows was enormously important in his life; thereafter his economic situation improved spectacularly, banishing the severe poverty under which he had lived for so many years. Moreover, he considered that the Monday evening meetings of the fellows and the lunch gatherings on Tuesdays and Fridays at Eliot House, during his three years in the society, did more for his general education than any three years before or since. He was nominated to become a member of the society by Menzel with the support of Harlow Shapley, Bart J. Bok, and John H. van Vleck.

Aller was an instructor of physics at Harvard in the 1942-1943 period. Together with Leo Goldberg, his classmate, he published the first edition of the book *Atoms, Stars, and Nebulae* in 1943; afterward, in 1971 and 1991, Aller alone produced the second and third editions. This book was the precursor of many more written by Aller.

Aller obtained his M.A. in 1938 and his Ph.D. in 1943, both at Harvard University. The title of his doctoral thesis was "A Spectroscopic Analysis of the Planetary Nebulae."

THE ACADEMIC JOURNEY

From 1943 to 1945 Aller participated in the war effort at the University of California Radiation Laboratory, where he was hired as a physicist to work on the electromagnetic separation of the 235 and 238 uranium isotopes. The director of the laboratory was Ernest O. Lawrence, and Aller was assigned to the group under the direction of Harrie S. W. Massey. During this period he managed to observe at Lick Observatory three days in a row every three weeks. As the war was ending, in June 1945, he was one of the first to be dismissed. Fortunately, he already had a job offer from Indiana University.

Aller worked as an assistant professor at Indiana from 1945 to 1948. During his stay at Indiana he wrote the first draft of the books, *The Atmospheres of the Sun and the Stars* and *Nuclear Transformations, Stellar Interiors, and Nebulae*. These books were written at the suggestion of Henry Norris Russell. At Indiana there were no observing facilities suited to the spectroscopic work needed by Aller. But the university obtained telescope time from the McDonald Observatory in Texas through an agreement between the universities of Chicago and Texas and Indiana University. Frank Edmondson showed Aller how to use the 82-inch reflector and the spectrographic equipment at McDonald Observatory.

Aller received an invitation in 1948 from Leo Goldberg to accept a position at the University of Michigan as an associate professor. There were two main reasons why he accepted the position at Michigan; his Indiana colleagues Edmondson and James Cuffey worked in quite different fields from those of Aller and, on the other hand, at Michigan there were a number of people with interests and backgrounds much closer to Aller's. Besides Goldberg, there were Keith Pierce, Helen Dodson, Orren Mohler, and Robert McMath in the solar research field, and Dean B. McLaughlin, the stellar spectroscopist. In 1954 the University of Michigan promoted him to professor.

During the 1948-1962 period Aller consolidated his reputation as a scientist and helped develop the Michigan graduate program. One of his main activities was the production of books for the popularization and teaching of astronomy. *The Atmospheres of the Sun and the Stars* appeared in 1953 and was revised a decade later. *Nuclear Transformations, Stellar Interiors, and Nebulae* appeared in 1954. In 1956 he published *Gaseous Nebulae*, and in 1961 he published the last of the Michigan books, *The Abundance of the Elements*.

The lack of adequate observing facilities at Indiana and afterward at Michigan led Aller to participate actively as a guest investigator at the Mount Wilson Observatory in the 1945-1982 period. According to Aller, in those days the Mount Wilson Observatory played the role of a de facto national observatory; he also thought that an essential contribution to his success in astronomy was due to the guest investigator program offered by the Mount Wilson Observatory. Most of his research work during his Michigan years was associated with Mount Wilson and a good fraction was done in collaboration with Olin C. Wilson, Rudolph Minkowski, Ira S. Bowen, and Jesse L. Greenstein, all of them staff members at the Mount Wilson and Palomar observatories.

His 1951 paper with Joseph Chamberlain on the atmospheres of A-type subdwarfs was chosen as one of the twentieth-century's most influential papers in astronomy. In the words of George Wallerstein, "The Chamberlain and Aller paper opened a huge field of research on the composition of metal-poor stars and the nucleosynthesis of the species found in such stars."[2] This paper provided definitive evidence of chemical abundance differences among stars.

The solar composition has generally been taken as the basic yardstick for chemical composition comparisons and for the study of the chemical evolution of our Galaxy and other galaxies. The 1960 paper with Goldberg and Edith Muller and the 1976 paper with John Ross were standard references for solar chemical composition from the 1960s to the 1980s.

Aller spent three sabbatical years in Australia as a visiting professor: in 1960-1961 at the Australian National Observatory, in 1968-1969 at Sydney University and the University of Tasmania, and in 1977-1978 at the University of Queensland. In 1960 Donald J. Faulkner, then a graduate student from the University of Queensland, was assigned to work with him. Most of their effort was devoted to the study of planetary nebulae in our Galaxy and of nebulae embedded in regions of recent star formation in our Galaxy and the Magellanic Clouds, the closest galaxies to our own. The observations were obtained at Mount Stromlo Observatory, which was under the direction of Bart Bok. Aller often mentioned that observing the treasures of the southern sky was one of the most thrilling episodes in his life. During the 1970s, in a cooperative program with Douglas Milne, Aller observed planetary nebulae with the Mills Cross at Molonglo and with the 64-meter dish at Parkes.

At the end of the summer of 1961 Daniel Popper convinced the University of California to accept the establishment of a new Ph.D. program in astronomy at the Los Angeles campus. Shortly afterward, Popper offered Aller a position as professor, which he immediately accepted, arriving at UCLA in 1962. Aller finally had come back to the University of California and to Lick Observatory. He had for the first time in his life direct access to first-class instrumentation.

In the 1963-1968 period he chaired the UCLA astronomy department and was instrumental in the consolidation of the Ph.D. program.

He was named professor emeritus in 1984, but this distinction did not imply the end of his academic career. He published his book *Physics of Thermal Gaseous Nebulae* in 1984, continued to teach into the mid-1990s, and kept doing research until the end of his life. A prolific writer and researcher, Aller produced 346 research papers during seven decades; his first one was published in 1935, and his last one in 2004. In addition to his productivity in research, Aller published a substantial number of advanced textbooks and monographs on fundamental astrophysical topics. These have had a vital influence on the academic development of young astronomers, both in the United States and abroad.

COLLABORATORS AND HONORS

The number of his collaborators was very large and included colleagues, students, and former students. A selected list of his main collaborators follows: D. H. Menzel, R. Minkowski, L. Goldberg, J. L. Greenstein, I. S. Bowen, O. C. Wilson, Jun Jugaku, E. A. Muller, Merle F. Walker, William Liller, D. J. Faulkner, Lindsey F. Smith, Jim B. Kaler, J. E. Ross, Stanley J. Czyzak, Charles D. Keyes, Karen B. Kwitter, Ben Zuckerman, Walter A. Feibelman, Francis P. Keenan, and Siek Hyung.

I first met Aller in 1964, and over the years we attended many of the same international astronomical meetings. Over four decades, I had the privilege to discuss matters of gaseous nebulae, astronomy in general, and global problems. He was always a sincere and passionate interlocutor. He was a man with strong views on social and political issues. He wanted a peaceful world where most of the resources would go into finding universal employment and education for all.

He was a visiting professor or a guest professor at a large number of institutions in the United States and abroad. He was elected to the American Academy of Arts and Sciences in 1961. He was a director of the Astronomical Society of the Pacific (1974-1977). He received the prestigious Henry Norris Russell Lectureship (1992) of the American Astronomical Society, awarded annually to commemorate a lifetime of preeminence in astronomical research. He received the Russell Prize for his research in the astrophysical study of gaseous nebulae, the chemical analysis of stars, and the analysis of the solar photosphere.

Since 1967 there have been eight international symposiums on planetary nebulae at five-year intervals. Aller participated in the first seven, giving invited reviews at all of them and sending his invited contribution to the eighth one. During the fourth meeting, which took place at University College London in 1982, I had the pleasure to give on behalf of the scientific organizing committee two academic medals, one to Michael Seaton and the other to Lawrence Aller, for their lifelong contributions to the study of planetary nebulae. The proceedings of the eighth symposium on planetary nebulae, which was sponsored by the International Astronomical Union, were dedicated to Aller's memory.[3]

Aller kept his initial love for planetary nebulae during his entire life. Most of his papers were dedicated to the study of the physical conditions in planetary nebulae and in particular to their chemical composition. His passion for astronomy was maintained during his whole life, and regardless of severe physical handicaps he kept working in astronomy until the end of his life.

In 1941 he married Rosalind Duncan Hall, who survives him. They had three children: Hugh, an astronomer; Raymond, a physician; and Gwendolyn Foster. One of his four grandchildren, Monique Aller, is a graduate student in astronomy.

THIS MEMOIR is based mainly on the scientific research papers and textbooks by Lawrence Aller, personal recollections, the autobiographical essay written by Aller,[4] the interview by David DeVorkin,[5] the obituary written by Jim Kaler,[6] and valuable information provided by Donald Osterbrock, Jim Kaler, Susan Grodin, and Benjamin Zuckerman.

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HARRY BEEVERS

January 10, 1924–April 14, 2004

BY MAARTEN J. CHRISPEELS

Harry Beevers's career of 50 years spanned the emergence of plant metabolism as a discipline, and he was one of its major contributors. The most notable achievement of his research group was the discovery of the glyoxylate cycle in seedlings of plants that store fat in their seed and utilize this fat as a source of energy and for the production of glucose during early seedling growth. These studies culminated in the demonstration that the glyoxylate cycle of fat-storing seeds is located in a specific metabolic compartment, the glyoxysome. In the course of this work he trained a large cadre of Ph.D. students and postdoctoral scholars from around the world, all of whom remember him fondly. He was an excellent and quick-witted public speaker who was always in demand as an after-dinner speaker/entertainer. With his repertoire of songs, the lyrics for many of which he had composed, he was the life of the party. He enjoyed life and was known to muse, "that young scientists shouldn't take themselves so seriously." In addition to being an outstanding researcher, Harry was a rigorous and much beloved teacher both in the classroom and in the laboratory. His achievements and dynamism are all the more remarkable considering he had to cope with diabetes for much of his career.

FAMILY MATTERS AND THE SOURCE OF HARRY'S BIOLOGICAL INSPIRATION

Harry Beevers was born on January 10, 1924, the second of eight children, in Shildon, a small industrial town in County Durham, England. When Harry was six, his family moved to the rural area of Upper Weardale. Although his parents did not have much formal education, they greatly encouraged the education of their children, and six of the eight received university degrees. Harry attended elementary schools at Wearhead and St John's Chapel and later attended the Wolsingham Grammar School (a "public" school in the U.S. sense). Attendance at grammar school required a 30-mile roundtrip, originally by rail and later by bus. While at grammar school, he met Jean Sykes, whom he married in 1949. According to his brother Leonard, Harry initially wanted to become a schoolteacher to instruct in woodworking and arts and crafts, but due to wartime shortages of materials the school lacked the supplies to teach those courses. During his secondary education, Harry was inspired by David Hughes, a dynamic biology teacher who took his students on field trips to study the local ecology of the nearby moors and the relics of the Alpine flora in the Upper Teesdale. Mr. Hughes encouraged independent projects, and with a microscope borrowed from the school, Harry analyzed the fauna and flora of local ponds. This early immersion in biology provided the inspiration for a life of scientific inquiry.

In 1942 Harry entered the accelerated wartime university program and received a B.Sc. first-class honors degree in botany from King's College in Newcastle upon Tyne (then part of Durham University). During the war, he performed nighttime fire-watching duties on the university campus in the company of the resident faculty, which included Professor Meirion Thomas, who later became his Ph.D. mentor. Harry supplemented his meager student grant by beating grouse on the moors for the local gentry and collecting rose hips that were used to make the vitamin-C-rich syrup that fortified the British wartime diet at a time when fruits and vegetables were scarce. Following the completion of his B.Sc. degree, Harry's military service was deferred, and he started his doctoral research, which he completed in 1946.

THE EARLY YEARS: UNDERSTANDING RESPIRATION

Ever since the work of de Saussure in the early nineteenth century, scientists have used gas-exchange measurements as a way to understand metabolism. Determination of the respiratory quotient—the volume of CO_2 released to the volume of O_2 absorbed—of plant organs allowed plant physiologists to make certain deductions about the metabolic processes involved. With complete oxidation of hexose, the respiratory quotient (RQ) is equal to 1, but respiration of fat results in an RQ value of less than 1. The RQ of the leaves of *Bryophyllum* (a succulent) kept in the dark was known to be less than unity, and Meirion Thomas had suggested that this might be caused by an active process in which CO_2 was converted to organic acids. He came to this idea by examining the literature on the nonphotosynthetic fixation of CO_2 by bacteria. The acidic taste and accumulation of organic acids during the night in the leaves of certain plants was known since Roman times, and the day/night cycle of acid accumulation during the night and its breakdown during the day and interconversion to carbohydrate had been studied in many laboratories. Harry was able to confirm this postulated CO_2 fixation process by growing plants in 5 percent CO_2 and measuring CO_2 uptake volumetrically and acid production by titration. The high level of CO_2 prevented the normal de-acidification during the day and even induced acid formation in the light in plants that had been de-acidified. These discoveries led Harry to a lifelong interest in plant metabolism. This type of carbon dioxide fixation is now known to be part of crassulacean acid metabolism, or CAM, photosynthesis, a form of photosynthesis that is found in many desert plants.

At this point a small digression about Professor Meirion Thomas is in order. Dr. Thomas (1894-1977) was known in his Welsh village as "Thomas the Book" because he wrote a classic textbook entitled *Plant Physiology* that saw five editions between 1935 and 1973. This book inspired generations of British and colonial students of plant biology. He had a very active laboratory where serious experimentation was conducted in an intellectually stimulating environment. Other influential British plant physiologists who were trained there include J. W. Bradbeer, David A. Walker, R. G. Paxton, R. F. Lyndon, and J. M. A. Brown. The lab was apparently a fun place to be in those somewhat drab years immediately after World War II. The Newcastle laboratory group served as an excellent model for the laboratory groups that Harry created later on, first at Purdue University and subsequently at the University of California, Santa Cruz.

From Newcastle, Harry moved to Oxford University, where he was first assistant and then chief research assistant in plant physiology in the medicinal plant research laboratory of W. O. James, an authority on plant respiration. Harry was given the job of looking into the biosynthesis of tropane alkaloids. The biosynthesis of alkaloids involves catechol oxidase, an O_2-consuming enzyme, and this was the subject of some of Harry's work. At that time the available tools were not up to the task of making real progress in understanding the biosynthesis of these complex secondary metabolites. Harry turned his attention to other scientific problems, including the unusually high rate of respiration of the spadix of *Arum maculatum*. The high rate of oxygen consumption of this organ was found to be resistant to poisoning by cyanide, and Harry concluded that the cytochrome system must not be operational in spadices. Much later it was shown that an "alternative" oxidase, which is cyanide insensitive, shunts electrons to oxygen and that this pathway operates at its highest level when cytochrome oxidase is inhibited by cyanide.

At Oxford, Harry collaborated with Eric Simon on the uptake of weak acids and weak bases by plant organs, and in 1952 they published a paper that would remain useful for years to come. They found that the response of plant cells to weak acids increases when the incubation medium has a pH value below the pK of the weak acid (in other words, when the acid is protonated) and that the reverse held for weak bases, which were more active under mildly alkaline conditions. As many investigations depended on incubating plant organs with inhibitors that were often either acids or bases, these observations proved extremely useful.

Radioactive $^{14}CO_2$ was becoming available in the United States, and several laboratories were beginning to use it to unravel the path of carbon in photosynthesis. Harry recognized the power of this approach to help answer the questions on which he was working after he attended a meeting on CO_2 fixation organized by the Society for Experimental Biology in Sheffield in 1950. He saw that job prospects were limited in postwar England, and R. E. Girton, a plant physiologist from Purdue University, who was spending a sabbatical year in the laboratory of W. O. James, got Harry a one-year appointment as a visiting assistant professor in the Department of Biology at Purdue University. Harry and Jean packed their bags barely a year after being married and set sail for the New World. Soon after arriving in the United States, Harry attended his first meeting of the American Society of Plant Physiologists (Columbus, Ohio, 1950), and there he met all the important players of the discipline. Throughout his life he remained a staunch supporter of the society and its journal *Plant Physiology*. Eleven years later his younger brother Leonard, who had also become a plant physiologist interested in metabolism, would follow the same route across the Atlantic. Leonard and his wife, Pat, came to the United States in 1961, and he focused his research on nitrogen metabolism, leaving carbon metabolism to Harry. Leonard became a well-recognized plant biochemist in his own right.

THE DISCOVERY OF THE GLYOXYLATE CYCLE IN PLANTS

At Purdue, Harry took up the problem of respiration again. At that time (1950) it was not at all clear that the tricarboxylic (TCA) cycle was operating in plants as part of respiration. The reason for this doubt was that malonate, a well-known inhibitor of the TCA cycle in animals, often did not inhibit respiration in plant organs. Having studied the entry of weak acids into plant cells, Harry lowered the pH of the incubation medium allowing malonate to enter the cells, and under those conditions respiration was inhibited. Pyruvate consumption in the TCA cycle was prevented, and pyruvate was diverted to ethanol. Some of these experiments were carried out with Martin Gibbs (later also elected to the National Academy of Sciences) and involved ^{14}C precursors. Gibbs had been hired by the Biology Department of the Brookhaven National Laboratory with the charge of producing ^{14}C-labeled metabolites that could be used by others (and by himself) in research. Harry spent the summer of 1953 with his family in Brookhaven and worked with Gibbs. By using labeled sugars they dispelled doubts that both plants and yeast use the same oxidative pathway that animal cells use. In a later collaboration with Gibbs, labeled sugars were used to demonstrate the operation of the pentose phosphate pathway in plants. With labeled precursors Harry's group pioneered the demonstration of pools of metabolites later shown to be attributable to subcellular compartments in the plant cell.

It was about this time that Harry decided to work on the conversion of fat to sugar in castor bean seedlings. It had been known for some time that castor beans store fat and that they have a low RQ and quantitatively convert fat to sugars (which accumulate primarily as cell walls in the seedling). This quantitative conversion of fat to carbohydrate had been shown 20 years earlier by the work of J. R. Murlin (*J. Gen. Physiol.* 17[1933]:283-302). The unknown metabolic pathway intrigued Harry, and the availability of ^{14}C-labeled compounds made it possible to investigate the problem. Although castor bean may seem an odd choice of experimental material in this day and age of molecular genetics on the one hand and national security on the other, castor beans were a major crop then and readily available. Harry purchased them from the Baker seed company in Vernon, Texas. Initial work centered on feeding labeled precursors to castor bean endosperm and analyzing the products, and on isolating mitochondria and studying their metabolic properties. This work was carried out by two visitors to the lab: David A. Walker and Takashi Akazawa.

Akazawa recalled that every morning he would prepare mitochondria using the only preparative Beckman ultracentrifuge then available on the Purdue campus and carry the preparation to Harry's lab for further studies using a Warburg apparatus. Although others had reported the isolation of plant mitochondria as early as 1951 (Adele Millerd in James Bonner's laboratory at Caltech), the soft castor bean endosperm proved to be a much superior tissue for the isolation of fragile organelles. Somewhat surprisingly, the labeling patterns produced by feeding various labeled compounds to tissue slices or isolated subcellular fractions of castor bean endosperm were not always readily interpretable by, or consistent with, the then-established metabolic pathways.

The "aha!" moment came when Harry was on sabbatical leave in 1956-1957 at Oxford University, where Hans Krebs occupied the chair of biochemistry. According to Hans Kornberg, Harry met Hans Krebs socially at a college function and explained to him his interest in the conversion of fat to carbohydrate; Krebs then suggested that Harry should collaborate with Kornberg. This was the time when the glyoxylate cycle was being formulated in bacteria. Hans (Kornberg) and Harry then collaborated to demonstrate that the two enzymes that characterize the glyoxylate cycle (malate synthase and isocitrate lyase) are indeed present in the endosperm of castor beans. This discovery set the direction of Harry's career for the next 25 years. To prove that the cycle was operating in castor bean endosperm, David Canvin examined the fate of C1- and C2-labeled acetate, and the labeling patterns he obtained were in agreement with the postulated pathways: Succinate produced from acetate served as a precursor for glucose by a reversal of glycolysis. Canvin's work provided the rigorous "proof of the pudding" that Harry loved!

A NEW METABOLIC COMPARTMENT: THE DISCOVERY OF GLYOXYSOMES

The next breakthrough came when Bill Breidenbach joined the laboratory as a postdoctoral researcher. It had been assumed that the glyoxylate cycle was operating in mitochondria, because three of the five enzymes needed for the cycle occur in mitochondria. However, there was circumstantial evidence to indicate that a different subcellular compartment might be involved. Widmar Tanner, a graduate student, had found numerous single-membrane-bounded organelles in his electron micrographs of castor bean endosperm, and compartmentation of the glyoxylate cycle in distinct organelles had been observed in Kornberg's lab in *Chlorella* and *Tetrahymena.* Breidenbach applied his expertise with linear sucrose gradients to endosperm homogenates, and in a paper published in 1967 he showed that mitochondria banded at a sucrose density of 1.19 g/ml, whereas the two glyoxylate cycle enzymes banded in a different organelle fraction at 1.25 g/ml. This fraction also contained the other three enzymes needed to complete the cycle (and also found in mitochondria). Examination by microscopy showed them to be similar to the organelles observed by Tanner. Beevers and Breidenbach called these organelles glyoxysomes. The announcement by Harry that he had discovered a new class of organelles in plant cells was made at the opening of the new facilities of the Atomic Energy Commission Plant Research Laboratory at Michigan State University in the spring of 1967.

Because glyoxysomes constituted a new metabolic compartment, they became a hot research topic. The race was on to understand their biogenesis, and to define other pathways that might be found in them. The laboratories of Paul Stumpf and Harry Beevers discovered simultaneously that glyoxysomes contain the enzymes for the β-oxidative breakdown of fatty acids. Leaves had been shown to possess structurally similar organelles termed peroxisomes, and Edward Tolbert's laboratory discovered that these contain glycolate oxidase, an enzyme essential for photorespiration. Glyoxysomes and peroxisomes were found to contain catalase, an enzyme that efficiently disposes of the hydrogen peroxide generated in the β-oxidation and photorespiration pathways. These findings raised questions of the relationship between the organelles. Are they discrete but related structures or did they evolve one into the other?

In the mid-1960s the study of peroxisomes in mammalian cells was initiated and propelled by Christian de Duve, a future Nobel laureate, but the research had stalled: The organelle was thought to be an evolutionary remnant of a primitive respiratory compartment in which substrates were oxidized without the benefit of ATP production. The discoveries made by Beevers and others that this was an important metabolic compartment in plant cells resuscitated research on mammalian peroxisomes. It was not until 1973 that the presence of fatty-acid β-oxidation was reported in mammalian peroxisomes. In mammalian cells peroxisome formation is induced by hypolipidemic drugs, and defects in peroxisomal enzymes or biogenesis are the basis of a number of human genetic diseases.

In 1969 Harry Beevers was elected to the National Academy of Sciences. That was also the year that Professor Kenneth Thimann, a member of the Academy, persuaded Harry to leave the shores of the Wabash and the weekly cheering of his favorite Boilermakers football team and join the biology faculty of the new campus of the University of California, Santa Cruz. Harry and Jean packed their bags once more and departed for the redwoods. There, Harry continued the study of metabolic compartmentation in plant cells.

The ontogenetic relationship between glyoxysomes and peroxisomes could readily be investigated in the cotyledons of certain fat-storing seeds such as cucumbers. When cucumber seedlings grow, they metabolize fat (just as castor beans do) but when exposed to light, they become photosynthetic and produce peroxisomes that contain the enzymes of photorespiration. Are these peroxisomes produced *de novo,* as proposed initially by Harry, or do they evolve from glyoxysomes by the simultaneous export/degradation of certain enzymes and the import of new ones? The question was hotly debated and eventually resolved in favor of a gradual change in enzyme composition. Resolving this question required the use of biochemical techniques as well as immunocytochemistry to study the enzymic complement of both peroxisomes and glyoxysomes in greater detail, work carried out by Anthony Huang, Roland Theimer, and Takashi Kagawa in Harry's laboratory.

MORE METABOLIC COMPARTMENTS: PLASTIDS AND VACUOLES

Harry's interest in the subcellular compartments that contribute to the conversion of fat to carbohydrate led him to study the role of both plastids and vacuoles in this process. With respect to plastids, he was interested in finding out whether the gluconeogenic process occurred in the plastids or in the cytosol. His interest in vacuoles stemmed from earlier work by David MacLennan, who showed in his laboratory that in roots, malate formed by the dark fixation of carbon dioxide ends up in vacuoles with very different kinetics compared with malate formed by feeding acetate into the glyoxylate cycle. (Remember that Harry worked on malate synthesis during his doctoral research.) Such anomalies tweaked Harry's interest in the role of the vacuole as an active metabolic compartment. Because chloroplasts and vacuoles are large organelles and fragile as well, they are notoriously difficult to isolate even if plant organs are chopped with razor blades rather than ground in a mortar. Working in Japan, Mikio Nishimura developed a technique that permitted the isolation of organelles from protoplasts, which were obtained by digesting small pieces of plant organs with cellulolytic enzymes. Harry learned of this technique while a visiting professor in the laboratory of Takashi Akazawa. After Nishimura came to Santa Cruz, he applied his technique to castor bean cotyledons and was able to isolate a pure preparation of plastids and protein storage vacuoles (then called protein bodies). The research with isolated plastids led to the conclusion that gluconeogenesis occurs in the cytosol. The work with isolated protein storage vacuoles led Nishimura in a different direction. Although vacuoles had been known (from the early work of Philippe Matile) to contain certain hydrolytic enzymes, the research of Nishimura and Beevers provided the first biochemical demonstration that they contain a host of hydrolases and that vacuoles can digest the proteins stored within. During seedling growth, the matrix of the protein storage vacuoles is digested and the vacuoles, which initially are quite dense, become light and translucent.

Another plant metabolism strand that runs through Harry's work in the 1970s concerns the biosynthesis of phospholipids and the relationship between the endoplasmic reticulum—the site of many lipid biosynthetic enzymes—and glyoxysome biogenesis. Electron micrographs of both plant and animal cells published at that time suggested that microbodies arose by budding the endoplasmic reticulum (ER), and research in the Beevers lab was aimed at providing biochemical evidence for this process. Michael Lord decided to look at the intracellular site of phosphatidyl choline synthesis because this is the major lipid of the endomembrane system of the plant cell. He identified the ER as the site of the final catalytic step in the synthesis of this phospholipid and in the process he developed the magnesium shift technique that has been useful to ER researchers ever since. When two lots of tissues are homogenized in parallel, one in a medium with 1 mM EDTA and the other in a medium with 2 mM magnesium chloride, and when the homogenates are fractionated on parallel sucrose gradients, the ER turns out to have a very different density in the presence of EDTA, because ribosomes are removed from the ER. This work led to many insights into the metabolism of lipids in plants, but the budding hypothesis could not be confirmed. Harry's interest in compartmentation and metabolism extended to mechanisms of metabolite transport both intracellularly (import into mitochondria, for example) and between tissues or organs (absorption of metabolites by the cotyledons from the self-digesting endosperm). In the mid-1980s Nick Kruger in Harry's laboratory extended work done by others on the role of fructose 2,6-bis phosphate in carbohydrate metabolism. Basically, Harry loved metabolic biochemistry. The great thing about biochemistry, he used to say, was that you could really nail something, in contrast to a field like, say, nuclear physics. "That's NUClear, not UNClear physics!" he would explain.

Harry constantly took exception to the intrusion of teleology into science. One of his pet peeves related to the assignment of strategies to plant functions. In a departure from the majority of his research, which was dependent upon subcellular fractions of plant cells, Harry used whole plants to refute the purported functions of plant transpiration. In a series of experiments Widmar Tanner and Harry demonstrated that illuminated whole corn plants under nontranspiring conditions did not overheat and die and still took up and transported mineral nutrients. Thus, transpiration may not function to cool the plant and transpiration is unnecessary for mineral transport.

Throughout these many years at Purdue University, UC Santa Cruz, and later in retirement, Jean remained Harry's steadfast companion, and she survives him, living in Carmel, California. Harry is also survived by his son, Michael, born in West Lafayette in 1951. Jean holds a B.Sc. degree from King's College, Durham University, but as was customary in those days, she did not pursue a career after marriage. She provided the warm welcome and the hospitality for all the U.S. and the many foreign Ph.D. students and postdocs who passed through Harry's lab. I can only imagine what those lab parties must have been like at the Beeverses' home with Harry dipping into his large repertoire of songs. With love and affection Jean tirelessly met the demands of Harry's diabetic condition and the health consequences of the prolonged treatment.

No account of Harry's contributions to science would be complete without the lyrics of at least one of his scientific songs. Indeed, he used these songs not only to liven up parties or conferences, but also in his lectures, as they made important points about scientific techniques or results. The lyrics below were composed by Bernie Axelrod and Harry in the summer of 1951 and refer to the use of a Warburg apparatus in combination with a culture of *Leuconostic mesenteroides.* This microbe allows one to determine the distribution of radioactivity in the various carbon atoms of glucose. The counter referred to is an early radioactive decay counter in which a light flashed with each decay event that was registered. The lyrics are set to the tune of "Clementine."

> Leuconostoc in the side arm, glucose in the center well
> Tip it in with phosphate buffer, carbon 1 comes off like hell
> Through the use of Leuconostoc we have carbons 1 through 6
> Pure and unadulterated, they don't mingle, they don't mix.
>
> Refrain:
>
> To the counter, to the counter, to the counter like a shot!
> Turn the switch on, see the lights flash! Is it cold or is it hot?

Like all scientists of some distinction, Harry did his fair share of administrative work and science reviewing and evaluating. Notable in this regard is that he was elected president of the American Society of Plant Physiologists, serving in 1961-1962. His most important act that year was to appoint Martin Gibbs as the editor of *Plant Physiology*, the society's journal. In 1970 the society awarded Harry its highest honor, the Stephen Hales Prize, "in recognition of his outstanding studies of glyoxylate metabolism and glyoxysomes."

ACKNOWLEDGMENTS AND NOTES

I knew Harry quite well, but I could not have written this piece without the information supplied by quite a few of Harry's friends and collaborators, including David Walker, Hans Kornberg, Martin Gibbs, Widmar Tanner, Nick Kruger, Anthony Huang, Michael Lord, Takashi Akazawa, Mikio Nishimura, and Joe Chappell, and by Harry's brother Leonard and his UC Santa Cruz colleague Lincoln Taiz. Harry wrote a prefatory chapter published in the *Annual Review of Plant Physiology* and *Plant Molecular Biology* (44[1993]:1-12) that contains many interesting details of his career. Another useful source of information is an article written by Tom ap Rees in *Molecular Approaches to Compartmentation and Metabolic Regulation* (A. H. C. Huang and L. Taiz, eds., pp. 22-29, 1991, American Society of Plant Biologists). An interesting source of Harry's early work with Martin Gibbs is recorded in "The Summer of '51" (M. Gibbs, *Biochem. Biophys. Res. Comm.* 312(2003):81-83).

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F. A. Bovey

FRANK ALDEN BOVEY

June 4, 1918–January 19, 2003

BY FREDERIC C. SCHILLING AND ALAN E. TONELLI

THE NAME OF Frank Alden Bovey will always be associated with nuclear magnetic resonance spectroscopy of polymers. Frank published some of the earliest scientific papers demonstrating the use of NMR to reveal detailed structural information of synthetic and natural polymers. Through his textbooks he introduced several generations of scientists to the power of NMR spectroscopy. At the time of his retirement Frank had published nearly 200 original publications and authored or coauthored nine books. Frank was an associate editor of *Macromolecules* (the preeminent research journal in polymer science) for more than 25 years, beginning with its first publication in 1968. The significance of his research is reflected in the many awards bestowed upon him, including the Polymer Chemistry Award (1969), the Nichols Medal (1978), and the Applied Polymer Science Award (1983), all from the American Chemical Society; the High-Polymer Physics Award (1974) of the American Physical Society; the Carothers Award (1991); and the Silver Medal (1991) of the Japanese Polymer Society. He was elected to the National Academy of Sciences in 1975.

Frank is remembered foremost as a modest gentleman of great intellect. His personal style was rather formal, from his Brooks Brothers clothing to his interactions with colleagues. However, he was always open to new ideas, treated everyone with respect, and appreciated a good sense of humor.

EARLY LIFE, EDUCATION, CAREER, FAMILY, AND PERSONAL REFLECTIONS (BY FREDERIC C. SCHILLING)

Frank Alden Bovey was born on June 4, 1918, in Minneapolis. His family traces its roots back to John Alden and Priscilla Mullins, who came to America aboard the Mayflower in 1620. Longfellow made famous this same Priscilla in his poem "The Courtship of Miles Standish." For seven generations the Alden line came down only through women, until 1856 when Hannah Caroline Brooks married Charles Argalis Bovey (Frank Bovey's grandfather).

Charles's father had come to America as a young man from the little town of Bovey Tracey in Devonshire, England. In 1869 Charles Bovey moved his family (six children) to Minneapolis. He played a major role in the commercial and civic development of the city, attempting to bring the cultured lifestyle of New York and Boston to this Midwestern community. In Minneapolis he established one of the first lumber companies in the area. Later he helped develop a small townsite in the Western Mesabi iron range, which had been recently discovered. Against the many protestations of Charles, the inhabitants insisted on naming the town Bovey. Charles, being a teetotaler and a very religious man, objected to the use of the Bovey name for a booming mining town with many saloons.

The family of Charles Bovey and descendants remained in Minneapolis. John Bovey (one of Charles's sons and Frank's father) also worked in the lumber business and early in the 1900s married Margaret Jackson. They and their three children (John, Frank, and Eugenia) lived in a beautiful brick colonial home that remains today and is used as a place to host catered parties. During Frank's childhood years, the family spent the winter months in Minneapolis. However, when the hot summer months arrived, the Bovey families all moved out to Lake Minnetonka, where his father, uncles, and grandfather commuted to work by streetcar, following the custom of the wealthy and influential families of Boston and New York.[1]

The family of John Bovey suffered financial misfortunes common to many in the 1920s and 1930s. The lumber firm was lost in bankruptcy. Frank's father struggled but managed a transition from lumber to investment banker, and Frank's mother, Margaret, against all feminine precedent, conducted a successful insurance business. As a result the family was able to maintain a semblance of the lifestyle the Bovey families had come to know.

As a young boy Frank showed an early interest in art, in particular painting. As a child he drew hundreds of cartoons. Frank and his older brother, John, loved to read, while their sister, Genie, became the athlete and social butterfly of the family. The children inherited from their parents and grandparents the Bostonian disposition toward literature and reflection. They "went East to college," the boys going to Harvard and their sister to Smith.

At Harvard, Frank and John shared a Model T Ford, which they drove back and forth between Cambridge and Minneapolis. This experience probably began Frank's lifelong love affair with cars. Later in life he always had two or three cars: a Cadillac, his car of choice for the family and for long trips, and a small sports car just for fun, from the red 1957 MGA roadster to the red Nissan 300Z.[2]

Knowing he had to make a living, Frank chose to study chemistry at Harvard, although he would have preferred English literature. While he chose to make his professional life that of a scientist, his interest and study of the arts continued throughout his life.

While studying at Harvard, Frank met Shirley Elfman, and after graduation in 1940 they married and returned to Minneapolis. In 1942 Frank moved his family to Louisville, Kentucky, and began work at the National Synthetic Rubber Corp., where he helped to develop a formula for synthetic rubber, an important contribution to the war effort. In 1945 the Boveys moved to St. Paul, Minnesota, where Frank entered graduate school at the University of Minnesota. In 1948 he received his Ph.D. in physical chemistry with Professor Izaak M. Kolthoff. Upon graduation Frank joined the research group at the 3M company. During his years at 3M and later at Bell Laboratories, he enjoyed phenomenal success in his research endeavors and was always a leader in the field of polymer science.

In the early 1950s Frank moved his family to White Bear Lake, a suburb of St. Paul, where their 20-acre homesite included an apple orchard, not unlike the summer home on Lake Minnetonka where Frank enjoyed his childhood summers. Whenever possible, Frank and Shirley kept with the family tradition of including European travel as part of a proper education. For example, they took their teenage children to England where Frank was presenting a series of lectures. For six weeks the children studied their lessons and sent their assignments back to the United States, meanwhile absorbing the culture of the Bovey ancestral homeland.

The homes of Frank Bovey always included a book-lined den for his vast book collection, as well as magazines and newspapers from subscriptions to Eastern publications like the *New Yorker* and the *New York Times.* This collection and frequent visits to the Minneapolis Symphony helped his children grow up with a strong sense of history and the arts. As was the Bovey custom, Frank and Shirley sent their children (Margaret, Peter, and Victoria) to college in the East.

Frank had many interests beyond the polymer chemistry that filled his professional life. He had a passion for photography, astronomy, music, art, and architecture. His camera recorded events throughout his life, and his collection of slides provided him much joy in his later years. Frank's interest in astronomy provided an opportunity to introduce his children to the world of science, whether it was a lesson in using the telescope or pouring over prints from the Palomar 200-inch telescope. He also obtained his pilot's license and loved the study of navigation (precomputer age). In music his taste was classical, particularly Mozart, Bach, Beethoven, and Brahms. Frank traveled extensively, principally in Europe.

In 1962 Frank moved his family to New Jersey, where he lived and worked at Bell Laboratories until his retirement. Even at age 75 he could be found in his office nearly every day, reading and studying what the younger generations of scientists were discovering. In his final years Frank moved to Amherst, Massachusetts, where he lived with his daughter Margaret. There he continued to spend his time enjoying literature, music, and watching his favorite movies. He passed away on January 19, 2003.

For more than 20 years Frank Bovey was my mentor, teacher, colleague, and friend. He taught me a great deal not only about polymer chemistry and NMR spectroscopy but also, by example, about the patience and determination required of a successful scientist. As a prolific and accomplished author Frank demonstrated that accuracy, brevity, and clarity are the hallmarks of successful publications.

Frank willingly shared his interests in areas outside of science. He loved to discuss architecture, particularly, of early American homes like Mount Vernon, Monticello, and the Biltmore Estate. Travel was something Frank enjoyed heartily, both in the doing and in the telling. From his descriptions, I think he enjoyed visiting Prague more than any other city. He always amazed me with his depth of knowledge in so many fields, from art and history to automobiles and astronomy. The one exception was sports, where he clearly felt little need to venture. Frank appreciated good humor especially if witty or wry in nature. I was pleasantly surprised when I discovered his love of Monty Python's antics.

Frank loved to sample the food at fine restaurants, be it in Summit, New Jersey, or Rome, Italy. He had no fear of red meat, and we shared a fondness for dessert. His ability to discuss almost any issue or subject with ease made him an excellent dinner host for groups large or small.

I would like to thank the children of Frank Bovey for their contributions and assistance in the preparation of his memoir.

SCIENTIFIC CAREER
(BY ALAN E. TONELLI)

The professional career of Frank Bovey began in 1943 when he joined the National Synthetic Rubber Corp. This was a U.S. government-sponsored consortium of companies charged by the U.S. Synthetic Rubber Program (USSRP) with developing a general purpose synthetic rubber to replace natural rubber from Southeast Asia, which came under Japanese control at the beginning of Word War II. In collaboration with a network of government and academic researchers, the USSRP successfully developed and manufactured in record time enough synthetic rubber to meet the needs of the United States and its allies during World War II.

Early in the USSRP it was decided to produce GR-S rubber, a styrene(25%)-butadiene(75%) copolymer that could be vulcanized, as the most likely synthetic replacement for natural rubber. Professor Izaak Maurits ("I. M.") Kolthoff of the University of Minnesota, often called the father of modern analytical chemistry, was an academic collaborator in the USSRP. Frank Bovey, while at the National Synthetic Rubber Corp., joined the Kolthoff research group to study various aspects of the emulsion polymerization and copolymerization of styrene,[3] which later resulted in his coeditorship of a book on emulsion polymerization.[4] This collaboration culminated in 1948, when Frank received his Ph.D. in physical chemistry for his work with Prof. Kolthoff.

After receiving his Ph.D., Frank joined the 3M company, where he remained for 14 very productive years and became head of the 3M Polymer Research Department. Here Frank studied the synthesis and characterization of fluoropolymers; the electron irradiation of polymers, about which he wrote a book[5]; biopolymers, including the synthesis of the polysaccharide dextran and the denaturation of the protein enzyme pepsin; and the ^{1}H-NMR spectroscopy of polymers. The NMR work was conducted with his able 3M collaborator George Tiers, who also collaborated with future Nobelist Paul C. Lauterbur (NMR imaging/MRI). The seminal studies of polymers by ^{1}H-NMR spectroscopy begun by Frank Bovey at 3M resulted in his becoming one of the earliest pioneers and leading researchers in this field, a role he continued during the remainder of his career of nearly 40 years. At 3M Frank Bovey successfully employed ^{1}H-NMR techniques to characterize the motions, configurations, and polymerizations of synthetic polymers. Most notable were his studies of the stereosequences of vinyl polymers and introduction of the m (meso) and r (racemic) repeat unit diad and i = mm (isotactic), s = rr (syndiotactic), and h = mr or mr (heterotactic) repeat unit triad terminology to describe the relative attachments of side-chains to polymer backbones,[6] and which is still used to describe their stereochemistry. Also in this paper ^{1}H-NMR was used for the first time to unambiguously determine the absolute stereochemistry of stereoregular vinyl polymers. This was particularly noteworthy, because X-ray diffraction often cannot distinguish the stereochemical nature of crystalline polymers.

Of course, the stereochemistry of vinyl polymers is critically important to their physical properties, and very early on Frank recognized this important structure-property relationship. This was later evidenced in a humorous incident on the occasion of his receipt of the High-Polymer Physics Prize of the American Physical Society in Philadelphia at the March 1974 meeting. In his award address Frank was making the point that the stereochemistry of vinyl polymers is key to understanding their properties. He did this in a manner that was very unusual and certainly uncharacteristic of his somewhat formal, intensely private, and shy nature. Frank noted that the regular and irregular attachment of vinyl polymer side-chains in isotactic and atactic vinyl polymers leads to strong crystalline and weak amorphous materials, respectively. He then displayed color slides of two well-known women: Twiggy, the very slender, almost emaciated, boyish British model and pop-culture icon of that period, and Raquel Welch, the voluptuous American model and movie actress. This was followed by “need I say more,” delivered in a manner that let the audience know Frank realized this humorous, slightly irreverent, but very effective analogy was very “out of character” for him.

On a personal note, I was a graduate student at Stanford working in Paul Flory’s research group. It was Paul’s custom to have members of his research group present informal lunchtime seminars. Sometime in late 1966, when my turn to present such a seminar arrived, I decided to describe the efforts of Bovey and Tiers to use ^{1}H-NMR to elucidate the microstructures of vinyl polymers. This choice was a personal one, because my Ph.D. thesis research dealt with the conformational properties (sizes and shapes) of poly(lactic acids) and vinyl polymers and establishing the sensitivities of their conformations to their stereochemistries. Flory had a general interest in this area, and in fact a few years later published two papers dealing with the configurational sensitivities of the conformations and ^{1}H-NMR spectra of vinyl polymers. Upon completion of my Ph.D. in 1968 I eagerly accepted a position in the Polymer Chemistry Research Department at Bell Laboratories, which was headed by Frank Bovey. Thus began my fortunate 23-year association with Frank Bovey.

Also while at 3M, Frank and Charles Johnson[7] and simultaneously Waugh and Fessenden[8] developed quantitative estimates of the effects produced by magnetically anisotropic groups, like phenyl rings, on ^{1}H-NMR spectra. The Johnson-Bovey Tables of ring-current NMR shieldings have been widely used to determine the conformations of polymers, both synthetic and biological. For example, the Johnson-Bovey ring-currents, as modified by Giessner-Prettre and Pullman[9] for the bases in DNA, were utilized to determine the detailed double-helical conformations of several DNA fragments in aqueous solution before and after binding antibiotics like Actinomycin D, containing aromatic rings. For example, the location and orientation of Actinomycin D bound to these deoxyribonuleotides were derived from a comparison of the ^{1}H-NMR spectra observed before and after binding with those calculated using the Johnson-Bovey ring-current effects. This information is critically important in the design of new and more effective drugs.

In 1962 Frank Bovey moved to AT&T-Bell Laboratories, where he remained for more than 30 years. During much of this time, he was head of the Polymer Chemistry Research Department, and more importantly he wrote nine books covering various aspects of NMR spectroscopy and its application to polymers. Not only did Frank Bovey pioneer this important field in his research but with his books he also introduced and educated generations of polymer scientists to the great utility of elucidating the microstructures, conformations, organization, and mobilities of polymers, through their observation with NMR, in order to gain fundamental understanding of their physical properties. Although he never held a permanent academic position, Frank Bovey successfully taught countless students, professors, and industrial scientists, principally through his books. The underlying reason for his success lay in Frank's twofold expertise in NMR spectroscopy and in polymer science, which is apparent when reading his books and which makes them both very relevant and readily understandable to researchers studying synthetic and biological macromolecules.

During his tenure of more than 30 years at Bell Laboratories, Frank Bovey conducted significant research into synthetic and biological polymers employing NMR spectroscopy, but he also conducted important studies using ORD, CD, fluorescence, and electronic spectroscopies. Frank used solution NMR to determine the microstructures of a wealth of different polymers and solid-state NMR to learn about the organization and mobilities of solid polymers. As new NMR techniques were developed Frank Bovey was among the first to adapt and apply them successfully to polymers. In the early 1970s high-magnetic-field superconducting NMR spectrometers were introduced and Frank Bovey made sure that Bell Laboratories obtained one of the first of these instruments. As a consequence of his foresight, many biologically relevant polymer samples were made available by a host of investigators from around the world for study at high resolution in Frank's laboratory. Frank amassed hundreds of such samples, and in fact he and his colleagues studied many of them. The late Murray Goodman, then at Brooklyn Poly (now New York Polytechnic), became a significant collaborator in the NMR study of biopolymers. I can remember many Saturday mornings driving from New Jersey to Brooklyn to discuss our NMR observations and their interpretations with Prof. Goodman and his students. Each of these trips was eventful, if not always for the science then surely for the excitement of driving with Frank, an automotive aficionado without the motoring skills that matched his enthusiasm for driving.

A good example of the extensive NMR studies of biopolymers conducted by Frank Bovey can be found in a paper[10] dealing with the solution conformation of the 9-amino acid polypeptide hormone lysine-vasopressin. The constraints provided by ^{1}H-NMR data were used to limit possible conformations/shapes for lysine-vasopressin. Subsequent conformational searches were conducted to determine those conformations that were consistent with the constraints provided by the ^{1}H-NMR observations. These NMR studies were clearly the precursors of the currently used approach for determining the solution conformations of native proteins by searching their conformational spaces under the constraints provided by multifaceted NMR observations.

Frank Bovey also pioneered the use of multidimensional NMR observations to determine the conformations of flexible polymers in solution.[11] These provide information concerning the dihedral angles of rotation about the bonds in the polymer backbones (polymer conformations, sizes, and shapes) and permit determination of the average local bond conformations for randomly coiling, disordered polymers in solution. The direct measurement of the local conformational preferences in polymers is vital to understanding their overall global sizes and shapes, which are so intimately tied to their physical behaviors.

At Bell Laboratories Frank also continued the NMR study of vinyl polymer stereochemistry, which he initiated while at 3M. In 1966 he and two colleagues[12] presented the mathematical relations describing the probabilities for observing all possible stereosequences of various lengths (2-repeat unit diads, 3-repeat unit triads, 4-repeat unit tetrads, 5-repeat unit pentads, etc.) in vinyl polymers formed in a random manner during polymerization to yield P_m and $P_r = 1 - P_m$ fractional m and r diad populations. This was a major breakthrough, because now NMR could provide a quantitative measure of stereosequence populations in vinyl polymers and could distinguish between polymers produced by polymerization mechanisms that were random (Bernoullian) or not (possibly Markovian).

Beginning in the early 1970s with the advent of commercial NMR spectrometers that employed pulsed field detection and fast Fourier transformation of time-domain resonance signals, high-resolution ^{13}C-NMR observation of polymers became possible, and Frank Bovey was among the first to take advantage of this development. ^{13}C resonance frequencies (δ^{13}C) are much more sensitive to chemical microstructure than δ^{1}Hs, by ~ an order of magnitude. As a consequence, polymer microstructures are generally more easily and sensitively determined by observation of their solutions with ^{13}C-NMR. Frank and his Bell Laboratories colleagues took full advantage of this development to determine in great detail the microstructures and mobilities of many homo- and copolymers.[13]

An illuminating and important example was the determination of the contents of short- and long-chain branching produced by intrachain and interchain transfer of radicals during the high-pressure, free-radical synthesis of low-density polyethylene (PE).[14] Using the Grant-Paul ^{13}C chemical shift ($\delta^{13}C$) rules,[15] which describe the additive effects on $\delta^{13}Cs$ (resonance frequencies) of carbon substituents located α, β, and γ to the observed carbon, and model copolymers, Frank Bovey and his colleagues were able to determine that low-density PE typically contained 10-12 four-carbon, 4 five-carbon, and 2 two-carbon short branches per 1,000 repeat units and ~ 1-2 long branches (6 carbons or more), resulting from intrachain and interchain radical transfer during free-radical polymerization, respectively. These results were very significant, because aside from molecular weight and its distribution and stereoregularity the types and amounts of branching in vinyl polymers are the most important structural variables influencing their physical properties.

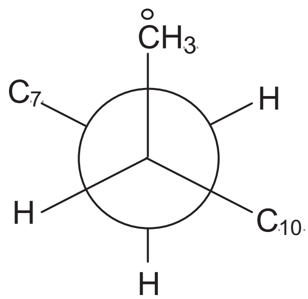
Very early on,[16] Frank Bovey realized that the conformational sensitivity of the γ-substituent effect on $\delta^{13}Cs$, first suggested by Grant and Cheney,[17] could have important consequences in the determination of the configurations and conformations of polymers by ^{13}C-NMR. This is most clearly illustrated by the ^{13}C-NMR spectra of polypropylenes (PPs), where, for example, nearly all 36 possible heptad stereosequences (seven consecutive repeat units) are observed[18] in the methyl carbon region of the spectrum for atactic-PP (a-PP). To assign the methyl resonances observed in the ^{13}C-NMR spectrum of a-PP to its individual constituent stereosequences, model compounds (3,5,7,9,11,13,15-heptamethylheptadecanes) –

$$C_1\text{-}C_2\text{-}C_3\text{-}C_4\text{-}C_5\text{-}C_6\text{-}C_7\text{-}C_8\text{-}C_9\text{-}C_{10}\text{-}C_{11}\text{-}C_{12}\text{-}C_{13}\text{-}C_{14}\text{-}C_{15}\text{-}C_{16}\text{-}C_{17}$$

| m,r | m,r | m,r | m,r | m,r | m,r |

C C C C° C C C

mimicking the heptad stereosequences in a-PP were synthesized, each with ^{13}C enrichment of the 9-methyl carbon, and their ^{13}C-NMR spectra were observed.[19] Alternatively, the relative amounts of proximal gauche arrangements between the 9-CH_3 and its γ-substituents, the 7- and 11-methine carbons, were evaluated.[20] When multiplied by –5 ppm (the



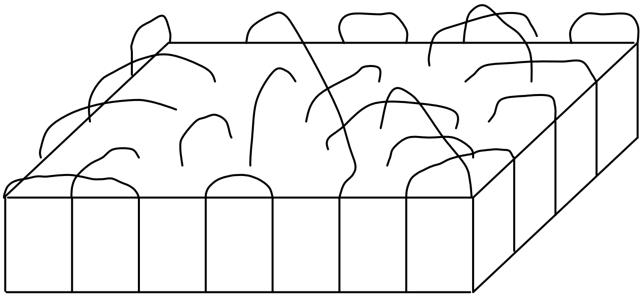
View along the C_8–C_9 backbone bond when it is trans and C^oH_3 and C_7 are close in space and shielded from the magnetic field.

γ-gauche shielding effect), the calculated ^{13}C chemical shifts faithfully reproduced the methyl carbon region of the ^{13}C-NMR spectra of a-PP and its heptad stereosequence model compounds. This was made possible only after the development of an appropriate description of the conformational characteristics of PPs.[21]

Though Frank Bovey had previously suspected[16] that the conformationally sensitive γ-gauche shielding effect was the source of the stereosequence dependent δ^{13}Cs observed in PPs and other vinyl polymers, it was not until an appropriate description of the conformational characteristics of PPs was developed that this important relationship was convincingly demonstrated. From that point in time onward the ^{13}C-NMR spectra of vinyl polymers could be effectively assigned to their stereosequences by evaluating their γ-gauche shieldings from knowledge of their conformational characteristics.

In 1976 Schaefer and Stejskal[22] first proposed and obtained high-resolution ^{13}C-NMR spectra for polymer solids by combination of several techniques. This made possible the study of the conformations, organizations, and mobility of polymer chains in solid samples, which is their most practical use. Within a few years of this development Frank Bovey and his colleagues at Bell Laboratories were applying the solid-state ^{13}C-NMR technique to a wide variety of solid homopolymers, copolymers, polymer blends, and guest polymers included in noncovalent clathrate compounds formed with small-molecule hosts.[23] Frank continued these studies until his retirement in 1993. His high-resolution solid-state NMR studies of polymers were not limited to ^{13}C-NMR but also included observation of the ^{19}F, ^{29}Si, and ^{31}P nuclei, as well.

1,4-trans-Polybutadiene (TPBD) was known to exist in two crystalline structures, Forms I and II, stable below and above ~65°C, respectively. Observations of variable-temperature, high- and low-resolution solid-state ^{13}C-NMR spectra of TPBD and surface-epoxidized TPBD crystals, which immobilizes their fold surface, permitted Bovey and colleagues[24] to conclude that TPBD chains are conformationally ordered and rigid in Form I crystals but disordered and mobile in Form II crystals. Epoxidation of the fold surfaces



Folded chains on polymer crystal surfaces.

of TPBD crystals did not affect the Form I → Form II transition temperature nor the mobility of TPBD chains in the interior of Form II crystals. This demonstrated that the fold surfaces of TPBD crystals were not involved in the Form I → Form II crystal → crystal transition. Rather it is the TPBD chains in the crystalline interiors alone, which undergo transition from a rigid single conformation to a collection of mobile conformations at ~65°C.

Somewhat related NMR studies of TPBD chains that are extended and isolated in the narrow (~5Å) channels of the crystalline inclusion compound (IC) formed with perhydrotriphenylene (PHTP) host revealed a close correspondence between the conformations and mobility of the included guest TPBD chains and those in bulk Form II TPBD crystals. Even more remarkable were the observations by ^{2}H (deuterium) NMR of deuterated TPBD and polyethylene (PE) when included as guests in their PHTP-ICs. This was surprising, because, while the included TPBD chains were thought to undergo conformational transitions, the motions of included PE chains were believed to be limited to librations (small-amplitude vibrations) around the extended planar, zigzag conformation. As a consequence, this comparative study of polymer mobility provided the first documented example that the motion of C-^{2}H bonds as revealed by ^{2}H-NMR may not always be unambiguously interpreted in terms of the underlying segmental motions of polymers.

Frank Bovey and collaborators employed both high-resolution ^{13}C and wide-line ^{13}C and ^{1}H-NMR techniques to study the organization and mobilities of the phase-segregated blocks in thermoplastic elastomeric copolymers made with poly(butylene terephthalate) hard blocks (semicrystalline) and poly(tetramethylene oxide) soft blocks (amorphous). They found the soft blocks to be mobile but with an angular range of motion that decreased with increasing content of hard blocks. In addition, the motions of CH_2 carbons were observed to be more facile than the OCH_2 carbons in the butylene diol portions of the hard blocks. ^{2}H-NMR observation of copolymers made with deuterated hard blocks (-O-CD_2-CD_2-CD_2-CD_2-O-) revealed the ability of the butylene diol segments to undergo trans-gauche conformational transitions, while in the amorphous regions of samples with perdeuterated phenyl rings, ^{2}H-MR observations revealed phenyl ring flipping. Solid-state NMR observations of the motions occurring in the phase-segregated regions of these copolymers permitted a much improved, detailed understanding of their mechanical properties.

Finally, Frank Bovey also used NMR to study compatible polymer blends. Mirau and Bovey[25] employed concentrated solutions of polymer mixtures and/or bulk, molten polymer mixtures to obtain resolution sufficient to perform ^{1}H-NMR studies. In this manner they were able to determine which protons belonging to poly(vinyl methyl ether) (PVME) chains interact with (are within ~4Å of) the protons in polystyrene (PS) and poly(ε-caprolactone) (PCL) chains in the

PVME = -(CH_2–CH-)- with side group *OCH_3 (methoxyl)* on CH

PS = -(-CH_2–CH-)- with side group *C_6H_6* (phenyl ring) on CH

compatible PVME/PS and PVME/PCL blends. They found the phenyl protons of PS to give cross-peaks with the methoxy protons (strong or closer) and methine protons (weak or farther) of PVME. For the PVME/PCL blend the interactions, distances between the methoxy protons of PVME and the C1,C3 protons of PCL were observed to be weaker, farther than those of the C2/C4 and C5 protons of PCL.

$$\text{PCL}\ (\text{-}\overset{1}{CH_2}\text{-}\overset{2}{CH_2}\text{-}\overset{3}{CH_2}\text{-}\overset{4}{CH_2}\text{-}\overset{5}{CH_2}\text{-O-}\overset{\overset{\Large O}{\|}}{C}\text{-})$$

Frank Bovey's scientific legacy was, and remains, the influence he exerted, and still commands, on generations of polymer scientists through illustration of the methods and utilities of NMR spectroscopy as applied to polymers to understand in detail their microstructures, conformations, mobilities, and organization. He accomplished this by example during his long career in research, and more formally by the many books he authored. For all of these we, and I in particular, remain grateful.

NOTES

1. Many details of the Bovey family history are from the unpublished memoirs of Ruth Alden Bovey, first cousin to Frank Alden Bovey.

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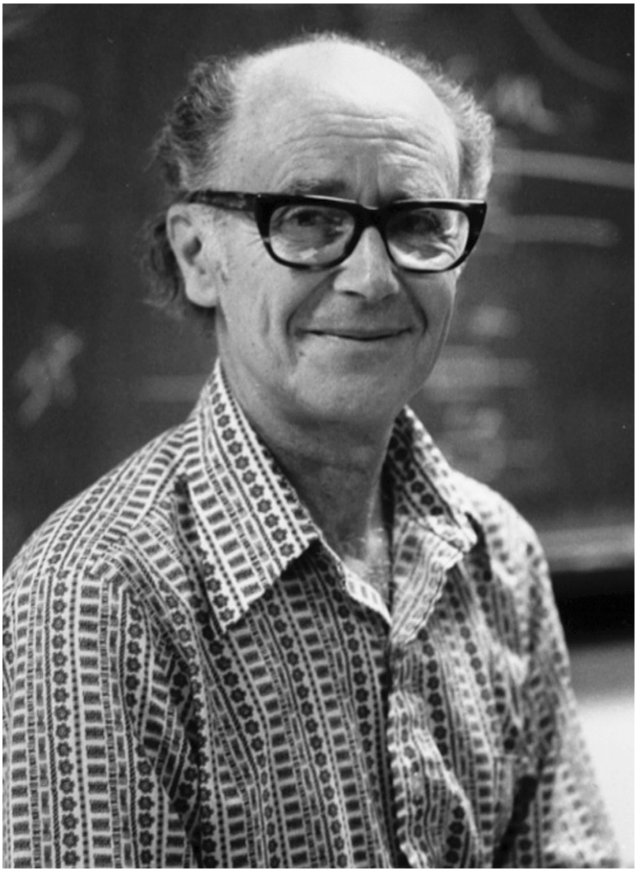

Norman Davidson

NORMAN DAVIDSON

April 5, 1916–February 14, 2002

BY HENRY A. LESTER AND AHMED ZEWAIL

NORMAN DAVIDSON WAS BORN in Chicago. He earned a bachelor's degree in chemistry at the University of Chicago in 1937 and completed another bachelor of science degree at the University of Oxford in 1939 as a Rhodes scholar. In 1941 he completed his Ph.D. in chemistry at the University of Chicago.

Before and during World War II, he worked on the problem of purifying plutonium for the Manhattan Project at the University of Southern California, at Columbia University, and finally at the University of Chicago. He also had a brief stint as a researcher at the Radio Corporation of America.

Norman Davidson's career as an independent scientist was entirely at the California Institute of Technology (Caltech) and covered the period from 1946, when he was appointed instructor in chemistry, to his death in 2002. Norman made important contributions sequentially in three quite different fields. From 1946 until about 1960 he worked in physical and inorganic chemistry. From about 1960 till about 1980 he was a founder of nucleic acid molecular biology; and from then until 2002 he made numerous contributions to molecular aspects of neuroscience.

Norman was admired by his many students and colleagues. His students organized symposia for his sixtieth, seventieth, seventy-fifth, and eightieth birthdays. The format was simple: present one's own science. Norman sat in front, with his yellow pad, and took in every word.

He is survived by his wife, Annemarie Davidson, of Sierra Madre, California; by four children, Terry Davidson of Poway, California; Laureen Agee of Mammoth Lakes, California; Jeff Davidson of Cayucos, California; and Brian Davidson of Walnut Creek, California; and by eight grandchildren. Norman rarely used his middle name, Ralph.

CONTRIBUTION BY A. ZEWAIL

1946-1960: PHYSICAL CHEMISTRY

Norman Davidson made significant contributions to physical chemistry before he shifted his efforts to biophysical chemistry and to biology. Perhaps these contributions can be classified into two major areas. One is theoretical and involves the work on thermodynamics and statistical mechanics that culminated in his classic textbook (1962), which was based on his course for first-year graduate students. The preface states, "The statistical mechanics of dilute systems of independent particles at equilibrium is a subject which is essentially fully developed. The practicing chemist should be able to apply this theory with assurance and accuracy to calculate the thermodynamic properties of substances in the ideal-gas state from molecular structure data." In 2003 this book received the accolade of republication as a Dover paperback edition.

The other area is experimental. Norman and his group were among the leaders in developing the shock-tube method for kinetics of reactions. Stimulated by the work in 1920 of

Einstein on the dispersion of sound velocity, Norman studied the rate for the reaction $N_2O_4 \leftrightarrows 2NO_2$. The work was scholarly and of highest quality, but Norman himself realized that the methodology needed to be advanced to one that is capable of better time resolution and cleaner chemical products.

It is not widely known that Norman was early in the development of flash photolysis and its applications. Flash photolysis began after World War II in 1949 at Cambridge University with the work of R. G. W. Norrish and G. Porter, who used intense flashes of light to create free radicals that could be studied spectroscopically. Together with G. Herzberg and D. A. Ramsay at the National Research Council of Canada, they used the method to study ClO, SO, CH_3, and others. In the process of developing flash photolysis around 1950, Norman tackled one of the most elementary yet complex reactions—the dissociation and recombination of iodine. This is fundamental to chemical change—how is a bond broken and reformed? Although the dissociation reaction involves only two atoms, the recombination was thought to involve "three body collisions." Norman found, from the kinetics, that the reaction occurs as a succession of two-body collisions, one between iodine atom (I) and iodine molecule (I_2) to form a relatively stable complex (I_3), and a second between I_3 and I to form a pair of I_2 molecules. The kinetics in those days was of microsecond to millisecond resolution, and these elementary steps could not be resolved directly in real time. Forty years later Norman was pleased and thrilled that we were able to freeze in time I_3 complexes with femtosecond time resolution in femtochemistry experiments involving the collision of halogen atom and diatomic molecules. The lifetime of such complexes was short, tens of picoseconds, but Norman's earlier inference was, as usual, insightful and correct. Norman remarked to

H.A L., "If I knew such experiments were on the horizon at Caltech, I would have stayed in chemistry."

TRANSITION TO MOLECULAR BIOLOGY

Norman's work from 1946 to 1960, he wrote, "is completely unrelated to molecular biology, but it resulted in my being elected to membership in the National Academy of Sciences in 1960. This kudo was very useful in my promotion at Caltech and my independence to shift fields from time to time" (2002,2). Indeed, Norman's audacity in switching to new fields played a large role in his ability to influence science so broadly and through so many young colleagues.

Norman was influenced by Linus Pauling, who directed Caltech's Chemistry Division during Norman's early years on the faculty. Pauling believed that chemists could make fundamental contributions to biology, and in 1951 he defined the α-helix and the β-sheet. Pauling attempted to solve the structure of DNA; and soon after the 1952 publication of Watson and Crick's model, J. D. Watson spent a year at Caltech. During the 1950s, other founders of molecular genetic biology who worked at Caltech included Howard Temin, Renato Dulbecco, John Cairns, Alex Rich, Jerome Vinograd, Robert Sinsheimer, and Max Delbruck. The Meselson-Stahl experiment, published in 1958, was conducted just down the hall from Norman's office.

Davidson believed that one contributed to molecular biology primarily through published papers and through excellent students. He wrote very few review papers and no textbooks in this field, in contrast to physical chemistry, where a textbook can be timely even after 40 years! However, in late 2001 he felt it appropriate to sum up his work since about 1960 with a prefatory chapter in the *Annual Review of Biochemistry* (2002,2). Published after his death, the chapter provides Norman's own clear views about the work that he considered most noteworthy.

Norman wrote of his transition to molecular biology as follows:

> Some time around 1958 or 1959, I was thinking about switching to biology-related research. . . . I learned that ion channels were selective for either sodium ions or for potassium ions. This fascinated me because I knew from my undergraduate analytical chemistry course how difficult this separation was. . . . [I told] Bernard Katz about my interest in doing something chemical about ion channels. He advised me to forget about it because the density of ion channels in the squid axon was only about 1 per μm^2, and it would be impossible to isolate a sufficient quantity to do anything chemical. He was of course right because before recombinant DNA and cloning came along it was not possible to do anything other than electrophysiological studies. . . . I decided that the field most suitable for biochemical studies was DNA. (2002,2)

NORMAN'S STUDIES ON DNA

With his students William Dove and James Wetmur, Norman developed fundamental facts about the effects of ionic strength and divalent cations on DNA hybridization and denaturation (1962, 1968). These ideas are still in use today, providing the foundation for hybridization-based phenomena such as Northern and Southern blots. With Tetsuo Yamane, Norman used his chemistry background to exploit the Hg^{+} ion as a probe for isolating characterizing DNA.

James Wang and Norman developed the chemistry and biology of closed circular DNA (1966). With Ronald W. Davis, Norman helped to develop electron microscopy as a technique for visualizing regions of single- and double-stranded DNA (1968). This team was the first to physically map a mutant genome (i.e., a deletion of the phage lambda). There followed a decade when electron microscopy was the dominant technique for high-resolution studies of nucleic acid interactions. Philip Sharp, who joined Norman's lab in 1971, studied details of the antibiotic resistance factors and their interactions with host chromosomes. They discovered that insertion sequences, which were usually of length 1–4.5 kb (and this abbreviation was introduced by Norman Davidson), contained a palindromic sequence at each end, flanking the genes for transposition (1972). Madeline Wu and Norman developed a way to employ antibodies to localize protein-DNA binding sites in the electron microscope. "We used a chemical method to attach the hapten dinitrophenyl to the protein that was attached to the DNA (for example, the protein that was bound to the two ends of adenovirus-2 DNA). By then adding an antibody to dinitrophenyl and, if necessary, a second antibody, we could observe the protein at each end" (2002,2).

STUDIES ON RNA

Madeline Wu and Norman also developed ferritin labeling to visualize tRNA molecules. Beginning in 1972 he concentrated increasingly on RNA, especially the retroviruses. He and Welcome Bender, working with SV40, used the poly(A) tail to map the 3′ end of the mRNA molecule.

Norman understood that studies of cDNA derived from mRNA were an appropriate way to assess complexity in a genome. He settled on *Drosophila* as a model system, and he showed his usual excellent scientific taste in devoting his efforts to molecules and topics that remain important to this day. With Eric Fyrberg, Norman cloned all six *Drosophila* actin genes and found homologies to both *Dictyostelium* (which served as the original probe) and vertebrate cytoskeletal actins (1976). Ronald L. Davis came to Norman's lab to study the *Drosophila dunce* gene, which encodes a cAMP phosphodiesterase. Norman and Davis cloned and sequenced this gene and discovered some aspects of its alternative splicing.

Norman played a role in recruiting Eric Davidson (no relation) to Caltech in 1971. Eric Davidson continued to study the arrangement of DNA sequences, sequence complexity, and selective transcription throughout the 1970s. These experiments interacted with Norman's developments in the fields of nucleic acid hybridization kinetics and electron microscopy. Norman and Eric had a close intellectual relationship.

CONTRIBUTION BY H. A. LESTER

NEUROSCIENCE

Norman wrote, "[About 1980] I felt that it was a good time to go back to my earlier interest in neurobiology. *Drosophila* was not a good organism for this work because its neurons are too small for patch clamping, which was a highly developed skill for vertebrate cells. My colleague in the Biology Division at Caltech, Dr. Henry Lester, was interested in learning molecular biology, so we teamed up and collaborated up to the present" (2002,2). Norman and I published our first joint paper in 1985, when Norman was 69 years old; it was Norman's 291st paper. The group eventually published a total of 93 papers together. Cesar Labarca joined the Caltech group in 1986 and played a key role in the DNA manipulations of many of these studies.

ION CHANNELS AND RECEPTORS

Davidson believed that cloning genes for ion channels would open up new vistas for neuroscience; so the Caltech group began studying cDNA clones for the classically defined ion channels, the nicotinic acetylcholine receptors, and sodium channels, the latter in collaboration with William Catterall at Seattle and Robert Dunn in Toronto. In the 1980s the group conducted several studies that first isolated these clones, then used *Xenopus* oocytes (after the key publication by Eric Barnard and Ricardo Miledi) and mammalian cells to express the functional channels. The expression systems, which are still used in many labs today, had several roles. First, one wished to prove function. Second, one wished to verify that one had all the clones for certain multi-subunit proteins. And one also conducted many mutagenesis studies to define important functional roles for individual amino acids.

Early colleagues on those studies included Mike White, Alan Goldin, Reid Leonard, Lei Yu, Pierre Charnet, and Doug Krafte. Norman's skills at DNA manipulations and his delicacy with RNA were vital to the experiments, which occurred before the days of molecular kits and PCR. There were spirited competitions with the lab of Shosaku Numa in Kyoto, especially as Numa teamed up with Bert Sakmann in Göttingen to perform physiological studies on the site-directed mutants. The Caltech group was usually the runner-up in those races. Norman was particularly pleased with the physical chemical elegance of a study that defined the permeation pathway of the nicotinic receptor by manipulating the millisecond interruptions that occurred when the open-channel blocker, QX-222, bound (1988). In Na channels Norman played a key role in the discovery that single amino acid changes could dramatically affect such functional properties as voltage dependence and inactivation (1990,1). Terry Snutch joined the group to contribute particularly on the diversity of calcium channel genes, as did Nathan Dascal, Joel Nargeot, and John Leonard (1990,3).

Ion channels were in the air elsewhere at Caltech. Seymour Benzer's *Drosophila* lab had previously worked on the *shaker* mutant and generated evidence that it was a K^+ channel. After Benzer's former students Mark Tanouye (then on the Caltech faculty) and especially Lily and Yuh Nung Jan (at the University of California, San Francisco) had separately cloned this K^+ channel, Norman's group conducted several structure-function studies, in collaboration with Tanouye and separately. Later, in the 1990s, Norman's group worked with Kai Zinn and his student John Bradley to clone, express, and study cyclic nucleotide-gated channels.

Norman was particularly taken with the idea of cloning by functional expression, which used his skills at nucleic acids to the fullest. In 1987 Norman's postdoctoral fellow Hermann Lübbert used antisense suppression to find a partial cDNA clone for the receptor now termed serotonin 5-HT2C, in collaboration with Paul Hartig and Beth Hoffman. Upon reading of the Caltech partial clone (1987), Richard Axel at Columbia promptly sent Norman a bottle of champagne. Then David Julius and Axel went on, using an even better expression cloning technique, to find the entire functional cDNA. This was probably the second G protein-coupled receptor cloned (the first having been found by Rich Dixon, Brian Kobilka, Marc Caron, Bob Lefkowitz, Cathy Strader, and their colleagues at Merck and Duke). Norman's appetite for the G protein pathway was thus whetted, and Mel Simon's lab at Caltech, which cloned many of the G protein subunits, became active collaborators.

In 1992 Nathan Dascal from Tel Aviv University and Wolfgang Schreibmayer from the University of Graz arrived as sabbatical visitors and set about expression cloning the gene for the cardiac G protein-activated inwardly rectifying K^+ channel. Roughly at the same time, the Jans at the University of California, San Francisco, and the Caltech group found the gene, now termed GIRK1 or Kir3.1 (1993). For several years after that the Caltech group studied the functional activation of this channel. The questions revolved around activation by the Gα subunits vs. the Gβγ subunits. The availability of expression systems enabled several labs to determine that the major activation occurred via the Gβγ subunits. However, the α subunits also clearly played a role, and these interactions are not yet settled. Paulo Kofuji and Craig Doupnik joined these studies and performed elegant experiments on the role of the regulators of G protein signaling (RGS) proteins, which nicely tuned up the kinetics of the Gi-coupled pathway (1997).

NEUROTRANSMITTER TRANSPORTERS

John Guastella joined the lab in 1988 to take a rather new direction: neurotransmitter transporters. The collaboration included Baruch Kanner of the Hebrew University, who had obtained a partial sequence for the GABA transporter. Norman designed degenerate oligonucleotide probes, and John isolated a cDNA clone, designated GAT-1, which in Xenopus oocytes caused the uptake of [^{3}H]GABA (1990,2). Shortly afterward Susan Amara and coworkers used an expression strategy to clone a noradrenaline transporter. The substantial regions of sequence homology between these two transporters then allowed many investigators to clone additional transporters.

As usual, we were fascinated by the opportunity that an isolated expressible clone provided for functional studies, so between 1992 and 1998 the Caltech group adapted voltage-clamp techniques to dissect mechanistic details of the GABA and serotonin transporters. Excellent postdocs, including Sela Mager and Michael Quick, deduced turnover rates and substrate binding orders. They also made pioneering observations that GAT1 could be modulated via membrane trafficking. Eventually Chi-Sung Chiu made knock-in mice with GFP fusions to GAT1, and he counted GAT1 molecules using quantitative microscopy: There are about 1,000 GAT1 molecules per μm^2 in a presynaptic nerve terminal (2002,1).

Beginning in the mid-1990s Norman saw clearly that X-ray crystallography would furnish the key answers to outstanding questions in ion channel and transporter function. He began to study overexpression for this purpose, while also engaging in the next phases of his career.

SYNAPTIC PLASTICITY

Erin Schuman came to Caltech in 1993. She had helped to show that nitric oxide as a second messenger could cause long-term potentiation (LTP). "I was fascinated by this. Neuronal nitric oxide synthase (nNOS) knock-out mice still expressed LTP. This suggested that endothelial NOS (eNOS), which despite its name was known to occur in the dendrites of hippocampal neurons, was the contributing enzyme for LTP. Furthermore, it had been shown that an inhibitor of myristoylation would inhibit LTP" (2002,2). Norman therefore engineered an adenovirus containing the signal sequence of CD8 (which occurs on the cell membrane of T cells) fused to the eNOS gene. With this gene, eNOS expression on the cell surface of neurons in rat hippocampal slices was not blocked by a myristoylation inhibitor, and tetanically induced LTP was expressed (1996).

Dr. Schuman's lab then helped to show that brain-derived neurotrophic factor (BDNF) could induce long-lasting enhancement of synaptic transmission. Norman, Erin, and I, with postdoctoral fellow Yong-Xin Li, followed up by studying the effects of BDNF on E18 neurons in culture. We showed that BDNF could enhance synaptic transmission between a synaptically connected cell pair. We made a dominant negative TrkB by deleting the intracellular portion of the gene and fusing GFP to the C terminus as a marker for expression. We then observed that only when the presynaptic cell was infected could we observe an inhibition of the BDNF enhancement of transmission. Thus, at least for short-term activation the effect of BDNF is presynaptic (1998).

Norman continued to study cAMP-dependent LTP, using organotypic cultures from E18 rats, with Tzu-Ping Yu. Surprisingly she observed LTD with a mixture of Sp-cAMPS and the GABA receptor inhibitor, picrotoxin. Norman was following up this observation until a week before his death.

A TYPICAL DAY WITH NORMAN, 1983-2002

A typical day for Norman started at about 7:15, with tennis at the Athenaeum (Caltech's faculty club). Norman kept in excellent physical shape with a combination of athletics and dietary discipline, until arthritis crippled him after the age of 80.

Later in the morning Norman enjoyed presiding at a "club" meeting. Norman knew how to run a sizeable research group; he termed our subgroups "clubs" and organized meetings every two weeks. There was the GIRK Club, the Culture Club, the Slice Club, and many others. The lab attracted a wonderful series of scientists, born in 26 different countries between the years 1920 and 1980.

At these meetings Norman discussed expression tactics to understand the molecules we and others around the world were discovering and studying. Norman loved gene transfer, and he experimented with all appropriate techniques as they appeared: simple transfection, vaccinia virus, adenovirus, Sindbis virus, lentivirus, and adeno-associated virus. Norman particularly enjoyed discussing antisense RNA, and he paid attention to the rapid advances in siRNA that occurred in the last three years of his life.

Early on, students and postdocs had desks in both labs, Norman's in the sub-basement of the Crellin building in Chemistry and mine on the third floor of the Kerckhoff building in Biology. In 1991 we confirmed this intellectual merger with a full physical merger, and Norman and I occupied adjoining offices on the third floor of Kerckhoff.

Norman's copious memos issued from beloved yellow memo pads. They were usually clipped to photocopied papers, annotated in two colors for emphasis, and often with a self-deprecating heading such as "Enthusiasm of the moment dept." These memos sent some lab members on to a series of experiments that lasted only a week and were abandoned. Some of those experiments lasted a month and produced interesting data, or a year and were followed by a paper. But former colleagues report that a few of those memos led to an entire career of satisfying research.

Norman also attended Molecular Biology Lunch at the Athenaeum every Monday, and he kept the conversation focused on science. The yellow pads also came to seminars. Norman paid attention to every word and typically asked the most incisive question (in an appropriate memorial, the seminar room has been renamed Norman Davidson Hall). Norman and his yellow pad then dined with the speaker. Norman noticed the food only when it was unusually bad, and he again kept the conversation focused on science.

Norman's day was not yet finished. He usually had a phone conversation with a colleague. My children, who were born just when Norman and I started our partnership, simply expected him to phone at bedtime. When they became teenagers, we got phones for them, a phone for Margaret and me, and a phone for Norman.

Saturdays were half workdays for Norman, but Sunday nights were reserved for an excursion to a cinema and a modest restaurant. Norman and Annemarie took these excursions with several generations of young Caltech faculty, and Norman kept up with the latest ideas and trends during these excursions.

AMGEN

Norman was an original member of Amgen's scientific advisory board (in 1980), and he kept up contact with the company in Thousand Oaks, California, until just before his death. He was well regarded for excellent advice, explanations, and career guidance. Many papers from Amgen thank Norman for comments on the manuscript.

AWARDS AND LEADERSHIP POSITIONS

Davidson's awards included the Peter Debye Award by the American Chemical Society in 1971, the California Scientist of the Year in 1980, the Dickson Prize for Science in 1985, the Robert A. Welch Award in Chemistry in 1989, the National Medal of Science in 1996, and a McKnight Senior Investigator Award in Neuroscience (1997-1999). He was a member of the National Academy of Sciences for 42 years, a fellow of the American Academy of Arts and Sciences since 1984, and held an honorary doctorate from the University of Chicago.

Despite his primary commitment to bench research, Norman held key leadership positions. He served two terms as executive officer at Caltech, in the 1960s for the Division of Chemistry and in the 1990s for the Division of Biology. He also served as first chair of the faculty at Caltech in the 1960s and briefly as interim chair of the Division of Biology in 1989. On the national scene he was a founding member of the National Advisory Council to the Human Genome Institute.

WE THANK Annemarie Davidson, Judith Campbell, Eric Davidson, and Philip Sharp for help with this memoir.

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HARRY GEORGE DRICKAMER

November 19, 1918–May 6, 2002

BY JIRI JONAS

HARRY GEORGE DRICKAMER ("Doc" to all his students) was a pioneer in high-pressure studies of condensed matter, with a major focus on pressure tuning spectroscopy. The energies associated with different types of orbitals can be varied to different degrees by compression. From these perturbations a wealth of information can be obtained about the electronic and vibrational properties and molecular interactions in various systems. The concept of pressure tuning, which Harry Drickamer developed and exploited, became a tool of great power and versatility, presently used by many research groups throughout the world. Harry Drickamer's own research has had a strong impact in the fields of physical, inorganic and organic chemistry, chemical engineering, solid-state physics, geophysics, and biochemistry. From an experimentalist's point of view it is most remarkable that Harry Drickamer was able to develop and perfect high-pressure instrumentation for so many spectroscopic and nonspectroscopic techniques, to name a few: infrared spectroscopy, Mossbauer spectroscopy, fluorescence spectroscopy, X-ray diffraction, conductivity measurements, and scintillation experiments. Furthermore, he was the first to develop instrumentation for high-pressure experiments to many hundreds of kilobars.

Thanks to the research accomplishments of Harry Drickamer, pressure is widely recognized as a versatile and essential tool in condensed phase science: There are now hundreds of papers involving high-pressure studies, many of them extending, amplifying, and improving on the results presented in Harry Drickamer's original studies.

It is noteworthy that Percy W. Bridgman, who received the 1946 Nobel Prize in physics "for the invention of apparatus to produce extremely high pressures and for discoveries he made in the field of high pressure physics," recognized very early the exciting research potential of Drickamer's high-pressure work. In a 1960 letter Professor Bridgman wrote to Harry Drickamer: "My sincerest congratulations on a masterpiece of design and execution. I had no idea that there were such potentialities for further development in the crude arrangements which I used myself. I envy you the adventure of the many further discoveries that you are certain to make with it." The International Association for the Advancement of High Pressure Science and Technology was formed some years after Bridgman's death, and the association established a prize in his honor. It was fitting that in 1977 Harry Drickamer was the first recipient of the Percy William Bridgman Award. The fact that Drickamer received 22 major awards, including awards from the American Chemical Society, the American Physical Society, and the American Institute of Chemical Engineers, illustrates well the major impact of his work on diverse fields of science and engineering.

Harry George Drickamer was born Harold George Weidenthal on November 19, 1918, to Louise Weidenthal and Harold Weidenthal in Cleveland, Ohio. His father died when Harry was very young, and after his mother remarried, Harry's stepfather, George Drickamer, adopted him. He was educated in the public schools of East Cleveland

and in his teens was very actively involved in sports, particularly baseball and football. In fact, after graduating early from high school he played minor league professional baseball in the Cleveland Indians farm system. As was typical for him, he wanted to succeed and to be the best, but with more experience in baseball he realized that playing in the big leagues would require a very special talent. Harry knew he would not like sitting on the bench, and so he decided to quit baseball. Clearly, this decision determined his future career as a scientist and engineer. However, taking advantage of his athletic skills, he matriculated at Vanderbilt University on a football scholarship. As a result of an injury he soon transferred to Indiana University and then to the University of Michigan, where he enrolled in chemical engineering. He received a B.S. in chemical engineering in 1941 and one year later a master's degree in the same field. Harry was a successful and popular student, which led to his election as president of his class in the Engineering College.

A very important event in his life took place during these years: He met Mae Elizabeth McFillen, a nursing student at the University of Michigan, originally from Toledo, Ohio. Harry and Mae Elizabeth were married in New Orleans on October 28, 1942.

The next several years were pivotal in Harry Drickamer's professional career. In 1942 he took a position at the Pan American Refinery in Texas City, Texas. At this point he was not interested in pursuing a Ph.D. degree, but his fellow students played a prank on him by forging his name on a signup sheet for the Ph.D. qualifying exam in chemical engineering. Harry decided to take the 16-hour exam anyway. A couple of months later, after he started work in Texas, a notice came that he had passed the Ph.D. qualifying exam!

It was fortuitous that Harry Drickamer met another Harry, Harry Hummel, who was also starting work at Pan American with an M.S. degree from the University of Wisconsin. According to Harry Drickamer's own words, his new friend and colleague was much more sophisticated scientifically, and encouraged him to study physics and math. After solving many problems in physics and quantum mechanics, Harry Drickamer developed a taste for science and realized that he would like to become a scientist.

In addition to the 48-hour workweek, Harry Drickamer used a vapor liquid at night and on Sundays to collect experimental data that the University of Michigan permitted him to use as a part of his Ph.D. thesis. The other part of the thesis was a plant test on an extractive distillation tower—the first of its kind in the world. Together with Harry Hummel, Harry Drickamer published this work, which led to the Colburn Award from the American Institute of Chemical Engineers in 1947. This research experience impelled him to finish his Ph.D. and to look for an academic position. In February 1946 Harry Drickamer returned to graduate school at the University of Michigan and received his Ph.D.

Because he was more interested in science than pure engineering, Harry Drickamer accepted a faculty position offer from the University of Illinois at Urbana-Champaign, where chemical engineering and chemistry were in one department. Obviously this was the right decision, as Harry Drickamer spent all his professional life at the University of Illinois. After his initial appointment as an assistant professor of chemical engineering in 1946, he was promoted to associate professor in 1949 and to full professor in 1953. In 1958 he was appointed professor of chemical engineering and physical chemistry, and in 1983 he became professor of chemical engineering, chemistry, and physics. In recognition of his major contributions to science and engineering he was appointed professor in the Center for Advanced Study at the University of Illinois in 1963. Indeed, his work encompassed the fields of chemical engineering, chemistry, and physics, and his 105 doctoral students were drawn from all three departments: chemistry, chemical engineering, and physics.

At this point it is appropriate to mention Harry Drickamer's working habits. He used to visit all his students and postdoctoral fellows at least twice a day in the laboratory; in addition, the group met for afternoon tea or coffee, where they discussed not only research but also sports and current events. Saturdays and Sundays were working days for Harry. His former students recall how excited Harry Drickamer became when he heard about some new experimental results. His passion for research remained with him as long as he lived; several months before his death Harry was still enthusiastic and excited about a new project and shared with me his ideas during the morning coffee break we had enjoyed together for about 25 years. His students knew him as "Doc" and all of them recall fondly their experiences as graduate students in his laboratory. Many of his students became successful and prominent scientists. Election to the National Academy of Engineering, the National Academy of Sciences, and the American Philosophical Society represents some of the major honors received by his former students.

In 1995 Harry Drickamer's former students raised funds for a professorship in his honor, but he decided that it was more important to finance fellowships for graduate students in the departments of chemical engineering, chemistry, and physics. His influence on graduate education in these fields is illustrated by the fact that contributors to this fund were not limited to his own students but included others who had the opportunity to know him when they were students at Illinois.

Harry Drickamer's intense work habits still left him time to enjoy his family. He was very proud and involved with his five children, who are successful professionals: Lee, professor and head of biology at Northern Arizona University at Flagstaff; Lynn, technical library assistant in the Law Library at the University of Michigan; Kurt, professor of biochemistry at Oxford University in England; Margaret, professor of medicine at Yale Medical School; and Priscilla Atkins, a reference librarian at Hope College, Michigan, who is also a poet with more than 50 published poems. His three grandchildren were a source of great pleasure and joy to him.

Harry maintained his interest in sports and kept himself very fit, as he exercised on a treadmill and walked daily several miles to work and during the lunch hour. He never skipped his walks even during frigid Illinois winter days. To me, aside from science, he was a source of information about the intricacies of baseball, a sport I was ignorant about because of my Czech background. During our daily coffee breaks Harry also impressed me with his knowledge of U.S., English, and Greek history. His preferred reading was in history, particularly English history.

Harry George Drickamer died of stroke on Monday, May 6, 2002, in Urbana.

A survey of the main characteristics of his work is in order before a chronological narrative of Harry Drickamer's scientific accomplishments (478 publications). Harry's work encompassed a wide range of scientific problems; the common denominator was the innovative use of high pressure to obtain unique information about condensed phase phenomena. The diversity of problems and systems required the use of various spectroscopic and nonspectroscopic techniques. Consequently, Harry had to continue developing new instrumentation for his high-pressure experiments. The dynamic nature of his work is evident: Harry Drickamer explored and solved important problems in a specific field and then moved on to new areas. Although Harry was an experimentalist at heart, the goal of his experiments was to verify or test theories or to provide unique insights into topical phenomena. Harry's work was also influenced by his strong collaborations with his colleagues at Illinois. After joining the faculty at the University of Illinois, Harry Drickamer's early interests were in the area of fluids. Together with a radiochemist colleague, Robert Duffield, he designed an apparatus to measure radioactivity at high pressure and then studied the diffusion of molecules in liquids and gases.

In the period from 1955 to 1958, after he developed the unique instrumentation to investigate electronic spectra by optical absorption under high pressure, his initial studies dealt with transition metal ions and ligand field theory, absorption edges of Si, Ge, and a variety of II-V and II-VI compounds. According to Harry he was greatly encouraged by professors of physics Fred Seitz and John Bardeen, who took interest in this early work.

From 1960 to 1963 Harry Drickamer and his students developed instrumentation to measure electrical resistance at pressures of several hundred kilobars. At the same time Harry Drickamer started X-ray diffraction and Mossbauer resonance experiments at high pressure. The latter experiments were carried out in collaboration with professors Hans Frauenfelder and Peter Debruner from the Department of Physics. According to Harry another collaborator, professor of physics Charles Slichter, contributed in a major way to the theoretical interpretation of the Mossbauer resonance experiments on changes of spin state and oxidation state of iron in inorganic compounds.

The series of investigations that had the broadest impact on solid-state physics were Harry Drickamer's studies of insulator-conductor transitions. In the period from approximately 1958 to 1963 he and his students carried out measurements of the optical absorption edge and electrical resistance of a variety of materials; they observed that with increasing pressure many of these materials became metals or narrow gap semiconductors. These insulator-to-metal transitions were observed in I_2, Si, Ge, Se, and in various compounds, using a combination of optical absorption and electrical resistance measurements. Electrical resistance studies on Ca, Sr, and Yb indicated that elevated pressures transformed them from metals to semiconductors and at even higher pressures transformed them back again into metals. Harry and his students observed such transitions involving alkali, alkaline earth, and rare earth metals. In addition, paramagnetic-diamagnetic and ferromagnetic-paramagnetic transitions were observed in ferrous compounds and in iron. Many electron-donor complexes formed radicals that reacted to give chemical bonds of new types, and photochromic materials became thermochromic.

In the 1970s Harry's group developed techniques for studying luminescence in liquid solutions at pressures of 12 kilobars and carried out a variety of studies that tested theories of the effect of viscosity, dielectric constant, refractive index, and freezing on luminescence peak energy, intensity, and lifetime. As an example of yet another unique collaboration, Harry with Professor Gregorio Weber (Department of Biochemistry) investigated the effects of pressure on protein conformation, using intrinsically fluorescent amino acids and fluorescent ligands. These pioneering experiments opened a new approach in the field of protein folding.

Currently there are at least 25 laboratories around the world applying high-pressure fluorescence techniques to the investigation of biomolecules.

In the period from 1986 to 1990 Harry's group continued earlier studies, using optical absorption to study changes in molecular conformation in the ground state, in crystals and rigid polymers, including photochromic and thermochromic organic molecules, molecules with metal-to-metal bonds, and a variety of complexes involving transition metal ions with ligands.

Between 1990 and 1993 Harry and his students published a series of papers on thermoluminescence in doped ZnS phosphors, in irradiated alkali halide crystals, in crystalline quartz, and in coronine. In the case of ZnS phosphors, which are of considerable technological interest, the first identification of so-called deep levels and the elucidation of their behavior had important commercial implication. High-pressure luminescence experiments were also used to characterize molecular interactions in polymeric materials, including tests of theories of energy transfer between molecules, the effect of modifying ligands in organometallic compounds, and the study of molecules that can exhibit more that one conformation in the excited state.

Regarding the theoretical aspects of his work, it should be emphasized that Harry Drickamer's high-pressure experiments provided tests of many important theories, including Bethe's ligand field theory, Van Vleck's theory of the high-spin to low-spin transition, the Forster-Dexter's theory of energy transfer in phosphors, and Mulliken's theory of electron-acceptor complexes. Harry's pressure studies on Ni^{2+} in NiO were in good agreement with the predictions of Bethe's point charge model. High-pressure Mossbauer resonance studies allowed a direct test of Van Vleck's prediction that as the ligand field increased, it would become energetically favorable to pair the spins of d electrons in violation of Hund's rule. Indeed, high-spin to low-spin transitions were induced by pressure. Radiationless transfer of optical excitation is an important process in various applications, such as photosynthesis and fluorescent lighting. According to the Forster-Dexter theory the efficiency of transfer is increased as r^{-6} where r is the donor-acceptor distance, and is proportional to the overlap of donor emission and acceptor absorption peaks. As both these parameters change significantly with pressure, Harry's high-pressure studies found that the theory is remarkably quantitative.

An overview of Harry's research accomplishments would not be complete without a discussion of his pioneering contributions to high-pressure instrumentation. In the early 1960s Harry designed and built an optical cell with a special optical window that could be used up to 12 kilobars. Today many laboratories continue to use this superior window design for absorption, luminescence, infrared, and Raman studies on liquids, gases, and supercritical fluids.

In 1930 Bridgman invented the principle of massive support; in the middle 1950s Harry extended this design to include support on the taper as well as the flat. Using this technique he was able to carry out optical absorption and luminescence studies up to 150 kilobars. Further extensions of this design from 1959 to 1965 permitted the first electrical resistance, X-ray diffraction, and Mossbauer studies up to 200-300 kilobars.

Many of Harry's high-pressure experiments produced results of technological importance but the experiments on semiconductor heterostructures and quantum well heterostructures, carried out with Nick Holonyak, Jr. (John Bardeen Chair and professor of electrical and computer engineering and physics) and their collaborators, deserve special mention. The measurements of the pressure dependence of AlGaAs light-emitting diodes, near the direct-indirect transition, established definitive design limits (crystal composition) for high-brightness red-spectrum heterojunction LEDs. The absorption measurements carried out at high pressures on AlAs-AlGaAs superlattices provided special insights into semiconductor lasers, and represented the first high-pressure experiments on quantum well devices.

Harry Drickamer received many honors and awards for his pioneering and seminal work in several different fields of science and engineering. As an illustration I include citations from a few awards to show Harry's impact on chemistry, chemical engineering, and physics.

American Physical Society, 1967 Oliver E. Buckley Solid-State Physics Prize: "for experimental inventiveness, originality and physical insight leading to significant results on the effects of extreme pressures on electronic and molecular structures of solids."

American Society for Engineering Education, 1968 Victor Bendix Award: "for his contribution in engineering education and, in particular, his leadership in educating engineers who entered into the new field of the design of solids."

American Chemical Society, 1987 Peter Debye Award in Physical Chemistry: "for the development of high pressure as a widely applicable independent variable of central importance in the generation of physical chemistry that is crucial to the exploration of theories of phenomena in solids and liquids."

1987 Robert E. Welch Prize in Chemistry: "for his outstanding contributions to chemistry and all sciences concerned with the study of matter."

I WOULD LIKE to acknowledge Mae Elizabeth Drickamer for permitting me access to all the materials pertaining to the personal and research life of Harry George Drickamer.

PRINCIPAL AWARDS AND HONORS

1965 Member of the National Academy of Sciences
1970 Fellow of the American Academy of Arts and Sciences
1979 Member of the National Academy of Engineering
1983 Member of the American Philosophical Society

1947 Coburn Award, American Institute of Chemical Engineers
1956 Ipatieff Prize, American Chemical Society
1967 Oliver E. Buckley Solid-State Physics Award, American Physical Society
Alpha Chi Sigma Award, American Institute of Chemical Engineers
1968 Victor Bendix Award, American Society for Engineering Education
1972 William H. Walker Award, American Institute of Chemical Engineers
1974 Irving Langmuir Award in Chemical Physics, American Chemical Society
1977 P. W. Bridgman Award, International Association for the Advancement of High Pressure Science and Technology
1978 Michelson-Morley Award, Case Western Reserve University
1983 Chemical Pioneers Award, American Institute of Chemists
1984 John Scott Award, City of Philadelphia
1985 Outstanding Materials Chemistry, U.S. Department of Energy
1986 Alexander von Humboldt Award, Federal Republic of Germany
Warren K. Lewis Award, American Institute of Chemical Engineers
1987 Peter Debye Award in Physical Chemistry, American Chemical Society
Robert A. Welch Prize in Chemistry, Welch Foundation
Distinguished Professional Achievement Award, University of Michigan
1988 Elliott Cresson Medal, Franklin Institute
1989 National Medal of Science
Award for Outstanding Sustained Research, U.S. Department of Energy

1994 Doctor of chemical science honoris causa, Russian Academy of Science
1996 Gold Medal, American Institute of Chemists

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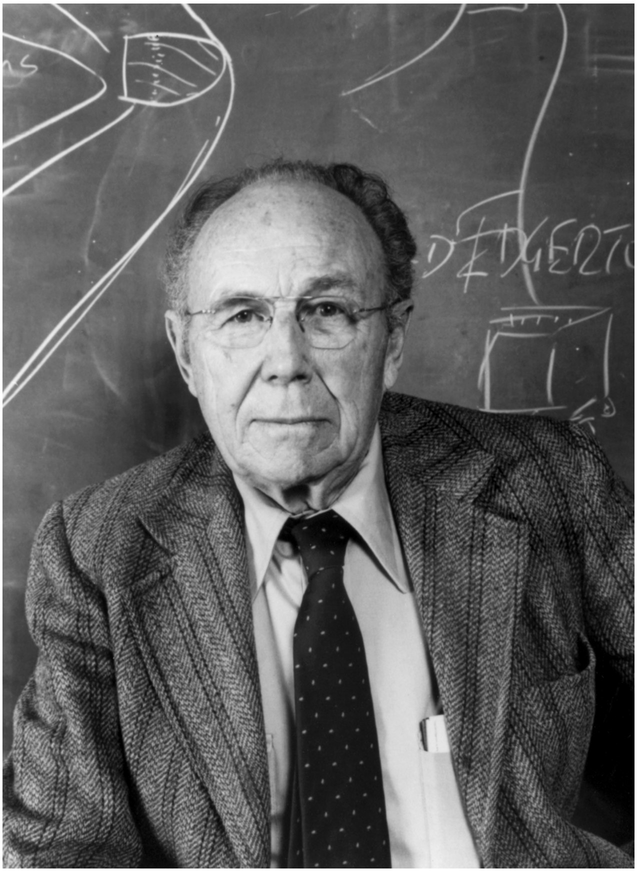

Harold E. Edgerton

HAROLD EUGENE EDGERTON

April 6, 1903–January 4, 1990

BY J. KIM VANDIVER AND PAGAN KENNEDY

HAROLD ("DOC") EDGERTON, born in Fremont, Nebraska, in 1903, transformed the strobe from an obscure technology to a fixture of American life. He made flashing light cheap and portable, and found endless applications for it, from the airport runway to the office copy machine. But despite his importance as an innovator, Edgerton is best known for the photographs he took. His images have become icons of the twentieth century: the drop of milk exploding into a crown, a bullet hovering beside an apple, an atomic blast caught the instant before it mushroomed, a smudge that might have been the flipper of the Loch Ness monster. His strobe photographs illustrated scientific phenomena in a way that was instantly understandable to millions of people. Later in his career he developed sonar tools that revolutionized marine archeology, again using images to explore the unknown.

From the 1930s on, Edgerton was the go-to man for anyone who needed a stroboscopic solution. Niels Bohr, golfer Densmore Shute, Jacques-Yves Cousteau, Colonel George Goddard, and a parade of other notables stopped by his lab at the Massachusetts Institute of Technology (MIT). Faced with many requests, Edgerton made key contributions to a variety of fields and garnered dozens of awards:

What other person can boast an Oscar, a Medal of Freedom from the War Department, and an induction into the National Inventors Hall of Fame?

Though Edgerton was celebrated as an innovator and photographer, he considered himself first and foremost a teacher. For decades he presided over some of the most popular classes at MIT. Edgerton kept his door open not just to his own protégées but also to the stray 14-year-old who might wander in with a question about cameras. Soon after his death in 1990, two science centers opened up to honor his memory, one at MIT and one in Aurora, Nebraska. Both Edgerton centers make it their central mission to carry on what he felt to be his most important work: hands-on teaching.

THE STROBE IN A SUITCASE

In the summer of 1933 Harold Edgerton and his wife, Esther, packed up their car and headed to Nebraska to visit family. It had been a momentous few years. In 1931 Esther had given birth to their first child, Mary Louise, and Harold had received his doctorate in electrical engineering from MIT. Now their baby slept in the front of the car and in the back, among the bundles and suitcases, sat the first strobe he'd developed for commercial use. Weighing 60 pounds, equipped with a mercury-filled tube that gave off a bright flash, and housed in a suitcase, the machine would have blended in with their other belongings. Edgerton planned to introduce it to every company he could find on his way from Massachusetts to the Midwest.

Early photographs show him as a spindly young man with brilliant blonde hair combed back in waves. Invariably, he wears a three-piece suit, and invariably the tie is askew. Already he had begun to exhibit the pluck that would become his trademark. Instead of booking appointments ahead of time so that he could demonstrate his strobe, Edgerton drove to Nebraska keeping an eye out for factory buildings. When he spotted one, he'd head to the nearest drug store, shut himself in the phone booth, and call the president of the company; oddly enough, top bosses always seemed to be available to the young man. Edgerton would announce that he had a strobe to show off: Did the company have any motors that whirred or shook when they shouldn't? Then, with the president's invitation, Edgerton would lug his suitcase into the factory and point his strobe at the offending gear or spring, synchronizing the flash with the movement of the motor part. Once he got the timing right, spinning gears appeared to stand still. Edgerton had found a way to photograph speed without the blur, allowing technicians to study the behavior of fast-moving objects.

At the General Motors Research Lab in Detroit he sold a dozen of his suitcase strobes. "It's lucky you were here today," the manager told Edgerton, and then led him into a room where a machine the size of a desk gave off a dim flash. "I was just about to order ten of these built by our people."[1]

As that anecdote illustrates, Edgerton was hardly the first to bring strobe technology into the factory. In the 1920s two French brothers, Laurent and Augustin Seguin, patented in Europe and the United States a "flash-producing apparatus," an unwieldy, expensive machine designed to help troubleshoot motors. Edgerton might have encountered one of these when he worked at General Electric in Schenectady, New York, in the mid-1920s. In the late 1920s as an electrical engineering graduate student at MIT, he worked under Vannevar Bush, using rudimentary computers to predict the behavior of motors: Results could be verified by flashing a strobe at the motor. Edgerton, the consummate tinkerer, was more interested in the strobe itself than in the theories it was used to test. "Though he was deeply involved in the mathematics of this problem in his masters and doctorate work as a student at MIT, his aptitudes were higher in laboratory work," according to his son, Bob Edgerton.[2] The young engineer had a hunch that the strobe could become a popular technology but only if it were repackaged. So he shrunk it down and gave it a souped-up tube, creating a flash bright enough to make compelling photographs.

SEEING SPEED

At first, Edgerton used his strobe to take pictures of moving motors—and only motors. And then one day in 1932 he aimed the flash at water flowing out of the faucet in his lab. Under a split-second burst of light the water turned motionless, bulbous, hollow in places, like an icicle from another planet (1939, p. 135). From then on, Edgerton would point his flash at the world around him, using photography to discover what the unaided eye couldn't see.

Again, he was not the first to do so. His predecessors had used flash photographs to investigate the motions of a pole-vaulter's body, drops of water, a horse's legs. Edgerton likewise created photographs that could answer scientific questions. But he also labored to make his images concise and compelling, as easy to read as advertisements, so that they would speak to people with no scientific training at all. In his photos of athletes in motion he showed a tennis racquet curving under impact, the toe of a boot embedded deep inside a football, a baseball that melted against the bat, taking viewers on a tour of the secret world of high-speed impact, where hard objects turn to mush (1939, pp. 55-115). In his hummingbird series for *National Geographic* magazine he posed the birds around a ruby-lipped girl in puffy sleeves who could have passed for Tenniel's Alice in

Wonderland; the tiny birds, with their wings stilled, appear to be floating around her. Most famously, Edgerton fussed over his drops of milk for 25 years, plinking and plunking, rejecting misshapen splashes, waiting for the milk to fly up into a near-perfect coronet. In the resulting 1957 photograph Edgerton finds a gorgeous logic even in spilt milk (1987, p. 127).

Though he never billed himself as an artist, the art community—and Hollywood—embraced him. In 1937 his works hung in the Museum of Modern Art's first photography exhibition. In 1940 MGM studios invited him to fly out West and show his stuff; he ended up collaborating on, and starring in, a short film, *Quicker Than A Wink!,* that won an Academy Award.

Most impressive, Edgerton's images remain familiar today. Charlie Mazel, who worked with Edgerton on marine archeology projects, found that out when he gave a slideshow to a room full of people. "I said, 'How many of you have heard of Doc Edgerton?' Only two hands went up. Then I showed a picture of a bullet going through an apple. Every hand went up. The image outlives the name. A hundred years from now people will still know those pictures."[3]

POPULAR MECHANICS

Right from the beginning Edgerton understood that the strobe could do more than provide data. It could entertain. By the late 1920s he had already hatched the idea of taking his gear to downtown Boston and catching dancers in midleap, to use the flash as a new window onto popular spectacles.[4] However, in those early years his equipment was not equal to the task of lighting cavernous arenas.

But a decade later Edgerton and his partners—his former students Kenneth Germeshausen and Herbert Grier—had tweaked the machine, replacing the mercury gas with argon, for a brighter flash. In 1938 Gjon Mili began using Edgerton's techniques to produce arresting art photographs in *Life* magazine. Still, commercial success remained elusive. Edgerton complained that engineers at Eastman Kodak Company regarded the strobe as a novelty item rather than a potential blockbuster. Even so, in 1939 Eastman Kodak struck a deal with Edgerton and his partners to develop a strobe for professional photographers, the Kodatron.

The next year, when a newspaper photographer named George Woodruff showed up at the lab, Edgerton hit on the gimmick that proved the commercial appeal of strobe once and for all. The two men hauled equipment to the Boston Garden, a sports arena with a busy schedule of races, rodeos, fights, and circuses. That day, they found a track meet in progress. Edgerton set up some of his largest lights and handed Woodruff the camera. When a pack of runners rounded the corner, Woodruff snapped the shutter and the flashes popped. The photo captured the runners hovering in the air, every fold in the fabric of their shirts delineated, their straining muscles petrified into odd shapes. It was an utterly novel take on news photography, and sports pages around the country ran it.

In the following weeks Edgerton returned to the Garden to photograph whatever happened to be there, including skater Sonja Henie. "All the [news] wires in the country were loaded with these beautiful, well-lighted strobe pictures," he remembered later. "It broke the impasse."[5]

For Edgerton, developing a new technology was only the first step. You had to go out and convince folks that they needed it. This he did, tirelessly. Boy Scout troops, garden clubs, and old-age homes; no group was too small for Edgerton's attentions. "People used to say, 'You're crazy,'" Edgerton wrote, about his willingness to lecture. "But I found that practically every time you go out, say to a Rotary Club out in Fall River, Massachusetts, you've got a cross section that covers that whole town. . . . There will be one man at the end of the demonstration [who] will up and say, 'Hey, there's a factory down here that needs that real bad.'"[6]

Edgerton gave his lectures with democratic abandon because he knew the strobe would succeed only if he won over thousands of ordinary people. For the same reason, he carried a stack of postcards in his pocket, reproductions of his famous photographs with his phone number on the back. He handed these out to everyone he met, sometimes with an invitation to stop by the lab. The postcards were tickets into the world of Doc. All were invited.

WAR AND PEACE

"The war effort started on a Saturday when an unannounced visitor popped into the laboratory and said his name was Goddard," Edgerton remembered later of the events of 1939.[7] Lt. Colonel George Goddard, the man in charge of the Army's aerial photography effort, had come to Edgerton for help with a problem that bedeviled the military.

The Allies needed to illuminate vast areas during the nighttime, to take aerial photographs of roads and bridges in order to track the movements of the enemy. During World War I, the military had used a system that involved tossing tins of flash powder from the hold of the plane, a method with obvious drawbacks. He hoped Edgerton could come up with a strobe lamp that could safely and reliably illuminate cities from thousands of feet above.

Then Goddard—with a dramatic widow's peak plunging down his forehead, a ramrod military posture, and a promotion to brigadier general in his future—proposed that they go to the circus. That, he revealed, was why he was *really* in town. He wanted to shoot circus photos with

Edgerton. And so the two men packed up strobes and cameras and headed off.

Edgerton had begun by thinking small, miniaturizing the strobe so that it could become a useful consumer item. During the war, he thought big: lamps the size of tympani drums that were capable of casting beams through a mile of darkness. Edgerton flew with crews in Italy, Britain, France—at one point taking a turn on a machine gun that poked out of the side of a B-24. The result: He and his team produced bright-as-day shots of the terrain. General George Patton depended on these photos to plan his route into Germany. Edgerton's aerial strobe photographs gave the Allies a crucial edge over the enemy.

After the war, he decided to treat his family to a cross-country trip. Edgerton's vacations were usually ambitious—and productive—affairs, and this one was no exception. Since the 1930s, he had been aiming his strobe at birds and bats, freezing them in the air to study the secrets of their flight. Now, because "he wanted to do something in the summertime with the family," according to Bob Edgerton, he organized an expedition around the country to photograph hummingbirds. He'd bought a 1941 Ford Woody (the station wagon that came to be identified with surfers) and into this he packed himself, Esther, three children, and two strobes. They spent weeks camping in Army-surplus tents.[8]

When the war ended, war work did not. The Atomic Energy Commission approached Edgerton and the two partners, Germeshausen and Grier, and requested that they form a corporation. The three men had been working together since the 1930s, and many of Edgerton's designs (including the original strobe) had been put together in collaboration with one or both of his partners. They had sealed deals with handshakes; Esther had been their bookkeeper. But, in 1947 with government contracts coming in, they made their arrangement official.

The new corporation, Edgerton, Germeshausen, and Grier (later EG&G), took over the job of photographing nuclear tests from the Army Air Corps. The task required a camera shutter that captured exposures of 1/1,000,000th of a second, too fast for moving parts. EG&G came up with a novel solution. Instead of a conventional shutter, two polarizing filters kept the film in the dark until the filters were exposed to a magnetic field. For an instant, as the plane of polarization rotated, light was able to hit the film. Thus, EG&G obtained its infinitesimally small exposure times.

The resulting photographs revealed the atomic blast as a bubble of light hovering over the desert, pocked and malevolent. Edgerton, who watched the fireball through a piece of glass from miles away, marveled at the silence in which it unfolded. The sound took half a minute to travel over the desert, roaring only as the cloud itself began to decay.[9]

In time EG&G would grow into a Fortune 500 company, one of the top providers of technical services to the U.S. government and industry. Edgerton, ever focused on practical concerns, regarded the EG&G offices as an adjunct to his lab, a place to scrounge up secretarial help and machine parts. "It would be Sunday morning and he'd realize he needed some stuff and he knew where it was over at EG&G," according to Sam Raymond, who worked for the company and then went on to found Benthos Corp., a maker of oceanographic equipment. Edgerton, dressed in his usual technician's outfit of rumpled khaki shirt and khaki pants, would use wire cutters to force his way into the stock room. When security guards stopped him, he told them that it was OK. He was one of the owners.[10]

UNDER WATER

Like so many of his other ventures, Edgerton's foray into sonar and underwater photography began with a visitor in his lab. In the 1930s Newton Harvey, one of the world's leading experts in bioluminescence, dropped by to ask for tips on taking pictures of deep-sea fish. Harvey had tried to protect his cameras in watertight, pressure-proof casings, with no luck. "Harvey's casing design was a square box, as I recall, which distorted badly, causing cracks that leaked," Edgerton wrote. "I became interested in the problem. Why not a spherical design or even a cylindrical one? Soon, I was sketching all sorts of designs."[11]

So Edgerton was prepared when Jacques-Yves Cousteau made the pilgrimage to MIT in 1952. Cousteau hoped to find an alternative to the dangerous rigs that divers were then using to take underwater photos. Edgerton had never heard of the French explorer, but the two men immediately hit it off. Within a few hours of his arrival, Cousteau had found his way to the MIT pool, where he tested out Edgerton's equipment. The men planned an expedition to the southern coast of France, to study clouds of living organisms in the ocean, called the deep scattering layer.

In the summer of 1953 teenaged Bob Edgerton and his father flew to Marseille, where they squeezed into Cousteau's crowded ship; Bob slept in a 6-foot-long drawer in the boat's workshop. The pair, dubbed Papa Flash and Petit Flash by Cousteau, joined evening jam sessions with the crew, improvising on pots and pans. During that trip, as well as the Cousteau expeditions that followed, Edgerton lowered his cameras and flash units off the side of the boat, to catch glimpses of the ocean at a variety of depths.

Edgerton did not confine his innovations to the camera work; he also came up with ways to improve the ship itself.

"In the mid-1950s, he realized that the heavy cables used to hang the camera and to anchor the ship could be replaced with nylon ropes" that would be nearly weightless in the seawater, according to Bob Edgerton.[12]

In the beginning of his undersea career Edgerton struggled to get the best photographic images he could, but he soon discovered that photography was not well suited to the murky conditions near the bottom of the ocean. Increasingly he came to rely on sonar as an alternative to photography. At first, he'd used a sonar "pinger" to find the ocean floor, so he knew where to position his photographic equipment. Later he dispensed with the cameras entirely, turned the pinger sideways, and dragged it along behind the boat. By taking continual side-looking soundings he could track the contours of the ocean floor, creating an image out of sound. This side-scan sonar, which had previously been available only to the military and academic communities, was perfected as a commercial tool by a team led by Marty Klein at EG&G. The new tool revolutionized the field of marine archeology and many other areas of ocean exploration. Where divers feared to go, it could explore.

From the 1960s on, shipwreck hunters and archeologists alike called Edgerton with endless invitations to join their expeditions: the Civil War ship *Monitor*, King Henry VIII's flagship *Mary Rose*, the HMS *Britannic,* and even a site rumored to be Atlantis. Edgerton almost always said yes. "Doc would just literally get on the next plane. Whereas maybe somebody else might stew about [an expedition] and plan it for months, he'd be on the next plane with all his stuff," remembers Marty Klein, a former student who later founded Klein Associates, a manufacturer of side-scan sonar equipment.[13]

In the 1970s Klein collaborated with Edgerton on creating equipment that could probe the depths of a Scottish lake, to turn up evidence of the Loch Ness monster or whatever else might be hidden in the peat-clouded water. The search attracted worldwide attention and became a staple of TV documentaries, so much so that cartoonist Garry Trudeau saw fit to poke fun in *Doonesbury.* Edgerton appears in pen and ink as a rumpled techie in a Cousteau-style watch cap. "It's a good thing," he says as he contemplates a sheet of Loch Ness data, "I've got tenure."[14]

Although the fabled monster was never found, Klein and the late Charles Finkelstein, another Edgerton student, did discover a World War II Wellington bomber, possible caves in the walls, as well as submerged stone circles in Loch Ness.

DOC

For Edgerton it was more than a casual honorific—everyone called him Doc. His son, Bob Edgerton, suggests why the nickname fit so well: "He was a Midwestern homespun guy. Someone who you have respect for, but he's friendly—like a country doctor."[15] The name brought together his two identities: MIT whiz and Nebraska burgher.

His family moved around during his childhood, but by the time Harold Edgerton was in junior high school, they'd settled in Aurora, Nebraska, a farming community with a power plant at its center. Edgerton spent his undergraduate years at the University of Nebraska, but got his real education at the power company, where he worked as a grunt and as a lineman. "I was climbing poles and working with 2,300 volts hot during storms when I was still in high school. . . . You learn, first of all, that the time clock doesn't matter and the real value is to get that power on."[16]

The nickname, "Doc," fit in another way, too: It announced his dedication to teaching, not just as a job but also as a way of life. Many of his students, as well as strangers who wandered into his lab, remember his avuncular warmth. He was willing to spend hours helping a young person to develop a thesis topic or solve a technical problem.

"As president of MIT [from 1971 to 1980], I was not above putting Doc's involving nature to use myself," recounts Professor Paul Gray. "Once a freshman came into my office to protest how inappropriate it was for us to have missiles on campus. He said he had found one in tall grass behind the swimming pool. I told him I'd look on my way home that night, and sure enough, lying on a wooden sled was a 16-inch naval shell. It was disarmed, of course, and I knew right away whose it was. Doc modified such shells and added plumbing to make pressure chambers for testing undersea cameras. After I wrote the young student an explanation, I suggested he drop by Doc's lab and get acquainted. Like many freshmen, he didn't yet appear to have his moorings and I thought the exposure to Strobe Alley couldn't hurt."[17]

Edgerton's hospitality didn't stop at the lab: He invited his students home to sample Esther's cooking and sing backup on "You Are My Sunshine," which he twanged on the banjo. An assignment sheet from a 1946 class reads as follows: "Appear at 205 School Street, Belmont, about 6:30 p.m. equipped with appetite. No textbooks, slide rules or class notes will be allowed. . . . Try to memorize words of these Tech songs. Penalty for nonperfection may be an opportunity to help with the dishes!"[18]

I (J.K.V.) was lucky enough to receive a good deal of Edgerton's attention when I worked as a teaching assistant in his Strobe Lab in the fall of 1972. He assigned me a project of my own, a tremendous kindness, considering that professors often expect nothing more from their teaching assistants than a stack of graded papers. He suggested I find a way to produce schlieren photographs in color. Schlieren photography—the name comes from the German word for "streak"—records variation in the densities of gasses. It is a way to make phenomena that are usually invisible to us, like shock waves and vapors and heat, into something we can see on film. A candle, for instance, appears to be surrounded by billowing veils and ghostly bubbles. When Edgerton suggested schlieren to me, no one had figured out how to create a sharp, full-color image using the effect. The results promised to be stunning.

After some library research, I built a system with 10-inch mirrors and brought some slides in to Edgerton in about October 1972. He pulled his 10-power magnifier from his pocket and held one of my slides up to the light for inspection. "Van," he said, "It looks out of focus to me." So I tried again a few weeks later with another technique. "Van," he said, "I don't like the color."

I was stumped until I came across an article in *Scientific American*; a graduate student named Gary Settles had found a new way to produce four-color schlieren photographs. I combined Settles's technique with a high-speed flash, capturing the shockwave of a bullet in blue on a red background. This time when I came to Edgerton with the slides in January 1973, he said, "Van, I think you've got it."

In August of 1974 *Scientific American* ran the schlieren photographs: a heady turn of events for a graduate student. That same summer Edgerton wrote to the organizers of the Eleventh International Congress on High-Speed Photography and arranged for the pictures to be displayed in a gallery during the conference. The editor of the journal *Nature* saw them and decided to run one of the images on the November 1974 cover. When the photographs appeared in these high-profile magazines, Edgerton insisted that my name be first in the credits, even though I'd relied on him for equipment, lab space, expertise, and counsel. I am at MIT today because of the boost he gave me at the beginning.

He supported generations of grad students like me; some of them he literally supported by paying them out of his own salary. "He 'invented' a summer job for me at EG&G when we were expecting our second child," according to Gray. "But that was nothing compared to what he quietly did for others when need arose."[19]

THE FACE OF MIT

The hallway echoed with the report of gunshots. Flashes jumped across the walls. Boxes spilled wires, capacitors, barnacled wood. By contrast, other wings of MIT seemed downright sterile. Strobe Alley, the hallway that cut a line between Edgerton's labs, sucked visitors in and invited them to become part of the action. To make his lair even more inviting Edgerton hung displays all along the hall: photographs, framed bits of equipment, buttons to push. Klein, who wandered into Strobe Alley as an undergraduate in 1961, loved the tantalizing smell of the place. It reminded him of the junk shops in lower Manhattan, the perfume of "connectors and coils and motors—sometimes motors that have burned out."[20]

The MIT campus tour would invariably take prospective freshman through this wonderland, where Edgerton, the reigning spirit, would emerge to shake hands and pass out postcards. He had become a star attraction at MIT, a living advertisement for the institute and for a certain spirit of inquiry. "If you don't wake up at three in the morning and want to do something, you're wasting your time," he famously said.

In 1964 he was elected to the National Academy of Sciences. Four years later he retired from his duties at MIT, but only on paper. He went right on teaching and stayed put in Strobe Alley. Only in the late 1970s did he begin to slow down.

"In 1980, I had the opportunity to work with a French group in Mauritania in West Africa," remembers Mazel. He asked Edgerton for some equipment, and was dismayed when the 77-year-old, who'd just had a stroke, insisted on joining the expedition. "I wasn't comfortable with saying, 'Doc, you can't do that.' But I was getting phone calls from Esther in the background saying, 'Make sure he doesn't go.' He didn't go."[21]

In the 1980s he ignored the stroke, dizziness, a blood clot, and heart problems, reporting to his lab as usual, and even taking up new interests. It was his heart that finally got him. He died in the MIT faculty dining hall on January 4, 1990. He was 86.

Those of us who'd been close to him—who had been changed by him—wanted to make sure that Edgerton would live on at MIT. His attorney, Marty Kaplan, brought a group of museum experts to campus in order to discuss how best to conserve the trove of photographic prints scattered around the lab. "Find out what Doc meant at MIT," Kaplan instructed them. The museum people interviewed Edgerton's friends, including myself (J.K.V.). We agreed that what we wanted most to preserve was the spirit of Strobe Alley itself, a place where anyone could walk in and push buttons, study photographs, smell the burning machines, and find help with a project.

So that's how the Edgerton Center came to open its doors in 1992. Housed in the Strobe Alley and its surrounding rooms, the center strives to follow Edgerton's magnificent example of hands-on teaching and generous mentorship. Any MIT student who needs help with a project will find the Edgerton Center stocked with lab benches, testing equipment, photography resources, and staff. In addition, the center hosts a hands-on educational outreach program for local teachers and their classes. It serves as a link between

MIT and the larger community, much as Edgerton himself once did.

Just after Edgerton's death in 1990, his admirers in Aurora, Nebraska, also began to discuss what they could do to preserve his legacy, and they came to the same conclusion we did at MIT. The Aurora planners realized that "for all Edgerton's fame as a photographer and an engineer, he'd always viewed himself as a teacher. So we conceived the notion of a center that would carry on the tradition of hands-on learning that he believed in," according to Phil Nelson, president of the Edgerton Education Foundation.[22] The Edgerton Explorit Center, which opened in 1995, now serves about 100,000 people a year with science exhibits and teaching programs, traveling demonstrations, workshops, and lectures. It has become Nebraska's premiere science education center.

What did Doc Edgerton give the world? Unforgettable photographs, 47 patents, and a new way of looking at objects in motion. Perhaps more important, he doled out thousands of kindnesses to students and friends, dispensing favors as eagerly as the postcards he kept in his pocket. It is this legacy—his habit of generosity—that we especially hope to preserve at MIT.

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NOTES

1. Harold Edgerton, interview by Marc Miller, transcript of tape recording, Cambridge, Mass., August 26, 1975, pp. 37-39. Collection MC 132, MIT Institute Archives and Special Collections, Cambridge, Mass.
2. Bob Edgerton, interview by Pagan Kennedy, tape recording, Somerville, Mass., December 30, 2003.
3. Charles Mazel, interview by Pagan Kennedy, tape recording, Cambridge, Mass., December 19, 2003.
4. Ken Beardsley, letter on occasion of Edgerton's retirement, undated, Collection MC 25, MIT Institute Archives and Special Collections, Cambridge, Mass.
5. Harold Edgerton interview, pp. 85-86.
6. Ibid, p. 58.
7. Ibid, p. 103.
8. Bob Edgerton interview.
9. Harold Edgerton interview, pp. 116-117.
10. Marty Klein and Sam Raymond, interview by Claire Calcagno, transcript of tape recording, Rockport, Mass., December 20, 2002.
11. Harold Edgerton, "Underwater Photography," January 28, 1985, and September 18, 1985, p. 2. Collection MC 25, MIT Institute Archives and Special Collections, Cambridge, Mass.
12. Bob Edgerton interview.
13. Marty Klein and Sam Raymond interview.
14. Garry Trudeau, *Doonesbury* cartoon, 1976.
15. Bob Edgerton interview.
16. Harold Edgerton interview, pp. 1-2.
17. Paul Gray, "Unforgettable 'Papa Flash,'" August 1, 1990, p. 5. Collection MC 25, MIT Institute Archives and Special Collections, Cambridge, Mass.
18. Harold Edgerton, "Fundamentals of Electrical Engineering," assignment sheet for May 3, 1946, Collection MC 25, MIT Institute Archives and Special Collections, Cambridge, Mass.
19. Paul Gray, p. 12.
20. Marty Klein and Sam Raymond interview.
21. Charles Mazel interview.
22. Phil Nelson, interview by Pagan Kennedy, tape recording, Somerville, Mass., January 5, 2004.

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HOWARD E. EVANS

February 23, 1919–July 18, 2002

BY MARY JANE WEST-EBERHARD

HOWARD ENSIGN EVANS, one of the twentieth century's leading entomologists and insect natural historians, was born in Hartford, Connecticut, the son of Archie James Evans and Adella Marian Ensign. He was also, in his spare time, a talented writer of popular books and articles on natural history and conservation.

Howard Evans's love of nature began on the Evans family farm near East Hartford, Connecticut, a 60-acre tobacco farm that was purchased, with the help of financing from his maternal grandfather, Howard Ensign, when his parents were married. Howard's mother was his father's second wife. She had been teaching school after having studied education at a normal (state teachers') school, and Howard was her only child, though he had a stepbrother and three stepsisters by his father's first marriage.

In his childhood Howard Evans was strictly an applied entomologist. Here is what he wrote about that stage in his life.

I suspect that when most people dig into the recesses of their minds for their earliest childhood memories they come up with scenes of kittens, puppies, or hamsters. My earliest memories are of tobacco hornworms, and how delightfully they pop and ooze between bare toes. Picture a tobacco farm in the Connecticut Valley, with kids walking up and down the rows looking for big, green caterpillars and executing them by the most primitive of control measures (Evans, 1985, p. 145).

As a youth, Howard helped to found the Hockanum Nature Club, a museum in a woodshed with collections of pressed leaves, wildflowers, birds' eggs, and insects. The name of the club came from the local American Indian name for the region of the Evans farm in East Hartford. Howard Evans undoubtedly had something to do with choosing the name, for later he often used indigenous words as names for new species of insects discovered during his travels, for example, naming the Australian sand wasp *Bembix mianga*, a fly-catching species, after the aboriginal word (*mianga*) for "fly, " and *B. uloola*, a bright orange species, after the aboriginal word, *uloola,* for "sun" (Evans and Matthews, 1973). His "first wealth (several dollars!)" came when he sold to a neighborhood hobbyist some of the moths that were attracted to the lights of the family fruit stand (Evans, 1968a, p. 25).

Life on the farm was ended not by tobacco hornworms but by a series of hailstorms and a drop in the tobacco market that drove Howard's father to other crops and eventually to bankruptcy during the Great Depression of the 1930s. But his rural background was a lasting influence and inspiration. Even as an undergraduate Howard wrote, in a classroom essay on "Experiences With Insects,"

I think the modern mind tends to debunk, or at least to minimize, the values and advantages of being country bred. . . . But I am sure that the appreciation of country life is merely going under a cloud, and will emerge again when the people once more take to the country rather than swarming in the cities like flies on rotten fruit. . . . Although my family moved away from the farm into the suburbs a few years ago, my absence from the country has tended to accentuate rather than suppress my affinity for the things of nature. . . . My hobbies then were not the ordinary ones, such as stamp or coin collecting, but consisted of recording the living things I saw,

> especially the birds, and, best of all, collecting insects. . . . I was an odd but happy figure in those days, roaming the countryside with a net in one hand and a pair of binoculars in the other. . . . I first became really interested in insects when I began to notice the attractiveness of certain moths which swarmed around the street lights. . . . The idea of making a net and of mounting what I caught in boxes of cotton covered with glass was adopted from a friend engaged in the same diversion. I soon became fanatic about the business, and, much to my parents' disgust, spent hour after hour chasing "bugs" over field and stream.

Howard credited his father with his "workaholic tendencies," and said that he was encouraged in his interest in natural history by his mother, who taught him the names of many birds, insects, and stars. Even so, when he went to the University of Connecticut in 1936 he started out as an English major. He switched to biology after his first course in entomology, taught by J. A. Manter, described by Howard as "a very unusual teacher," who "in his quiet way . . . introduced me to the world of professional entomology" (Evans, 1968a, p. 25). After writing an undergraduate thesis on insects reared from the downed trees and branches of the 1938 hurricane, he graduated magna cum laude in 1940.

The intention to major in English probably reflected his lifelong passion for writing. His first book was a volume of poetry titled *The Song I Sing* (Evans, 1951), a compilation of poems previously published in the Hartford newspapers. During spare moments while later in the army he wrote a novel that he later destroyed. Throughout his active life as a scientist he wrote popular books, not all of them related to entomology. The best known of his 16 books, *Life on a Little-Known Planet* (Evans, 1968a), was translated from English into French, German, and Japanese, and was reprinted many times during the more than 30 years that it has remained in print. Unlike some popularizers of science, Evans did not lose respect in science as a result of his popular writing, because his scientific output—a lifetime total of 265 scientific publications, including a number of books and monographs—was undiminished by his avocation as a writer for the general public. It was as if he led two highly productive lives in perfect harmony with each other.

Immediately after college graduation Howard worked at the Connecticut Agricultural Experiment Station in New Haven, and then went to Cornell, where he completed a master's thesis on spider wasps (Pompilidae). Then, in December of 1941, while he and his mother listened to the New York Philharmonic on the radio, indulging a love of classical music acquired while in college, he learned of the attack on Pearl Harbor. Howard asked his draft board to move his name to the top of the list. He spent four years in the army. Because he already had a master's degree in entomology, he was assigned to be a medical laboratory technician in a hospital in Newfoundland, where he discovered that a mysterious ailment of servicemen was being caused by the parasite *Giardia*. Probably as a result of that discovery he was promoted to second lieutenant upon return to the United States. He spent the rest of the war at a base hospital in North Carolina working as a parasitologist on stool samples from servicemen returning from the Philippines. In one of his books (Evans, 1985, p. 125), he said of this experience that "in a grim and odorous way, it was rather fun." Following this interlude of service in the army he was able to return to graduate studies at Cornell without financial problems, thanks to the GI bill. At Cornell, with J. Chester Bradley and V. S. L. Pate as cochairmen of his doctoral committee, he finished a doctoral thesis on the systematics of the tribe Pompilini (Hymenoptera, Pompilidae), and then joined the faculty of Kansas State University in Manhattan, Kansas, as assistant professor of entomology.

At Kansas State he taught courses on general entomology, immature insects, and morphology and curated the insect collection (from an unpublished "History of the Department of Entomology, KSU," by Herbert Knutson, deposited in the department; courtesy of John Reese). There he spent three productive years (1949-1952) studying the behavior and systematics of sand wasps, along with his graduate students Carl Yoshimoto and C. S. Lin. During this time he expanded his general interest in animal ethology with the encouragement of a fellow faculty member, A. M. Guhl, a well-known student of social dominance in chickens, and read works by Lorenz, Tinbergen, Thorpe, and others. During this period, he also took a summer field trip to Mexico with the late Paul D. Hurd, Jr., of the University of California.

Howard Evans and Mary Alice Dietrich were married in 1954, soon after Mary Alice had finished her Ph.D. in science education at Cornell and not long after Howard had returned to work there as assistant professor of entomology in 1952. They had three children, Barbara (Galloway), Dorothy (Tuthill), and Tim. Mary Alice was the daughter of the Cornell entomologist Henry Dietrich, who had "warned his daughters to stay away from entomologists, who were likely to be impecunious and little appreciated by society." "Fortunately," Howard wrote, "Mary Alice failed to take his advice" (Evans, 1985, p. 217). Howard declared in his autobiographical notes that "few persons have been lucky enough to enter a partnership with someone so congenial and supportive." He considered his marriage to Mary Alice his main not-exactly-scientific achievement, and meeting her in 1953 "the most important (and fortunate) event in my life." During the early years of their marriage the Evanses lived on 8 acres of land on South Hill in Ithaca, New York, adjacent to Buttermilk Falls State Park, a home that became the inspiration for *Wasp Farm* (Evans, 1963), one of Howard's most successful books and a nominee for the National Book Award. With Mary Alice as senior author, they wrote a 363-page scholarly biography of Harvard entomologist William Morton Wheeler (Evans and Evans, 1970), a major figure of early twentieth-century science, whose story as recounted in the Evanses' biography gives a fascinating view of the issues, personages, and Old World influences that marked biology in the United States at the turn of the twentieth century.

I first met Howard Evans in 1966 when I was a graduate student. By that time he had moved from Cornell to Harvard, and I had an appointment to meet him in his office at the Harvard Museum of Comparative Zoology, the MCZ. I had heard that Evans was shy and reserved, a man of few words. What if we would end up having nothing to say? I soon found out that Howard Evans was the kindest and least pretentious of men, and he was not the least bit shy when it came to talking about insects. Later I learned that some people misinterpreted Howard's shyness as snobbishness. One person told me that she had ridden with Howard on the excruciatingly slow MCZ elevator many times over the space of an entire year without Howard ever saying a single word. He would just stare absently and wouldn't smile or attempt small talk. Those who knew Howard well, especially those who spent time with him in the field, learned that he was a person who was not embarrassed by silence.

When I went to the MCZ as Howard's postdoctoral associate in 1967 he was at the height of his productivity. The year before, 1966, had been what he later called his "banner year." In that single year he authored 10 publications, totaling well over 1,000 pages. They included his now classic synthetic review on "The Behavior Patterns of Solitary Wasps" (Evans, 1966a), a 526-page book on *The Comparative Ethology and Evolution of the Sand Wasps* (Evans, 1966b), and a 443-page monograph on the systematics of pompilid wasps (Evans, 1966c), an astounding list of achievements for one man in one year. In 1967 Howard was awarded the William J. Walker Prize of the Boston Museum of Science, for contributions to natural history.

In addition, he produced a constant stream of high-quality popular works that did much to promote entomology and conservation in the public realm. He accomplished this prodigious output by dividing his workday strictly into two parts: While at the museum he did his "scientific" work and at home he did what he called his "literary work," meaning work on the essays and books that were outside his museum duties. At home he often played recorded music while working or relaxing. He would always leave his desk at the museum completely clear of clutter when he went home at the end of the day.

Some think that Howard's clean desk top was made possible by a set of messy drawers underneath, but I think it was the same orderly discipline that enabled him to accomplish large amounts of work and writing without pause for the 54 productive years spanning his career. Whatever his secret for rapid publication, Howard never seemed pressured or nervous. He always had time for students. He never seemed too busy to write an encouraging letter to an amateur insect enthusiast, or to a kid hoping for a career in entomology.

E. O. Wilson recently told me the following story from those days at Harvard. When Howard was in his office he sat hidden from view behind a high bookcase, and the department secretary worked at a desk on the other side, near the door to the collection. If you came for department business you never knew whether Howard was present on the other side of the bookshelf, and if he ever listened to what went on there, he never let on. Ed Wilson decided to put this to the test by performing an experiment. He knew that Howard and Mary Alice were working on their biography of Wheeler, and that they had spent hours interviewing Wheeler's daughter Adaline. But there weren't many other people still around who had known Wheeler. So, to test Howard's quiet discretion, Ed walked in and said to the secretary, in a squeaky imitation of a 95-year-old voice, "I am a friend of Professor Wheeler, and I'd like to see him." Howard instantly popped into view, revealing himself to be as much an eavesdropper as anybody else.

Howard considered his move from Harvard to Colorado State University in Fort Collins in 1973 as one of the best things he ever did. Tired of the long commute between home and the museum, disillusioned with a new administration at the MCZ, and with good places for fieldwork diminishing in Massachusetts, the Evans family decided to move. As Howard put it, they decided not to give Harvard tenure.

Howard's unpublished autobiographical essay, "A Brief Review of Scientific Accomplishments," written when he turned 80 (in 1999), mentions that when the Evans family moved to Colorado State, he actually accepted a nontenured position! He had already published 170 papers and 6 books. Not surprisingly, he received tenure at CSU soon after he arrived. At Harvard, Howard had only one graduate student, Robert Matthews (now a professor at the University of Georgia). At CSU he served as advisor for several graduate students, including the late Byron Alexander (a CSU master's student who later studied with George Eickwort at Cornell), Darryl Gwynne, Mary Hathaway, Allan Hook, Rob Longair, Kevin O'Neill, and William Rubink. Three years after the move to Colorado, Howard was awarded the Daniel Giraud Elliot medal (in 1976) by the National Academy of Sciences, given for "recently published meritorious work in zoology." A year later he was elected to the National Academy of Sciences. Howard says in his essay that he had "no illusions about these awards" and once in a letter he told me that he thought of dropping out of the Academy, which he called "an elitist club" that wasn't "his cup of tea," but that he kept his membership in the hope of helping to elect other field biologists to the Academy. He did regard election to the Academy as an important recognition, however. With characteristic modesty he mentions in the unpublished "Autobiographical Notes" written for his family that it was "an indication that I have done reasonably well as a scientist." By that time, at the age of 80, he had described a total of 782 new species of insects, plus 31 new genera, and even a new family: the Scolebythidae, a family of wasps found in the Southern Hemisphere. Ten of the new genera were based on the study of fossils.

Evans was a pioneer in the use of behavioral data in systematics, and he proposed a number of important ideas that I would call "transition hypotheses" showing how complex behaviors such as nest building, social life, and specialized prey transport could have evolved from hypothesized ancestral states. Along with phylogenies and adaptive explanations in terms of natural selection, such hypotheses establishing the feasibility of particular phenotypic changes are an essential part of evolutionary explanations. Among his publications Howard was most proud not of the theoretical ones but of those packed with new data on natural history, such as his 1970 monograph on "Ecological-Behavioral Studies of the Wasps of Jackson Hole, Wyoming" (Evans, 1970). "I have always been especially proud of that paper," he wrote.

Some of Howard's ideas were far ahead of their time. He presented data on wasps that showed how behavior, including learning, could affect evolution, and he discussed the general importance of this especially in his 1966 book on sand wasps (see also Evans, 2002). Howard's ideas on how behavior can take the lead in evolution are now being cited more frequently than before, as evolutionary biologists are increasingly aware of how the condition sensitivity of organisms can supplement the genetic study of evolution. Howard saw that connection long ago, and because of it he wrote important critiques of overly gene-centered thinking, such as some analyses involving kin selection (e.g., see Evans, 1977). One paper he considered underappreciated described what he called dual sex-limited mimicry in South American spider wasps (Pompilidae), where he showed that in several species the males mimic social wasps, and females of the same species mimic tarantula hawks (*Pepsis*) (Evans, 1968b).

Howard Evans was never a powerful administrator or a biopolitician. He did not run a big lab bustling with technicians. He wasn't a brilliant orator, and he didn't hobnob with the rich and famous. Yet he was a leader among biologists and had a deep influence on those who knew him. He exerted a special kind of leadership in entomology because he stood for certain values in science and a certain kind of decency in human affairs. His way of promoting those values, aside from his personal interactions with the people around him and the quality of his scientific work, was to write clean, beautiful poetic prose that was at the same time lighthearted and earnest and deep. Three things stand out among the ideals that he promoted in both his scientific publications and his books and articles written for the general public. First, he stood for a love of nature, for the humble inhabitants of this planet, especially the insects, and he argued eloquently for their respect and preservation. Second, he stood for the value of curiosity-driven research, though it is worth mentioning that he never praised pure science at the expense of the applied, for which he had an equal respect. "Curiosity," he wrote, "may have 'killed the cat,' but it has nourished every good scientist" (Evans, 1985, p. 23). Third, he defended the importance of research on natural history. He was incensed when he read in a book review that "biology is a system that proceeds from biochemistry to the associated subjects of neurophysiology and genetics. All else . . . is stamp collecting" (Evans, 1963, p. 149). "If this is so," Howard wrote, "I can lay no claim to being a biologist . . . I find Darwin, Gray, and Fabre worth emulating in this twentieth century." But Howard never allowed himself to be preachy and pedantic for very long. In the middle of this tirade about stamp collecting he says, "And it seems unfair to call me a stamp collector when I can never remember what it costs to send a postcard." With Howard Evans, science was serious but it was never too serious; there was always room to lighten up.

I was lucky enough to be working with this supremely humane man when I came up against the two greatest crises in the life of a woman in science: the birth of my first baby and the offer to my husband of an attractive job in a place where there would of course be no formal job for me. A lesser advisor than Howard—or perhaps I should say one with a less strong-minded wife than Mary Alice—might have given up on me then and there. But Howard never withdrew his support, even when I began to work mostly at home, and most of my projects began to lag, including my chapters for a book we were writing together on wasps (Evans and West-Eberhard, 1970). On the contrary, he recommended both my husband, William Eberhard, and me for a fellowship at a summer research station, even though we would be going there directly from a maternity clinic; and he waited with seemingly endless patience for my chapters of our book. Howard always treated us with respect as a couple, reinforcing our own natural optimism that we would both keep going in science, never turning his back when, in the eyes of others, the signs probably did not look too good.

Howard Evans was legendary among entomologists for his athletic prowess with an insect net. One of his students, Allan Hook, remembers that once while collecting at night in Australia he and Evans were trying to catch some hawk moths that were zipping up and down a trail. Allan couldn't come close to catching one even though he considers himself especially fast with a net, but Howard managed to get one. When he thrust his hand into the net to extract the specimen, he exclaimed, "Hey, it's hairy!" The elusive specimens were bats, and Howard was quick enough to catch one.

After Howard retired from his position at Colorado State in 1986, the Evanses moved to a beautiful mountain home 35 miles from Fort Collins. At 7,800 feet it had a spectacular 50-mile view on all sides. The view did not completely distract Howard from writing, and he completed five books and many scientific articles after his retirement. He continued to do fieldwork, and taxonomic research on collections, throughout the rest of his life.

Howard Evans's approach to science and nature was most completely stated in his book *Life on a Little-Known Planet* (1968a), where he concluded (p. 293), "The earth is a good place to live. We shall appreciate it more and more as we explore the moon and the planets. If man shall ever have another home, it is presently unimaginable. We had better learn to respect the little-known planet beneath our feet."

Howard Evans departed this little-known planet on July 18, 2002, at the age of 83, leaving life here a little better known than it was before he arrived. I think it is fair to say that he was one of the finest entomologists of all time. Not only was he the leading authority on the systematics of a number of large groups of insects but he also published widely on insect behavior, larval morphology, and insect paleontology. His pioneer analyses of behavioral and biological data, published in the 1950s and 1960s, contributed to a major change in how systematics was done. Howard Evans was a shy and unsentimental man, a man who treasured fieldwork in remote and beautiful places far from people, and who often, in good humor, compared humans unfavorably with cockroaches and fleas. But at the conclusion of the autobiographical sketch he wrote in 1999 he refers to the "sterling people" he knew during his career in biology, and says that knowing these people had been "the greatest reward" of his professional life. He was a fine colleague and a warm friend who is not just missed but is also irreplaceable in the lives of those who depended on his mastery of broad areas of entomology and his eloquent enthusiasm for research in natural history. A complete list of his 265 scientific papers, 17 books, book reviews, and popular articles has been published elsewhere (West-Eberhard, 2004).

Parts of this essay were presented as a lecture in the symposium "Life on a Little-Known Planet: A Tribute to Howard Ensign Evans," sponsored by the Entomological Society of America, Cincinnati, Ohio, on October 26, 2003. This biography is abstracted from a longer version published in the *Journal of the Kansas Entomological Society* (West-Eberhard, 2004). Mary Alice Evans provided a copy of the unpublished scientific autobiography of Howard Evans written in 1999, an essay of personal reminiscences written for his family in 1986, and a complete list of his publications. Arnold Menke provided the frontispiece, dated 1968, from a collection of portraits of noted sphecidologists printed by the U.S. Department of Agriculture in 1974.

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W H Flygare

WILLIS H. FLYGARE

July 24, 1936–May 18, 1981

BY DAVID CHANDLER

MAY 18, 1981, WAS a rainy morning in Urbana, Illinois. My phone rang. It was Peter Beak. "Bill died," he said. "Bill" was Willis H. Flygare. He was a close friend to Peter Beak, a mentor to me, husband of Ruth, and father of Karna, John, Amy, and Sarah. He was 44 years old. He was a great physical chemist. He died of amyotrophic lateral sclerosis (ALS; in America often called Lou Gehrig's disease).

During the next two years the American Chemical Society held a memorial symposium honoring Bill Flygare, and both the *Journal of Chemical Physics* (vol. 78, no. 6) and the *Journal of Physical Chemistry* (vol. 87, no. 12), the primary physical chemistry publications of that time, produced large memorial issues honoring him. The issue of the *Journal of Chemical Physics* alone contained 117 original research articles written by scientists throughout the world, filling 965 journal pages covering every imaginable facet of research in physical chemistry. It was the largest single issue of its kind, an unprecedented outpouring of respect and gratitude reflecting the significance of Bill's life. He was a remarkably productive and influential scientist. He was charismatic. He had an enthusiasm for life and a sense of humor that were difficult to match. Above all, as I try to describe in the following pages, he was brave.

EARLY YEARS

Bill Flygare was born on July 24, 1936, to Willis B. and Doris H. Flygare in Jackson, Minnesota. Jackson is a county seat in southern Minnesota, about 8 miles from Sherburn, where Bill Flygare grew up and went to high school. Sherburn is a small farming town, with a population of little more than 1,000. Bill's parents were of Scandinavian descent like many residents of this area. Strong family ties, with frequent and large gatherings of relatives and friends, marked his childhood. Discussions focused on the dramatic seasonal variations of the area and their effect on the regional agro economy and on the politics of the day, which was dominated by World War II and its aftermath.

Bill was the older brother to Tom (eight years younger) and Nancy (nine years younger). Their father owned a clothing store. In contrast to Bill's usual ways of dressing in later years, the store concentrated on relatively formal men's attire and had the motto "Bill Flygare Suits Me." In addition to their house in town, the family had a summer cottage adjacent to a lake and a golf course outside of Sherburn. Bill spent many summer hours boating, fishing, and golfing. His teenage years coincided with the expanded opportunities of the relatively optimistic postwar period in the United States. Bill was a good student, particularly interested in mathematics, physics, and chemistry. He was introduced to the building and construction trade during the summer months. He began by working as a carpenter's apprentice, and by the time he graduated from high school, he had his own construction crew building houses in his community. Years later in Urbana, Illinois, Bill would build and remodel parts of his house, as well as rental properties in which he invested.

Bill was a fine athlete, a natural leader, and competitive. Ruth Swansson first noticed Bill Flygare as an athlete. She was a cheerleader for the neighboring Trimont High School. Bill led the Sherburn High School teams in football, basketball, and baseball against Trimont. He was gifted and handsome; she was very impressed. In later years, whether it was in pickup basketball games with Illinois students and faculty where he outclassed most, coaching his son's Little League team to win the Champaign-Urbana twin city championship, or smacking wild serves and forehands on a tennis court, he continued to play with the greatest possible enjoyment and wholesome competitive zeal. Describing Bill's approach to skiing down a mountain, Peter Beak would say, "It was best to arrive at the bottom in one piece, but it was not as important as being first."

HIGHER EDUCATION

Bill graduated from high school in June 1954. The subsequent fall he entered St. Olaf College in Northfield, Minnesota. He would later say of his first year in college that his chemistry, physics, and calculus courses were boring, but that he learned to read and write to his satisfaction. He was unusually facile in these skills, as I would often think when I worked with him in Illinois. By his third year of college his science and mathematics courses became interesting, and he began dating Ruth Swansson. Ruth had graduated from Trimont High School the previous spring, and she entered Gustavus Adolphus College that fall. St. Olaf and Gustavus colleges are within driving distance from one another, but Bill did not have access to an automobile. He would regularly hitchhike between the two schools to visit Ruth.

The St. Olaf College faculty was overwhelmed with Bill Flygare's enthusiasm for learning. One of his teachers, Al Finholt, would later describe Bill's insistence that chemistry majors needed to learn large amounts of mathematics, physics, and humanities. The faculty believed there was no realistic way to pack all that Bill wanted to learn into four years of courses. Bill nevertheless did absorb all this material, reading and learning by himself outside the regular program of courses. Indeed, self-reliance and confidence were invariants of Bill Flygare's personality, whether teaching himself science or fixing automobiles (his family car in Urbana was always an old one for that reason).

The knowledge and ways of thinking about nature that Bill acquired in his independent way were often unorthodox and underestimated by others. One of Bill's former students, Ben Ware, remarked that he initially confused Bill's unorthodox thinking for lack of extraordinary intelligence or insight. "Only after years of tolerating his special type of diversionary thinking," Ben wrote, "did I realize that that was actually [Flygare's] distinguishing genius."

At the start of Bill's fourth year of college in October 1957, the Soviet Union launched *Sputnik I.* This event was both exciting and frightening. The United States had yet to achieve such success. Many young Americans perceived a threat to their country, and it motivated them to pursue scientific careers. Bill was among those, and he applied to graduate school. Also during that fourth year, Bill obtained an automobile, and Ruth moved even closer to St. Olaf, to Minneapolis, where she was taking courses in nursing.

Bill graduated from St. Olaf in June 1958 with majors in chemistry, physics, and mathematics. Later that summer he and Ruth were married. They traveled west together, where Bill entered graduate school in chemistry at the University of California, Berkeley. There he blossomed. Bill took his Ph.D. in microwave spectroscopy under the direction of William D. Gwinn. He chose this area of research because it combined sophisticated experimental techniques with a strong theoretical component of molecular quantum mechanics.

Bill enjoyed both experiment and theory. He was brilliant with the former and fearless in his approach to both.

One hurdle for all Berkeley chemistry graduate students is an oral qualifying examination instituted long ago by Berkeley's founding chemist G. N. Lewis. In this examination students are required to explain and justify their research projects to a committee of faculty. The student's research advisor is not part of this committee. Through aggressive questioning the committee tries to determine the boundaries of the student's useful knowledge of physics and chemistry, and thereby decide whether the student is qualified to continue working toward a Ph.D. The examination takes place during the student's second year of graduate study. While many able students require two attempts before passing this daunting examination, Bill required only one, and he did more than just pass. The committee found it impossible to learn the extent of Bill's knowledge. There was simply nothing they could think of asking that he could not answer. The eminent Berkeley physical chemist Hal Johnston recalls Bill's performance to this day. No performance of any student he examined before or since matched it. "It was a joy," Hal says.

William Gwinn later wrote that Bill was additionally exceptional in his ability to work with others on a very high level and that he was particularly good in discussions of newly developing ideas. Such discussions were always friendly but far from detached or dispassionate. Indeed, Bill's participation in constructive scientific debate remained an integral part of the way he worked throughout his career. He would often change views and bias during a discussion. Original ideas would be greatly developed and expanded.

Bill spent long hours in the laboratory at Berkeley, "working like crazy," Ruth would say. But he would also take breaks to enjoy the city of San Francisco and the California countryside, hiking and skiing. These were wonderful times for Bill and Ruth. They often thought they would like to eventually settle in the San Francisco Bay area. Their apartment was in a comfortable old building on Francisco Street. Today the building no longer exists. It was removed to build what is now the North Berkeley station for the commuter train line known as BART. In the mornings they would often walk together up the hill to campus, where Ruth would then take a bus to her nursing job at the Oakland Children's Hospital. Ruth would sometimes audit courses at the university, including Edward Teller's lectures on physics for laymen.

Bill progressed quickly in his research, and by the start of his third year in graduate school, William Gwinn had written to Herbert Gutowsky at the University of Illinois, recommending Bill for a faculty position: "Flygare is a very pleasant person with whom to work. He is married and has a wife who will be a great asset to him. I believe he has very good prospects for a future in academic life and I recommend him to you with enthusiasm."

In January 1961 Bill interviewed at Illinois. The faculty was impressed and offered him a job during the visit. Ruth had accompanied Bill on the trip. The last day of their visit they were scheduled to depart by airplane. It was the bitter aftermath of a snowstorm, and the Ozark Airline DC-3 planes that serviced Urbana-Champaign were grounded. Herb Gutowsky drove them to the Illinois Central train station to catch a late afternoon train to Chicago. As Herb unloaded them and their luggage in the subzero (Fahrenheit!) weather, Herb and Bill discussed intensely the resources needed and the opportunities available in Urbana for his development as a scientist. Ruth could see that Bill was very happy and excited, but the wind-chill factor concerned her.

Bill accepted the offer from Illinois, to begin working as a chemistry instructor in September 1961. Ruth was pregnant with their first child. Karna was due in August but kept everyone waiting until September 13. Two days later Bill packed up their car and the attached U-Haul trailer and drove east with their pet cat on his shoulder. He made the drive from Berkeley to Urbana in less than three days, arriving just in time to teach his first class. Two weeks later Ruth and Karna arrived in Urbana. Bill met them at the airport and drove them to their first house on Bliss Drive, a University of Illinois rental property for new faculty. Another new chemistry instructor and his wife, Peter and Sandy Beak, had recently moved into a similar house two doors down. The Flygare's cat liked to sit on the Beaks' automobile. That is how the Beaks and Flygares got to know each other. It was a deep and lasting friendship, bringing Peter Beak to write many years after Bill's death, "I still miss him."

ILLINOIS, EARLY YEARS

Bill Flygare would spend the next two decades, his entire career, at the University of Illinois. It was an extraordinary time at Illinois. Harry Drickamer, Herb Gutowsky, Jiri Jonas, Rudy Marcus, and others were among its stellar faculty. During that period, it was arguably the best physical chemistry program in the world. While Marcus is the only one to have actually won a Nobel Prize (for his theory of electron transfer reactions), Gutowsky (who developed the first ways to fingerprint molecules with nuclear magnetic resonance) and Drickamer (who pioneered high-pressure chemical physics) were equally deserving.

Bill quickly showed he was rightfully part of this heady group. Within five years of his arrival at Illinois, he was a full professor, and within six, MIT and the University of Chicago had courted him. Turning them down, he wrote, "The primary consideration is that I have a very large personal investment in the future at Illinois." Within 13 years of his arrival, in 1974, he was elected to the National Academy of Sciences. More than 30 graduate students would eventually receive their Ph.D.s under his direction. He wrote the advanced physical chemistry textbook *Molecular Structure and Dynamics*, and he coauthored more than 200 research papers, the last 25 were written during the last two years of his life while resisting the effects of a ravaging illness, about which I have more to say later.

As the numbers indicate, his students worked very hard. Bill demanded as much. He wrote, "New students are expected to . . . do new and interesting experiments. The experience gained in designing and constructing complex metal, glass, optical, electronic, high vacuum, and high pressure apparatus is vital to the student's future as a viable scientist. . . . A close union of theory and experiment is [to be] attempted."

Many students responded positively. One of those, Ben Ware, later explained: "I knew that science was supposed to be exciting and here was a man who carried that excitement with him and conveyed it to all who would listen. For me, and for many others of his students, he was a scientific pied piper whom we would have followed anywhere." Another, John Pochan, recalls Bill's love of athletic competition, and adds that Bill "enjoyed the scientific competition just as much." On one occasion, Pochan remembers, Bill and his students learned of another group closing in on a research problem that they too were trying to solve. Bill worked with his students for three days straight, day and night, finishing the project ahead of the competing group. "He expected a lot and he gave a lot," Pochan says.

For some students, however, and for some colleagues as well, Bill was hard to take. As with all people, his strength could also be a weakness. He was often impatient with those who could not keep up with his train of thought, and he was intolerant of those he felt did not work hard enough. Since his thoughts were quick, often unorthodox, and sometimes not quite right, his impatience could be inappropriate. While many found it stimulating to talk science with him—which invariably meant formulating new ideas, arguing, reformulating or refining, arguing again, and so forth—others found the exercise overwhelming. A few would go away insulted. A famous physical chemist once wrote asking a question concerning one of Bill's long and detailed papers on the molecular Zeeman effect. Bill felt that the answer to the question was clearly spelled out in the paper and that the scientist was lazy and possibly insulting not seeing as much. He wrote back answering the question and adding gratuitously "with friends like you, who needs enemies."

By the fall of 1966 the Flygare research group had eight graduate students and four postdoctoral assistants. It would remain close to this size for the next 15 years. Experiments were being done on high-resolution microwave spectroscopy, microwave-microwave double resonance and rotational relaxation, and matrix isolation infrared spectroscopy. They were able to routinely measure the small splittings of rotational transitions caused by spin-rotation interactions, and in this way they studied many systems of chemical interest. Before then others had been able to observe these splittings only in a few special cases. The Flygare group's observations were particularly influential because Bill's theoretical work established their relationship to molecular electronic structure and the nuclear magnetic shielding of a molecule.

Leading this large group of research students in such a varied range of projects required special skill, all the more remarkable as Bill had just turned 30 the previous summer. Rick Shoemaker, who joined the group during that time, recalls that Bill would usually suggest possible research projects, but then encourage the students to work on their own. He would regularly look to see that his students were making good progress, usually every day, and take an active role when students were struggling. He had the more advanced students instruct the newer students on how to operate and maintain equipment, and he encouraged capable students to pursue well-designed projects of their own making. "He came across more as a wise older brother than a research director or manager," Rick says.

At the 1967 American Chemical Society meeting Bill reported the measurement of a molecular magnetic susceptibility anisotropy by microwave spectroscopy. These results on formaldehyde, published in 1968, were made possible by Bill's exploitation of the molecular Zeeman effect. In a previous theoretical paper he had analyzed the principles of the molecular Zeeman effect and established that molecular quadrupole moments could be measured directly by using both the linear and quadratic field Zeeman effect. With this and related connections his group's subsequent measurements of the molecular Zeeman effect determined molecular *g*-values, magnetic susceptibility tensor elements, molecular quadrupole moments, second moments of electronic charge distributions, and in some cases the signs of the electric dipole moments for about 90 molecules. In 1969 alone Bill and his students published more than 20 papers exploiting the molecular Zeeman effect.

This body of work attracted wide attention, and it catapulted Bill into the National Academy of Sciences. The results of these experiments seemed important at the time. It was believed that knowledge of molecular dipole and quadrupole moments would significantly contribute to a good understanding of intermolecular forces. In current times, however, it is understood that intermolecular forces and their manifestations, especially in condensed phases, are more complicated than those numbers reveal. Further, experiment is no longer required for these quantities because theoretical quantum chemistry can now provide the information easily and reliably. In retrospect, therefore, the results of Bill's Zeeman effect measurements seem less important than the training provided to the students who helped make the measurements. A postdoctoral student with Bill during that time, J. J. Ewing, says that "a key aspect of the experience was that working with Bill on spectroscopy helped prepare people for a diversity of careers that took many of us a long way from . . . [traditional] chemical physics." Indeed, the diversity includes optics, uranium enrichment, liquid crystal and polymer physics, laser development, as well as other fields. Recalling his own professional development in the field of eximer lasers, Ewing says the connection to high-resolution spectroscopy appears weak, but "the 'close union of theory and experiment' that Bill pitched was always in my mind."

ILLINOIS, MIDDLE YEARS

I first met Bill Flygare in late January 1970 during my interview trip to Illinois. Bill was the chair of the recruiting committee, so it was to his office that I went when I first arrived. With a hint of the belly laugh and sense of humor I later knew well, he greeted me and immediately entered into a conversation about my scientific interests. He was much younger than one would expect of someone so accomplished and well known, and he was more casually dressed than the standard professorial attire of the time. Despite the youthful and casual appearance, he was intense! No one had ever before seemed so focused on what I was talking about. I was excited when he described his own work and showed me his laboratories that were filled with seemingly every kind of instrument plus the huge magnet required for his observations of molecular Zeeman splittings. At the time, Bill was finishing his work on the Zeeman effect and was developing a new interest in dynamic light scattering. When I returned to La Jolla, where I was doing a postdoctoral year, I told my wife, Elaine, about this amazing man, and she knew we would be moving to Urbana.

Bill was introduced to light scattering by his student Ben Ware. Ben's first research advisor had recently left Urbana to take a position at the University of Minnesota. Ben decided to remain in Urbana and work with Bill, provided Bill would be willing to study some type of biophysical problem. Since Ben was a fine student and Bill feared nothing new, he agreed to the conditions and in the spring of 1970 he purchased the makings of a new light scattering laboratory. That summer, while Ben was assembling the laboratory, Bill took off to La Jolla, California, to work for a month with the Materials Research Council, a group of about 20 scientists that advised the Materials Science Office of the Advanced Research Projects Agency (ARPA).

The U.S. Department of Defense created ARPA (now DARPA) during the late 1950s. The Materials Research Council still exists but now with the name Defense Sciences Research Council. The mission of the Council, together with a second ARPA group known as JASON, is to bring the most current ideas and outstanding advice on science and technology to the U.S. military. The JASONs focus on classified issues, while members of the Council do not. The Council, when Bill worked on it, included such notables as Nicolaas Bloembergen, Walter Kohn, and Robert Schrieffer. They would assemble each summer and work together to tackle technical problems of timely concern to the military. Bill was part of this group during the late 1960s and the 1970s, and he served as the chair of its Steering Committee during the mid-1970s.

At some point during the workshop that summer in 1970, Bill heard a lecture on blood and blood plasma and how components are typically analyzed using the technique of electrophoresis. Ben Ware recalls that "[Bill] got the idea that we could effectively do electrophoresis spectroscopically using quasi-elastic light scattering. He did a few calculations and called me. I was having trouble getting the equipment together to do any simple experiments, and it annoyed me that he was off on an idea that was beyond anything we could do. . . . When I told him that we should do the standard experiments before moving on, he said, 'Ben, this is NEW!' with such passion that I knew we were on this new track."

Bill had carried out a set of calculations to derive how light scattering coupled with electrophoresis would permit high-resolution multicomponent analysis of blood plasma. Specifically, because of their different net charges, different components would have different Doppler shifts in the presence of an electric field. Bill's calculations suggested that the broad spectrum of Doppler lines due to diffusion would not obscure the distinctive electrophoretic shifts of the various components. Unfortunately, Bill had made some mistakes with dimensional constants and factors of π. When Ben corrected the mistakes, it seemed that the electrophoretic shifts were only a small fraction of the diffusion widths. "I hoped that this would dissuade him. It did not," Ben says.

In the fall of 1970 Bill and Ben were pursuing this area of research. The results of their efforts would become known as "electrophoretic light scattering." This topic and also Bill's simultaneous interest in astrophysical observations occupied many lunchtime conversations. Along with light scattering Bill's group was following up on their high-resolution microwave work with radio astronomy. I had just started my career in Urbana as an assistant professor, and I was yet to have a research group of my own. I would tag along and listen to Bill arguing with and cajoling his students to reach for what less confident people might think impossible. In part, this motivation spurred Ben Ware to consider a new design for his light-scattering cell and improved signal-processing equipment.

While exploring various technological ideas for light scattering throughout the 1970-1971 winter, a crucial piece of theoretical insight was still missing. The break came when Ben considered that electrophoretic Doppler shifts increase relative to diffusion widths as the light-scattering vector is decreased. With a newly designed sample cell he realized he would be able to resolve the Doppler shifts by doing experiments at a very low scattering angle. That spring Ben observed the first measurable electrophoretic shift and verified that it was properly proportional to field strength and to scattering vector. This success attracted significant attention and led to Ben acquiring an assistant professorship at Harvard. They patented the technique, though it is now overshadowed by new gel electrophoresis techniques.

After Ben's graduation, Bill remained interested in light scattering, turning his attention to using the technique to probe orientational correlations between small molecules in liquids. His experiments on this topic proved to be complementary to ideas I was developing on the theory of molecular liquids. But in the midst of figuring it all out, neither of us recognized the complementarities. We argued about the science quite a bit, often heatedly, even in public. On at least one such occasion, we shocked our students and a few of our colleagues. Early on I would worry about whether Bill appreciated my ideas. But as I gained confidence I understood that the intensity of these interactions reflected our personalities rather than our ideas. The interactions helped me learn to use scientific debate constructively to develop and persuade others of my ideas. Bill's last paper on the topic of intermolecular correlations in liquids is unequivocal about his agreement with my theoretical work.

During this period, in the mid-1970s, Bill turned his attention back to high-resolution microwave spectroscopy, now in the time domain. He was devising techniques that resembled the free induction decay and pulse methods exploited with nuclear magnetic resonance spectroscopy. He was thinking and working with concepts like quantum coherence and dephasing in his characteristically unconventional ways. A young physical chemist interviewing for a faculty position in Urbana at that time talked to Bill about these things. Bill was unimpressed with the candidate's understanding. The candidate did not get a job offer from the University of Illinois, but he did get one from the California Institute of Technology. Some 20 years later this Caltech chemist was awarded a Nobel Prize for work done with time-resolved spectroscopy.

ILLINOIS, FINAL YEARS

By the summer of 1977 Bill's efforts with time-domain microwave spectroscopy seemed to be nearing fruition. His student Bill Hoke had nearly finished the work that he would submit the next spring as his Ph.D. thesis, "Transient Rotational Relaxation Studies and Fourier Transform Microwave Spectroscopy." The techniques described there were important elements in a new and powerful spectroscopy that Bill and his students Terry Balle, Ed Campbell, and Mike Keenan would invent during the next 18 months. Also by that summer the first signs of disease appeared that would make this invention Bill's last contribution to science. Weakness in his right hand plus annoying sensations in his right wrist and arm caused him to seek medical advice. In late September he was told that he was suffering from ALS, and that he would die from this disease, most likely within two years; Bill would actually survive for nearly four more years.

After learning of his illness, Bill kept a diary from late September through mid-October 1977, which he used to collect his thoughts and help plan for the short future left to him. No one but Bill knew of the diary until after his death. Ruth found it while cleaning out his office. Bill was most concerned about the effect of his illness on Ruth and their children. The diary is laced with the hope that his condition was misdiagnosed. By the end of these weeks, however, with second opinions solicited from doctors across the country, the truth was unambiguous. His right hand and arm were far gone, and damage could already be detected in his other limbs. Eventually the disease would make it impossible for him to talk, and finally lead to suffocation.

Later that fall, Illinois's senior theorist Rudy Marcus was offered an appointment at Caltech, and it soon became clear that he would accept the offer. Bill saw his own failing health together with Marcus's imminent move as a significant blow to the physical chemistry program at Illinois. He convinced Harry Drickamer and Herb Gutowsky that a recruiting effort was necessary. Drickamer and Gutowsky were the only physical chemistry colleagues who knew of Bill's condition at the time. In the late 1970s at Illinois, support from Drickamer and Gutowsky was sufficient to proceed. Offers were tendered in the spring of 1978 to the theorist Robert Zwanzig and to the experimentalist George Flynn. Both recruiting efforts failed. The subsequent year furtive plans to attract the British experimentalist Alan Carrington ended quickly when Carrington expressed no interest in leaving England. Attempts aimed at Bill Miller and John Tully proved similarly unsuccessful. Finally, the theorist Peter Wolynes, then an assistant professor at Harvard, accepted an offer to join the Illinois faculty. This move strengthened the theory program at Illinois for a few years, but no similar quality senior appointment was made in experimental chemical physics.

Despite the difficulties in recruiting and despite the ominous diagnosis, Bill was determined to make the most of what remained. He published his book *Molecular Structure and Dynamics*, which is based upon the course that he created to teach entering graduate students a working knowledge of quantum mechanics and spectroscopy. In the late winter of 1978 he began to anticipate how his group's developments of time-domain microwave spectroscopy might soon lead to the possibility of determining structures of transient or weakly bound molecular species. He and his students imagined studies of van der Waals complexes, such as argon clustering with hydrogen halides. The techniques that would make these studies possible were not yet completely developed. But forever confident, Bill began to make plans for comprehensive studies of many systems, an onslaught on nature not unlike those he had carried out in the 1960s. Unlike that earlier decade, however, Bill would now have neither sufficient energy nor remaining life to properly lead the effort. His group would need another senior scientist. Bill spent considerable time in the fall of 1978 thinking about whom that person might be. He made a decision, and then planned a trip to England, where he would introduce himself to the prospect.

Anthony ("Tony") Legon remembers that he first met Bill on a snowy day in January 1979. Tony was then a young faculty member at University College London, collaborating with D. J. ("Jim") Millen. Tony and Jim had succeeded at observing and analyzing the rotational spectra of a few hydrogen-bonded complexes, not unlike the systems that Bill hoped to study. Bill turned up at their laboratory and asked Tony whether he was due any sabbatical leave and whether he would like to spend some time in Urbana. Tony answered yes to both questions. They arranged for Tony to arrive in Illinois at the start of the 1979-1980 academic year. The actual arrival date was four months later because Tony's wife gave birth to their son, Anthony, in November 1979.

Between Bill's visit to London and Tony's arrival in Urbana, Bill had phoned Tony with "typical Flygare enthusiasm," Tony recalls. Bill was reporting to Tony the first working of a pulsed-nozzle Fourier-transform microwave spectrometer. Terry Balle had been able to synchronize a microwave $\pi/2$ pulse with gas pulsed into a Fabry-Perot cavity. This synchronization was the crucial final element required for this new spectroscopy. It was May 18, 1979, exactly two years to the day before Bill would die. The following day Balle worked with another Flygare student, Ed Campbell, and used the new spectrometer to detect strong rotational transitions of the hydrogen bound Ar-HCl complex. Bill called Tony Legon with this news. "He could see immediately all the wide range of possibilities for the technique and could not contain himself," Tony says.

Bill and his students quickly wrote the first report of this technique. It was published as a communication in the September 15, 1979, issue of the *Journal of Chemical Physics*. Simultaneously Bill contacted the National Science Foundation and arranged for a creativity extension of his current funding that provided an additional $135,000 per year for the next two years. A series of studies were then carried out on hydrogen halide molecules bound to rare gas atoms. The impressive spectra and structural analysis of these van der Waals molecules formed the basis for research lectures that Bill would give that fall. One of these was given at Columbia University in New York City. While listening to his brilliant presentation, Columbia faculty could see that Bill was not well, and Bill sensed as much. The next morning he was scheduled for meetings at Columbia, but instead he simply returned home to Urbana. George Flynn was among those that Bill was supposed to meet with. George phoned me in Urbana to express his concern. It was no longer possible to hide the sad truth that Bill was dying.

On Tony's arrival in January 1980 the Flygare group moved quickly with his help to apply their new spectrometer to systems with chemically interesting interactions: first CO complexes with HCl, HBr, HF, and next ethene-HX, ethyne-HX, cyclopropane-HX. Bill's physical condition was rapidly deteriorating, and he could no longer do the experimental work. He was nevertheless an important presence in the laboratory. Tony remembers: "[Bill] was quite ill. He was sitting with me while I was working at the spectrometer. He said: 'Aren't you lucky. You are the first person in the Universe to see that molecule.' His enthusiasm was unattenuated by his condition." The program of measurements they had mapped out seemed too voluminous to conquer in a reasonable period of time, especially since there was only one of these remarkable spectrometers. The students decided to work in shifts, exploiting this piece of equipment 24 hours a day.

These were not the first studies of van der Waals molecules, but they were the most far reaching and comprehensive. A competing group at Harvard using different techniques soon appreciated that they could not match the ease and speed with which the "Balle-Flygare" spectrometer probed these systems. Considering that work today, more than 20 years later, Berkeley spectroscopist Richard Saykally writes, "Bill Flygare's greatest contribution was the development of the Balle-Flygare Fourier transform microwave spectrometer, which affected nothing less than a total revolution in the field of microwave spectroscopy. The sensitivity, simplicity and generality of this design permit a wide variety of applications. . . . Perhaps a hundred of these instruments are currently in operation around the world, and are used for the study of molecular species ranging from extremely weakly-bound clusters like KrNe to novel inorganic donor-acceptor complexes."

Results were pouring out, but Bill was no longer able to hold a pen. A year earlier he had switched to writing with his left hand, but now even his left side was incapacitated. In addition, his speech was becoming inaudible, so dictation was becoming problematic. Art Gaylord provided a tool that would enable Bill to continue communicating. Art had recently joined the Illinois technical support staff after obtaining a Berkeley Ph.D. as William Gwinn's last graduate student. He fashioned one of the original Apple II computers so that Bill could type by simply dropping his hands onto the keyboard. A ball would pass along a menu on the screen, and striking the keyboard instructed the computer to branch to the item adjacent to the ball. The alphabet was one such item, and a touch of the keyboard at that point instructed the printer to type the letter adjacent to the ball. For the next several months Bill would write in this way, one stroke at a time.

The group's results generated significant excitement throughout the chemical physics community. In November 1980 Bill received notification that he had been selected as the next recipient of the Irving Langmuir Award from the American Physical Society. A group of us assembled that afternoon to congratulate Bill for being honored by this most prestigious recognition in chemical physics. He came to the party only briefly, as he was overwhelmed with emotion. It was the only time we observed Bill upset by his illness. Tony Legon recalls: "The [Langmuir] Award provided a focus for all those conflicting thoughts that he must have had at that time and especially because he knew he was a great scientist whose life was about to be cut short at its zenith."

During the next six months Bill's group would continue its productive march. Tony returned to England in January 1981, but the people working to carry on had now swollen to include seven new students along with those that had been there at the start of the project: Terry Balle, Ed Campbell, and Mike Keenan. Through the weekend of May 16 and 17 Bill remained scientifically active and in touch with his group, but he was having difficulty breathing and his lungs were beginning to fill with fluid. Bill passed away early that rainy Monday morning, at home, in Ruth's arms. A memorial service was held two days later. Referring to Bill in a written eulogy, Harry Drickamer would quote from Shakespeare's Hamlet, "He was a man, take him all in all. We shall not look upon his like again." The next winter the Flygare family scattered Bill's ashes across the mountainside above Aspen, Colorado, where Bill had so often loved to ski.

PROLOGUE AND ACKNOWLEDGMENTS

Bill's life ended more than two decades ago, but his influence continues. Writing this memoir has given me the opportunity to learn many heartfelt things about this influence. His children have all become interesting and productive adults with children of their own. Ruth remains lovely and healthy. She has remarried, to Vern Halberstadt. Vern was a widower. Together they melded a family of six children and ten grandchildren. Much of what is described in this memoir I learned from conversations with Ruth and the materials she provided. Bill's greatest concerns were for the health and future of his wife and children. It seems clear to me that Bill succeeded at building a foundation for their lives that served them well.

Peter Beak remains an active member of the Illinois Chemistry Department. Ben Ware is currently vice-president for research at Syracuse University. John Pochan is a materials scientist with Kodak. Rick Shoemaker is a professor at the Optical Sciences Center of the University of Arizona. J. J. Ewing lives in Seattle, where he is president of Ewing Technology Associates. Tony Legon now lives in Exeter, England, where he is a professor and fellow of the Royal Society. Each of these people has generously written to me describing their work with Bill and the impact his energy and enthusiasm had on their lives. Several others have provided helpful information that I have used in this memoir, including David Buckingham, Henry Ehrenreich, John Flygare, Richard Saykally, and Jeremiah Sullivan.

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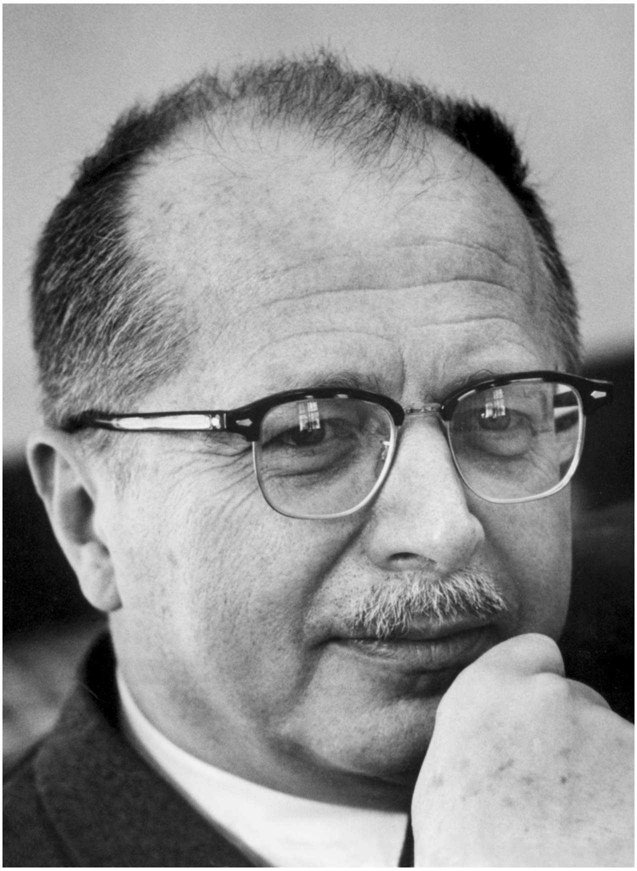

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JESSE LEONARD GREENSTEIN

October 15, 1909–October 21, 2002

BY ROBERT P. KRAFT

JESSE L. GREENSTEIN WAS born and raised in New York City. He received advanced degrees from Harvard University just before and during the Great Depression. As a graduate student he made pioneering advances in understanding the influence of interstellar dust on the colors and magnitudes of stars. His studies of the nature of interstellar dust continued as he moved westward in the post-Harvard years, first to Yerkes Observatory and later Caltech, where he developed the analysis tools and founded a program of study devoted to the determination of the chemical abundances in stars, a field in which he became the world's observational leader. His work on a wide array of problems ranging from the properties of QSOs (quasi-stellar objects), the nature of interstellar grains, the evolution of the chemical composition of stars, to the physical properties of white dwarfs provides the currently available fundamental knowledge of each of these fields. He was a leader of U.S. astronomy, and his advice, heeded by both universities and government agencies, shaped the present organization of astronomy as it is conducted in the United States.

THE EARLY YEARS

Jesse was born in New York City in the year of Halley's comet. His grandfather Samuel Greenstein had emigrated to America in 1888, leaving behind in Bialystok (now in Poland, but then part of Russia) his wife and two small children. He prospered in the manufacture of living-room furniture and brought the family to New York when Jesse's father, Maurice Greenstein, was two years old. After his grandfather's death, Maurice, the oldest of nine children, assumed leadership of the business. As had his father before him, Maurice bought real estate in Brooklyn, as well as Manhattan, and he and the other members of the family eventually became quite prosperous. In 1908 Maurice married Leah Feingold, who had been working as a stenographer in the furniture factory founded by his father. Jesse was born the following year.

Jesse often described his upbringing in the folds of a typical upper-middle-class Jewish family of the period as "indulgent." In an unpublished memoir he recounts his early interest in both science and the arts. When Jesse was eight years old, his grandfather gave him a small brass telescope with which he regaled his friends with the wonders of the heavens. The contemporary semipopular books on astronomy were available to him, thanks to his grandfather's excellent private library. In his grandfather's basement laboratory Jesse set up a Gaertner prism spectroscope, electric arc, rotary spark gap, and a rectifier from which he tried to identify, with some success, the spectral lines of chemical elements, using Kayser's *Handbook of Spectroscopy* as a resource. It was a harbinger of Jesse's main research interest after his move to Caltech 30 years later.

At the age of 11 Jesse was enrolled in a private high school, the Horace Mann School for Boys, where he learned

Latin as well as the "virtues," as he put it, of "hard work." Chemistry fascinated him, but the classical physics curriculum of statics and levers he found of little interest.

HARVARD UNDERGRADUATE YEARS, THE GREAT DEPRESSION

Jesse entered the Harvard astronomy program at the tender age of 15. In the mid-1920s the program still retained its classical roots, with emphases on transit circles, navigation, and celestial mechanics, and it was not until 1928-1929 that Jesse came into contact with faculty members of the Harvard College Observatory, an administratively separate entity. There he learned of the new developments in astrophysics and galactic structure under the influence of such figures as the young Canadian theorist Harry Plaskett; Donald Menzel, whose interests were in solar physics, gaseous nebulae, and atomic processes; and especially Cecilia Payne, an English astronomer whose thesis "Stellar Atmospheres" Jesse described as "one of the great theses in astronomy." His undergraduate education was diffuse, but included a course in the philosophy of science and an introduction to what one of his professors termed the new fad of quantum mechanics.

On graduating in 1929 Jesse was offered a postgraduate appointment at Oxford by E. A. Milne, a British theoretician whom he had met during Milne's visit to Harvard. But a bout with a throat infection prevented Jesse from accepting the appointment, and he remained at Harvard, taking a master's degree in 1930. As Jesse himself has noted, loss of this opportunity was "just as well"; his interests throughout his subsequent career lay not so much in developing the computational skills of a serious theoretician as in understanding the basic physics underlying astronomical phenomena.

The period just before and immediately after the October 1929 stock market crash was for Jesse, the scion of a well-to-do family, a time not only of serious astronomical study but also a time for indulgence in modern art, fashionably avant-garde literature, and adventures in the theatre, not only in the Boston area but also in New York itself. All this came to an abrupt end in 1930 as Jesse left Harvard for home, M.A. in hand, to help rescue the failing fortunes of the family business. It was time to come to grips with reality, to deal with financial losses, to learn how to manage difficult social relationships emerging from the Depression. But fate intervened on the side of science; at a dinner party Jesse met the outstanding Columbia physicist I. I. Rabi, who encouraged him to leave the real estate business and re-embark on his scientific career. Rabi introduced Jesse to Jan Schildt, a Columbia professor who had returned from Mt. Wilson in 1926 with several direct photographs of the globular cluster M3, and he made this material available to Jesse as a "volunteer." With his practiced Harvard eye Jesse measured magnitudes and derived periods for M3 RR Lyraes. Although the work was monotonous, it convinced Jesse that his real love was astronomy, not the attempt to make money in the real estate business, even though by 1934 he had already made a very lucrative real estate deal.

Jesse, now confident of his future course, returned to Harvard graduate school, despite the warning of Harlow Shapley, the Harvard College Observatory director, that astronomy had changed too much for Jesse to catch up. In the meantime Jesse had married Naomi Kitay on a snowy day in January at her home in New Jersey. Jesse had met Naomi as early as 1926. A young woman of wide interests in literature, the arts, and especially theatre, Naomi was fluent in French and had traveled widely in Europe. She had graduated from the Horace Mann School for Girls, and from Mt. Holyoke College in 1933. The author remembers Naomi as the charming hostess of soirees in the Pasadena home of the Greensteins; Jesse referred to her in conversation as “Kitty,” obviously a term of endearment derived from her family name.

RETURN TO GRADUATE WORK AT HARVARD

On returning to Harvard College Observatory (HCO) Jesse found a much-changed atmosphere, with graduate study being dominated by a group working on the physics of ionized gases under Menzel and another under Bart Bok counting stars as a technique for studying galactic structure. Completing his course work in a scant two and one-half years, Jesse returned for his Ph.D. thesis to a problem that had vexed him as an undergraduate: the influence of interstellar dust on the colors and magnitudes of early type (B) stars. Thanks to the work of R. J. Trumpler at Lick, it was by 1934 pretty well accepted that interstellar space was filled with clouds of dust that not only dimmed but also reddened the colors of stars. On the basis of low-resolution objective prism spectra obtained with the resident Harvard 24-inch telescope, Jesse measured photographically the energy distributions of little and greatly reddened B-type stars (difficult to do accurately with the techniques available in the 1930s), and showed not only that absorption by dust followed a $\lambda^{-0.7}$ law but also that the slope was essentially independent of the choice of interstellar cloud (i.e., the law appeared to be universal). In a difficult series of calculations based on Mie theory, Jesse examined the absorption produced by small grains of water ice, silicates, and metals, showing that for appropriate choices of composition and grain size distribution, the observed law, so different from the Rayleigh scattering of the earth’s atmosphere, might be reproduced. These pioneering observational results were confirmed later by more accurate photoelectric techniques (of J. Stebbins and A. Whitford), and the calculations provided a basis for a more definitive treatment (by C. Schalen) along the same lines.

With Fred Whipple, then an instructor at HCO, as a collaborator, Jesse noted K. Jansky's startling discovery of cosmic radio noise from the direction of the galactic nuclear bulge, and the two of them attempted to explain the phenomenon as the result of thermal emission from the heavily obscuring dust clouds in and around the galactic center. Although the explanation failed to give the correct answer by several orders of magnitude, the attempt highlights salient features of Jesse's scientific outlook. One was his intellectual curiosity. Few astronomers of the time took an interest in Jansky's discovery (although, to be fair, it had been published in an engineering journal not generally read in the astronomical community), but Jesse and Fred Whipple found it exciting and tried to explain it using the best theoretical notions of the time. In addition, Jesse's recognition of the new world opened up by this discovery was the spark that ignited his support of radio astronomy at Caltech some 20 years later.

TRANSFER TO YERKES (AND MCDONALD), WORLD WAR II YEARS

On graduation Jesse accepted a National Research Council Fellowship for 1937-1939, a rare opportunity in those days, and chose Yerkes Observatory of the University of Chicago as host institution. As Yerkes was located in a tiny Wisconsin town surrounded by farmland, one might wonder why a city boy would choose such a place, but Yerkes was then entering an intellectual golden era. First, in contrast with the almost purely observational orientation of Lick and Mt. Wilson, Yerkes was an observatory that welcomed theoretical astrophysicists both as staff members (e.g., S. Chandrasekhar) and as visitors (e.g., A. Unsöld and K. Wurm). Second, the organization was undergoing staff expansion, preparing for the inauguration of the 82-inch telescope of the McDonald Observatory, located in the superior climate of the Davis

Mountains of west Texas. Finally, Yerkes was moving forward under the dynamic leadership of Otto Struve, an observational astronomer well versed in the new astrophysical spectroscopy, a man whom Jesse had greatly admired when Struve visited Harvard during Jesse's years as a graduate student. It was the kind of intellectual atmosphere that appealed to the young Jesse Greenstein. Then, too, the appointment might provide an opportunity to mount an observational program with the new facilities at McDonald.

In addition to Struve and Chandrasekhar, Jesse met such important figures as Gerard Kuiper, Bengt Strömgren, and W. W. Morgan. He soon developed a friendship and scientific collaboration with Louis Henyey, a recent Ph.D. from Yerkes, a theoretician of formidable mathematical skill, whose interests in gaseous nebulae and interstellar matter paralleled his own. Henyey's Ph.D. thesis had described the physics of interstellar dust in a way that complemented Jesse's own Harvard thesis. Stimulated by a late-night brainstorming session with Struve, the two young researchers built a Yerkes nebular spectrograph of unusual fast design, which—jerry-built for the 40-inch refractor—nevertheless yielded spectacular spectra of the earth's aurora. An improved version of the design was transferred later to a mountainside at the McDonald site and used subsequently by Struve and Strömgren to discover what Strömgren later called "H II regions." Meanwhile, Greenstein and Henyey employed a novel Fabry photometer on the 40-inch and discovered diffuse galactic light, the scattering of light from stars produced by dust clouds in the Milky Way, along lines of sight far from the stars themselves. Although the clouds are essentially opaque in transmitted light, the starlight scattered by them makes the clouds appear bright, indeed, bright enough to require the dust to be more luminous than snow! According to Jesse's thesis the dust was therefore a dielectric having low true absorption, either ice or a silicate, a result that has held up well, although it is not the entire story.

In 1939 at the termination of his fellowship Jesse was appointed an instructor, which allowed him to remain at Yerkes after the dedication of the 82-inch reflector, then the second-largest telescope in the world. Jesse's research took a new turn, partly as a result of Struve's influence and partly as a result of the availability of the brand-new coude spectrograph at the 82-inch reflector. After assisting Struve in obtaining spectra of the standard hot B-type star tau Scorpii for analysis by Unsöld's stellar atmospheres group in Kiel, Jesse went on to obtain similar spectra of upsilon Sagittarii, a star having an unusually peculiar spectrum, which after analysis by Jesse, proved to have a hydrogen-poor, helium-rich atmosphere. A parallel analysis of Canopus, a southern supergiant, yielded normal (i.e., solar-like) abundance ratios of the chemical elements. It is easy to forget now, in our present era of excellent model atmospheres and vast computing power, just how primitive abundance studies were in the late 1930s. Jesse developed the practical side of the so-called method of "grobanalyse," or curves of growth, that had been pioneered by Unsöld and Struve. Transition probabilities came from theory and/or solar line strengths, something of a grab bag, even though the source of solar continuous opacity, H-, had by then been identified. The fact that upsilon Sagittarii did not conform to the expectation that stellar abundance ratios were universal was a major breakthrough in what would become, in the years following World War II, the idea of galactic chemical evolution. The analysis of upsilon Sagittarii thus profoundly influenced the direction that Jesse's research would take in his later years.

Even so, Harvard-influenced projects had not entirely left Jesse's repertoire. The year 1939 saw publication of a color-magnitude array of the relatively nearby globular cluster M4, based on photographic photometry of Harvard plates. The array, with its strange configuration of giants and horizontal branch stars, so different from that of the so-called "open" clusters of the Milky Way, was one of unusually high accuracy and was for the time a particularly fine illustration of those differences. It was yet a decade in the future before Walter Baade would generalize the differences into his two-population concept, and 15 years before the differences were linked to the effects of mass, age, and composition on stellar evolution. Although Jesse never returned to the construction of cluster color-magnitude arrays, his future research would contribute mightily to understanding the chemical composition and age differences between stars in globular and open clusters.

With the advent of U.S. participation in World War II, Struve struggled to keep Yerkes and McDonald telescopes in operation as staff scattered into defense industries and those who remained at Yerkes, Jesse among others, also moved into war work. Consequently Jesse was not drafted; he and Henyey went to work under the auspices of the Office of Scientific Research and Development (OSRD), designing lens systems for military application. The two of them produced a number of OSRD reports on military applications of optical design, with titles such as "Unit Power Periscopes," "Tank and Anti-Tank Telescopes," and "Wide-Field Fast Cameras." Working with strange glasses, developing new methods of ray tracing, as well as dealing with industrial and military personnel, were new experiences for Jesse. As a junior Yerkes staff member he did not participate in administrative decisions, but the war work initiated him into the world of committee meetings and the responsibilities of management. He was thus prepared for membership on the postwar committee of the Office of Naval

Research, which funded small grants for astronomical research projects, in effect, a forerunner of what later would become the National Science Foundation's astronomy program. Jesse's observing runs at McDonald continued as he advanced by 1946 to the rank of associate professor.

THE MOVE UP TO THE 200-INCH TELESCOPE: FOUNDING THE CALTECH GRADUATE PROGRAM IN ASTRONOMY

In June 1948 Jesse arrived in Pasadena, having accepted an offer from Caltech of an associate professorship, with the principal duty of organizing a new graduate program in astronomy and astrophysics. Now a family man with two sons, eight-year-old George and two-year-old Peter, he and Naomi welcomed a return to urban living after 11 years in the remote countryside. The move coincided with the dedication of Caltech's great new 200-inch telescope, to which Jesse would now have access, and which held the promise of unprecedented research opportunities. At the same time, as executive officer of astronomy, a post he held until 1972, Jesse would need to devote a large amount of time to the administrative activity of selecting professorial and research staff for the department, as well as directly participating in the teaching and mentoring of graduate students. Despite these administrative and teaching responsibilities, Jesse chose not to eschew his research programs. He had been author or coauthor of 70 papers by 1948. His output expanded to more than 400 papers by the end of his active career in the early 1990s.

Jesse, who was almost immediately advanced to the rank of full professor, began teaching stellar atmospheres and interiors to physics and new astronomy graduate students, and took up the task of recruiting astronomy faculty. Early appointees included a number of Yerkes Ph.D.s (e.g., Arthur Code, Donald Osterbrock, Guido Münch), all of whom gained fame as prominent research astrophysicists in later years. Thus began the as-yet-untold story of the westward migration of the Yerkes style, which led to the reformation of science as it was conducted in the west coast departments and observatories. Already in 1947 Henyey had left Yerkes for the Berkeley astronomy department, to be followed in 1950 by Struve himself. Bolstering the graduate course offerings at Caltech were the lectures of resident Carnegie-Mt. Wilson astronomers, who became de facto visiting faculty following the formation of the joint authority known as the Mt. Wilson and Palomar Observatories. To this mix were added the unique contributions of Fritz Zwicky, who had already been a member of the Caltech physics faculty well before the expansion. The first Ph.D.s in the new program were Allan Sandage, Helmut Abt, and Halton Arp, all of whom became important research figures in later years.

In this milieu in which astronomy was closely coupled in a divisional structure with physics and mathematics, Jesse's research flourished as his penchant for exploring and explaining new phenomena expanded. In collaboration with Leverett Davis, a Caltech physicist with wide knowledge of electromagnetic theory, Jesse returned to his old love affair with the properties of interstellar grains. From studies of the polarization of starlight W. A. Hiltner had shown that the Milky Way was threaded with a weak but highly organized magnetic field. Davis and Greenstein modeled the grains as elongated and rapidly spinning, and predicted the field should lie along the spiral arms of the galaxy, as observed.

THE ABUNDANCE PROJECT

By 1952, I. S. Bowen, the first director of the Mt. Wilson and Palomar Observatories, had completed construction of the innovative 200-inch coude spectrograph, and using this instrument Jesse returned to the study of relative abundances, particularly in stars with abnormal spectra such as carbon stars and metallic-line A-type stars. Stimulated by friendship with William A. Fowler of the Caltech Kellogg Radiation Lab, as well as the 1956 predictions of the stellar origin of the heavy elements by Fowler and colleagues Fred Hoyle and Margaret and Geoffrey Burbidge, Jesse obtained funding from the Air Force Office of Scientific Research for the famous Abundance Project, which spanned the period from 1957 to 1970. The grant supported Jesse's observational program with the coude spectrograph and also a substantial analysis team of graduate students and postdocs, many of whom became prominent figures in today's astronomical world.

Advances in the theory of stellar evolution initiated by Sandage and Martin Schwarzschild and continued principally by Icko Iben, coupled with the corresponding interpretation of globular and open cluster color-magnitude diagrams by Sandage, Arp, Harold Johnson, and others, led to the conclusion that globular clusters and other halo stars must be older than the sun and much older than stars of open clusters. These results set the stage for the study of stellar abundances as a function of time since the big bang. The Abundance Project was thus at the heart of the quest to delineate galactic chemical evolution. Perhaps the most exciting of the early papers is one by H. L. Helfer, G. Wallerstein, and Greenstein in which it was shown conclusively that globular cluster giants had metal abundances as much as two orders of magnitude below that of the sun (relative to hydrogen). Using the photographic plates of the day, with their 1 percent quantum efficiency, obtaining spectra of even the brightest globular cluster stars represented a monumental task: Exposure times for a single star usually required more than one night, even using the 200-inch telescope coupled to Bowen's state-of-the-art coude spectrograph.

There followed a series of papers that established that halo field giants and subdwarfs, i.e., stars usually identified as having kinematics (proper motions and radial velocities) associated with the galactic halo, had metal deficiencies similar to those of globular cluster giants. Generally such objects proved to have subnormal ratios of the heavy elements (e.g., Sr, Y, Ba, Eu) to the common metals such as Fe. On the contrary, oxygen and the elements containing an even number of alpha particles (e.g., Mg, Ca, Ti) were found to be overabundant relative to Fe. Another paper showed that stars in the Hyades, a cluster much younger than the sun, nevertheless had compositions similar to that of the sun. Those taking part in these studies included colleagues such as Lawrence Aller (who had been a pioneer in showing the existence of stars with metallicities below that of the sun) and several postdocs who became notable figures in the astronomical world: Helfer and Wallerstein, already mentioned, along with G. and R. Cayrel, W. Sargent, P. Conti, H. Spinrad, A. Boesgaard, V. Weidemann, B. Baschek, J. Jugaku, and R. Parker, among others. A paper by Jesse and Wallerstein devoted to an analysis of two metal-poor halo giants known as CH-stars showed that these were not only carbon-rich but also were seriously overabundant in heavy elements such as Ba, La, and Ce, totally unlike the situation with the common metal-poor stars of the halo. All these results provided the observational testing ground for theories both of stellar structure and evolution and for scenarios on the origin of the chemical elements in the post-big-bang era.

RADIO ASTRONOMY, QSOS

Harking back to his 1937 collaboration with Fred Whipple in the abortive attempt to explain Jansky's newly discovered radio noise, Jesse retained his fascination with radio astronomy and, beginning in 1956, helped to organize Caltech's Owens Valley Radio Observatory. Most of the rest of the optical astronomy community had remained fairly indifferent to the postwar development of radio astronomy, until it was discovered in 1951 that a powerful radio source was associated with Cygnus A, a strange galaxy thought to be, in fact, two galaxies in collision. By 1954 many weaker radio sources were discovered, some identified with optical galaxies, some possibly with blue stellar objects, but lack of angular resolution at radio wavelengths led to significant uncertainties in optical identification.

In the same year Jesse organized a conference at Carnegie headquarters attended by radio astronomers from around the world, plus optical astronomers such as Jesse himself and R. Minkowski, as well as Lee DuBridge, then president of Caltech. Out of this meeting arose the founding not only of Owens Valley Radio Observatory but also the National Radio Astronomy Observatory. DuBridge, with Jesse's strong support, then invited John Bolton, an Australian radio astronomer, to set up Owens Valley and start the Caltech radio astronomy program. Owens Valley was dedicated in 1958 and quickly became one of the premier observatories of its kind.

Jesse's interest in radio astronomy soon crossed paths with another of his new interests—faint blue stellar objects—in an unexpected way. Stars with very blue colors were known to be a varied lot; some were halo horizontal branch stars, others white dwarfs, and still others cataclysmic variables such as old novae. Meanwhile, some strong radio-emitting objects that had been found in the Cambridge radio survey were clearly identified with active galaxies. Jesse took an interest in the subset of radio emitters that appeared to be stellar and that exhibited blue continua crossed by a few weak emission lines. The object called 3C48 was a good example; its few rather broad emission lines defied certain identification. The anomaly cleared up when Maarten Schmidt discovered the unmistakable signature of the hydrogen Balmer series in 3C273, a blue starlike radio source, of which an accurate position had been obtained by means of a lunar occultation. Amazingly, the hydrogen lines of 3C273 were redshifted by 16 percent. In consultation with Schmidt, Jesse realized that the lines of 3C48 could be identified with emission lines of [O I], [NeIII], and [NeV] redshifted by 37 percent. As Jesse himself has noted, the "logjam was broken." In a landmark paper Greenstein and Schmidt (1964) discussed the physics of the emitting region in these two objects, rejected the notion that gravitational redshifts could explain what was seen, produced evidence in support of the idea that the redshifts were cosmological, and noted the impossibility of a nuclear energy source as an explanation of the extraordinarily high luminosities required. Aside from a student study of the radiation from M31, this was Jesse's only foray into extragalactic astronomy. Having participated in the breakthrough physics of understanding QSOs, Jesse was content to leave the remaining mysteries for others to solve. It was entirely characteristic of his scientific style.

LATE RESEARCH: WHITE DWARFS, RED DWARFS

Beginning around 1965 and on into his post- (de jure) retirement years, Jesse's taste for conquering new horizons led him to an intensive study of white dwarfs, the last stage of evolution of stars with masses less than about 8 M(sun). They are a unique subset of the kind of blue stellar objects that had already piqued Jesse's interest. Observing these intrinsically as well as apparently faint stars presented a formidable task even for the 200-inch telescope; the lines are weak, often diffuse, and low spectral resolution is required if one is to have sufficient signal to noise. Until he retired from active observing in 1986 Jesse provided fundamental data for 550 white dwarfs, using the prime focus nebular spectrograph, later the multichannel spectrometer and double CCD spectrograph constructed by his faculty colleague J. B. Oke. Early in this period he and Olin Eggen (1965-1967) turned out spectral classifications, colors, luminosities, and space motions for 200 white dwarfs. With Virginia Trimble he derived the mean mass of white dwarfs (0.6 Msun) from the K-term in their motions and the corresponding gravitational redshift. In more than 60 papers spread during 20 years Jesse explored such additional topics as surface composition, energy distribution, line profiles, magnetic fields, rotation, gravitational diffusion, and cooling theory for white dwarfs. Accurate energy distributions plus models led to an accurate temperature scale. The well-known so-called "DA" white dwarfs were shown to have essentially no helium and virtually no metals, whereas non-DAs were shown to have no hydrogen but rather small amounts of carbon and metals. Many of these atmospheric abundance puzzles were found to be a result of gravitational diffusion rather than real abundance anomalies. Of more than usual interest was the discovery in one star of a seriously displaced Lyman-alpha line, resulting from a dipole magnetic field of 350 megagauss! Jesse also showed that white dwarfs have very small angular momentum, meaning that in the course of evolution their predecessors must lose not only mass but relatively more angular momentum presumably in magnetically coupled winds. In short, Jesse's work provided the essential backbone of all we presently know about white dwarfs. Near the end of his active career Jesse made excursions on the other side of the subject of intrinsically faint stars, with adventures in trying to detect faint M dwarfs and exploring the properties of candidate brown dwarfs. With Jesse there was always a new stellar astronomy field to conquer.

SCIENTIFIC STATESMAN

During his Caltech years, Jesse's advice was sought by many university groups and government agencies responsible for overseeing the growth of support for science in the post-World War II era. He served on some 50 different committees at various times in this period. Space does not permit an exhaustive listing, but a few of the important ones follow. He was a member of the Committee on Science and Public Policy of the National Academy of Sciences, advised NASA on such matters as the scientific management of the Space Telescope (which ultimately led to the establishment of the Space Telescope Science Institute), served on the Board of Overseers of Harvard University, and was chairman of the Board of Directors of the Association of Universities for Research in Astronomy, the managing entity of the National Optical Astronomy Observatories.

Probably of greatest importance to astronomers was his acceptance to be the chairman of the second decadal review of astronomy, the results of which are published in *Astronomy and Astrophysics for the 1970's*, more generally known as the Greenstein Report. Following on after the initial study of this kind, the Whitford Report of 1964, it was a response to the demand of the federal funding agencies for a prioritized list of future possible astronomical facilities and associated costs. Except for planetary missions, the Greenstein Report considered all of astronomy, ground-based (IR, radio, optical, solar), space (X and gamma rays), as well as theoretical astrophysics, dynamical astronomy, planetary astronomy, and statistical studies. Out of the panel discussions and further evaluations arose a rank-ordered list of priorities, with the VLT (Very Large Array) of radio telescopes, constructed ultimately in New Mexico, topping the list.

The period marked the end of an era, especially in ground-based astronomy, in which observational work had been dominated exclusively by private or semiprivate organizations in which access to facilities was restricted to local staff members. Facilities open to all qualified scientists on a competitive basis were now being funded with federal dollars. Having spent his career in the private observatory sector, Jesse quite naturally defended the idea that the private sector deserved federal support, too. He felt strongly that the future of astronomy lay in the kind of large-scale observational programs that could be mounted by brilliant and dedicated staff having virtually unlimited telescope access. But this outlook did not prevent Jesse from supporting the federal capitalization of public projects, even those that seemed contrary to more parochial interests. Marshall Cohen, Caltech professor emeritus, has recalled his experience with Jesse's leadership when he was a member of the Radio Panel in the 1970 decadal review.

At one point the panel members had a serious disagreement over the recommendation for the next large radio astronomy facility. Caltech, MIT and the National Radio Astronomy Observatory (NRAO) all had plans, they were mutually exclusive, and no one would give in. Jesse demanded that the panel pick one instrument for the Report, and finally we picked the NRAO proposal, which later was funded and became the VLT, an extremely important telescope. In this process Jesse was above institutional concerns, and while he would have been pleased if the Caltech proposal had been chosen, he did not push me or the Caltech proposal. In this and other ways, he was a national servant.

HONORS AND HONORIFIC OFFICES

In recognition of his scientific accomplishments and services to astronomy, Jesse was elected to many scholarly organizations including the National Academy of Sciences (in 1957), the American Academy of Arts and Sciences, the

American Philosophical Society, the Royal Academy of Sciences (Liege), and was elected an associate of the British Royal Astronomical Society. Honors included the California Scientist of the Year award in 1964, the Henry Norris Russell Lectureship of the American Astronomical Society in 1970, the Bruce Medal of the Astronomical Society of the Pacific in 1971, the NASA Distinguished Public Service Medal in 1974, and the Gold Medal of the Royal Astronomical Society in 1975. He received an honorary D.Sc. from the University of Arizona in 1987. At various times he was a visiting professor at Princeton, the Institute for Advanced Study, NORDITA (Copenhagen), and the University of Hawaii and was appointed Lee DuBridge Professor of Astrophysics at Caltech in 1970, a post he held into his (de jure) retirement years. He was honored at several international symposia dedicated to fields in which he worked, in recognition of his sixtieth, seventieth, and seventy-fifth birthdays.

SOME PERSONAL REMINISCENCES

I was not an insider and knew Jesse mostly from a distance. But we did share a seven-year collegial period (1960-1967) as staff members of the Mt. Wilson and Palomar Observatories, he at Caltech, I at Carnegie on Santa Barbara Street. I consulted with him occasionally on scientific matters, but as a junior staff member I played no role in the administrative activities and thus saw no part of Jesse in that theatre of action. As part of the broader observatory group one was invited to attend Friday luncheons at the Athenaeum, where if you were lucky, you might sit at Jesse's large table and be privileged to hear his take on the important astronomical topics of the day. He was a font of knowledge on many topics and loved to work out the basic astrophysics of some new discovery, his pen busy scratching its way on the large paper napkins the Athenaeum regularly provided. He often said that the real fun for him in astronomy was learning the physics (new to him!) required to explain some new astronomical phenomenon. He was not interested in routine activities. Jesse maintained his love of spectroscopy as the fundamental observational tool of astrophysics. From time to time he would utter a kind of mantra. "Spectroscopy is the queen of astronomy," he would say. Regrettably, I never thought to ask him who or what was king!

Jesse maintained a collection of spectrograms of old novae, since these fell in the category of faint blue stars (in their postexplosive stage). He was especially fond of the old nova called WZ Sagittae, because its spectrum contained not only the characteristic hydrogen emission lines but also the absorption lines of a white dwarf. At one of these luncheons I had just returned from an observing run at Palomar and had discovered that WZ Sagittae was a binary star with an incredibly (for those days) short period of 82 minutes. I had vaguely remembered hearing an arcane (to me) lecture by S. Chandrasekhar on gravitational radiation and wondered whether this old nova might be a candidate for emission of gravitational waves. But where was one to look for information? Jesse knew, as always a font of knowledge. I should look in the book by Landau and Lifshitz and go talk with Jon Mathews in the physics department who knew about such arcane things. In the end, two developments came out of this. Jesse, Mathews, and I wrote a paper on the possibility that WZ Sagittae might emit gravitational energy on an interesting cosmic time scale. A few weeks later there appeared on my desk a package containing Jesse's complete library of old nova spectra with a note: "Bob, the field is yours now. Here is my collection. Use it well." It was a demonstration of Jesse's kindness toward junior scientists.

Earlier, knowing of my interest in old novae while I was still on the Yerkes staff, Jesse wrote asking whether I would be interested in analyzing spectra of nova DQ Herculis, then the best-known old nova (1934), around its 4-hour and 39-minute orbital cycle. Of course I would! Jesse got the spectra with the 200-inch nebular spectrograph and they were amazing; they showed the rotational disturbance through the eclipse, thus proving that the collapsed star was surrounded by an accretion disk. Again Jesse shared with someone much younger the joy of discovery and publication of research. But this was characteristic of the man; of the scores of papers turned out by members of the Abundance Project, Jesse's name almost never appears as the first author. He was content to generally direct and be a major advisor and allow the younger people to have their place in the sun. According to Ann Boesgaard, professor of astronomy at the University of Hawaii and earlier a postdoc in the Abundance Project, Jesse was the first of the old-guard Mt. Wilson and Palomar astronomers to support the work of, and suitable accommodations for, female astronomers at the telescopes, significant progress into what had been an exclusively male-oriented club.

A man of wide cultural interests, Jesse loved classical music, particularly the chamber music of Mozart and Schubert, and especially the late quartets of Beethoven. At Jesse's memorial service, son Peter engaged a string quartet to play the cavatina from Beethoven's Quartet, op. 130, which seemed especially appropriate. During a six-month visit to Caltech in 1980, I learned of Jesse's deep interest in Asian art and was shown some of his collection of Asian treasures. His interest extended beyond collecting, serving as he did on the Board of Trustees of the Pacific Asia Museum from 1979 onward.

Jesse also had an interest in things more mundane. He loved fast foreign cars and once held a Caltech speed record for the trip from Pasadena to Palomar. He enjoyed Bordeaux wines and worried on one occasion whether he should buy a case of a well-known first growth from a good year fearing he might not live to drink it in its maturity. He also loved a brand of small cigar that was especially fragrant; you didn't have to look at the schedule to know whether you were following Jesse in the 200-inch prime-focus cage: There was something in the air!

Naomi Greenstein passed away in 2001 after a long illness. Jesse is survived by his two sons, George, a professor of astronomy at Amherst, and Peter, a librarian and jazz musician working in Oakland, California, and their families. Jesse was one of the great figures of U.S. astronomy in the twentieth century. In this era of multiauthored papers occasionally numbering in scores, he was virtually the last of a vanishing breed, representing a style no longer much with us: the single astronomer alone on a dark night in a cramped telescope cage, exposed and cold, in a personal confrontation with God, or Nature, or the Truth, or whatever you may choose to call it.

I AM INDEBTED TO George Greenstein for valuable family information and to Peter Greenstein for access to his father's extended personal memoir. I thank Prof. Marshall Cohen for his encouragement and permission to quote his experiences as a member of the Radio Astronomy Panel of the 1970 decadal review. Valuable information was also provided by Profs. George Wallerstein and Ann Boesgaard, to whom I am greatly indebted. I thank Don Osterbrock, Marshall Cohen, Peter and George Greenstein, and Ann Boesgaard for reading and commenting on a first draft of the manuscript.

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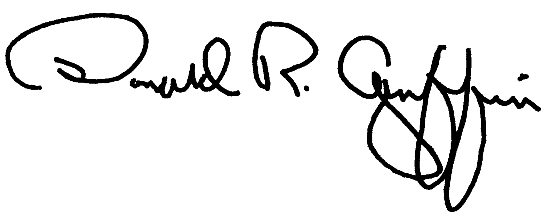

DONALD R. GRIFFIN

August 3, 1915–November 7, 2003

BY CHARLES G. GROSS

MOST SCIENTISTS SEEK—but never attain—two goals. The first is to discover something so new as to have been previously inconceivable. The second is to radically change the way the natural world is viewed. Don Griffin did both. He discovered (with Robert Galambos) a new and unique sensory world, echolocation, in which bats can perceive their surroundings by listening to echoes of ultrasonic sounds that they produce. In addition, he brought the study of animal consciousness back from the limbo of forbidden topics to make it a central subject in the contemporary study of brain and behavior.

EARLY YEARS

Donald R. (Redfield) Griffin was born in Southampton, New York, but spent his early childhood in an eighteenth-century farmhouse in a rural area near Scarsdale, New York. His father, Henry Farrand Griffin, was a serious amateur historian and novelist, who worked as a reporter and in advertising before retiring early to pursue his literary interests. His mother, Mary Whitney Redfield, read to him so much that his father feared for his ability to learn to read. His favorite books were Ernest Thompson Seton's animal stories and the *National Geographic Magazine's Mammals of North America*. An important scientific influence on the young Griffin was his uncle Alfred C. Redfield, a Harvard professor of biology, who was also a bird-watcher, hunter, and one of the founders of the Woods Hole Oceanographic Institution.

Young Griffin's hobbies were to become the core of his professional interests and achievements. By the age of 12, Griffin was trapping and skinning small local mammals. Because of his poor teeth, his parents regularly took him to a Boston dentist. These trips were rewarded with visits to the Boston Museum of Natural History, where its librarian introduced him to scientific journals, and its curators to turning his trapped animals into study skins. At 15, with his uncle's encouragement, he subscribed to the *Journal of Mammalogy*, where he was to publish five papers before graduating from college. In his autobiographical writings Griffin described his schooling as "extraordinarily irregular." After a few years at local private schools his "long-suffering" parents decided on home schooling. His father taught him English, history, Latin, and French. A former high school teacher handled the German and math. After a few years of trapping, skinning, sailing, and a couple of hours of daily lessons, his parents sent him to Phillips Andover, where he started but never finished the tenth and eleventh grades. The next year was spent at home again, collecting and sailing, and with tutoring adequate enough that he was admitted to Harvard College in the fall of 1934.

During his high school years Griffin seemed to be more of a nascent serious scientist than, say, Darwin, who had spent his undergraduate days hunting and collecting beetles rather than studying. For example, young Griffin thought he would be able to describe a new subspecies of California mice, but then he realized his hopes were based on errors in the literature, his first realization of the fragility of scientific fact. He tried to estimate the population of various hunted species by obtaining the number of animals killed from the state game authorities. He spent several weeks learning about bird banding at a major banding station and was then authorized to set up a banding substation of his own near his home.

Soon he combined his interests in trapping small mammals and banding birds by banding bats. Recruiting friends, he banded tens of thousands of little brown bats, *Myotis lucifugus*. (For the rest of his life he readily found research volunteers to help in such things as lugging heavy electronic equipment into the field, climbing into unexplored caverns, following birds in an airplane, building huts on remote sand spits, and navigating Amazon rivers in dugout canoes full of recording devices.) This bat-banding project resulted in finding that bats migrated between caves in Vermont and nurseries as far away as Cape Cod. Eventually it produced evidence of homing after displacement of more than 50 miles and of unsuspected longevity of these animals. It also yielded his first scientific publication, as a Harvard freshman, in 1934.

Griffin's sailing interests led to his second paper. While sailing in the summer before entering college, he had encountered several seal carcasses left by hunters who only wanted their noses for the bounty provided by the state. Little was known about what these animals ate, so he collected the contents of their stomachs and, with the help of several curators at Harvard's Museum of Comparative Zoology, identified their contents. In one of his characteristically dry and self-effacing memoirs Griffin tells of how, many years later when he was the chairman of the Harvard biology department, some young discontented molecular biologist in the department sent him reprint requests for this paper in the names of several well-known molecular biologists. Griffin actually sent out the faded reprints until he realized it was a hoax.

UNDERGRADUATE YEARS

As an undergraduate biology major, Griffin took his first science courses but reported mediocre grades in everything but the courses on mammals or birds. At this time John Welsh was studying circadian rhythms in invertebrates and encouraged Griffin to do so in bats. This was an interesting problem because the bats hibernated for long periods under constant conditions in dark caves. Griffin brought some of his bats into the lab and, using the standard physiological instrument of the time, the smoked drum kymograph, showed that indeed they had endogenous rhythms under constant conditions, yielding another paper in the *Journal of Mammalogy*.

Griffin knew Lazzaro Spallanzani's (1729-1799) work on bat orientation. In a brilliant series of experiments with all the requisite controls Spallanzani had demonstrated that bats do not require their eyes, but do need their ears, to navigate. He speculated that perhaps the sound of the bats' wings or body might be reflected from objects. Griffin also was familiar with the English physiologist Hartridge's suggestion that bats might use sounds of high frequency to orientate. At this time a Harvard physics professor, G. W. Pierce, had just developed devices (the first of their kind) that could detect and produce high-frequency sounds above the human hearing range. Two fellow students, James Fisk (later president of Bell Labs) and Talbot Waterman (later a Yale zoology professor), suggested to Griffin that he take his bats to Pierce to find out whether they produced high-frequency sounds.

Pierce was quite enthusiastic about the idea. In fact, he had been studying the ultrasonic sounds of insects (with the help of Vince Dethier, later the doyen of U.S. experimental entomologists). When they put the bat in front of Pierce's parabolic ultrasonic detector, they observed that the bats were producing sounds that the humans could not hear, but when the animals were flying around the room no such sounds were detected. Nor did the production of high-frequency sound seem to have any effect on the flying bats' ability to orient. When they published their observations, they suggested that the function of the supersonic sounds might be in social communication rather than orientation. (Later Griffin realized that the detector had not been sufficiently directional to pick up the bat signals in flight. Even later the social communication role for certain bat ultrasonic cries was confirmed.)

When Griffin was a senior, he was in a quandary about applying to Harvard's graduate school in biology because its faculty had little regard for Griffin's current interest in bird navigation. "Wiser heads emphasized that if I really wanted to be a serious scientist I should put aside such childish interest and turn to some important subject like physiology." The problem was solved with the announcement of the joint appointment of Karl Lashley to the Harvard psychology and biology departments. Lashley's appointment had been the result of the command of Harvard's president, James B. Conant, to hire "the best psychologist in the world." Karl Lashley was the leading "physiological psychologist" of his time and the teacher of many subsequently famous investigators of brain function and behavior. His particular interest to Griffin was that he had written a long and authoritative historical and experimental paper on bird homing (with J. B. Watson, later the founder of behaviorism) and had carried out his own experiments on orientation in terns. Lashley took him on as a graduate student but encouraged him to take several courses in experimental psychology, which he did.

GRADUATE SCHOOL

In graduate school Griffin met another student, Robert Galambos, who was recording cochlear microphonics from guinea pigs under Hallowell Davis, a leading auditory physiologist at Harvard Medical School and suggested Galambos look for bat cochlear microphonics in response to high-frequency sounds. They borrowed Pierce's instruments, and Galambos was soon able to demonstrate responses of the bat ear to ultrasonic sounds. In a series of experiments Griffin and Galambos then showed that bats do indeed avoid obstacles by hearing the echoes of their cries. Here is a recent reminiscence by Galambos of these experiments.

> Don divided a sound treated experimental room into equal parts by hanging a row of wires from the ceiling. We aimed the microphone of the Pierce device at this wire array, and began to count the number of times a bat flying through the wires will hit them when normal, or deaf or mute. . . . The impairments we produced [by plugging the ears or tying the mouth shut] were all reversible. . . . We also recorded the output of the Pierce device and correlated the bat's vocal output as it approached the barrier with whether it hit or missed the wires. . . . Everything we predicted did happen. Nothing ever went wrong. We never disagreed. . . . We suspected our claims might be controversial and decided a movie demonstration might help silence the skeptics. [In recent years this original silent and sound movie has been increasingly shown on nature and science television programs in many different countries around the world. Galambos at age 90 is still a very active professor of neuroscience at the University of California, San Diego.]

Needless to say, the scientific community was very skeptical at first, but the film and visits to their laboratory were soon convincing. As Griffin put it later, "Radar and sonar were still highly classified developments in military technology, and the notion that bats might do anything even remotely analogous to the latest triumphs of electronic engineering struck most people as not only implausible but emotionally repugnant."

These experiments establishing bat echolocation were reported in Griffin and Galambos's two seminal papers and formed part of the latter's doctoral thesis. Griffin's thesis, by prior agreement, was on bird navigation, the problem he had originally planned to study in graduate school experiments. The central question was whether birds released in unfamiliar territory immediately determined the homeward direction and flew directly back to their nests. He captured petrels, gulls, and terns and transported them, often in rotating cages, in different directions from the site of their capture, then released them and timed their return home. However, their flight times home were consistent with both a search until they found familiar landmarks and a leisurely but direct route home.

Directly tracking them should disambiguate these possibilities he hoped, so he got Alexander Forbes (professor of physiology at Harvard Medical School and one of the founders of modern neurophysiology) to take him up in Forbes's single-engine plane to try to track some gulls. Later Griffin took flying lessons and bought his own two-seater with funds from the Harvard Society of Fellows. The results were again consistent with both a search pattern and true homing.

The Society of Fellows awarded three-year Junior Fellowships with generous research funds. The fellowship was originally supposed to be a super elite substitute for a Ph.D. with no required courses, teaching, exams, degrees, or requirements except for attending candlelit dinners along with the senior fellows. In practice, when the junior fellows went on the job market, say in distant Berkeley, they were told in effect "no degree, no job" and had to go back and get conventional doctorates. Today most junior fellows earn their doctorates first, and it is a kind of fancy postdoc club, imitated predictably at such places as Princeton and Columbia. Griffin was fortunate to get elected to a Junior Fellowship, since his undergraduate grades had been too poor for a conventional graduate fellowship.

WARTIME

With the onset of war in 1941 Griffin became involved in war research at Harvard. His first assignment was to S. S. Stevens's psychoacoustic laboratory. (Stevens was the founder of modern psychophysics.) There Griffin worked on auditory communication problems and acquired valuable familiarity with acoustic equipment. After a stint in the Harvard fatigue laboratory (working on such problems as the optimum gloves for handling fly buttons), he worked with George Wald (subsequently a Nobel laureate) on problems of night vision.

One rather weird wartime incident was the Bat Bomb project. Lytle Adams came to Griffin with the idea of equipping bats with small incendiary bombs and releasing them by plane over Tokyo, where they would roost in Japanese "paper" houses and set fire to them. The government was supporting this idea, and Griffin agreed to help until he realized that there was no way bats could carry an adequate payload. In spite of Griffin's disavowal of its feasibility, the Bat Bomb project continued on, even involving at one point Louis Fieser, the distinguished organic chemist and inventor of napalm. In his account years later Adams continued to defend the project and claimed that it would have ended the war in a quicker and more humane way than Hiroshima and Nagasaki.

After the war Griffin moved to the Cornell zoology department for seven years before returning to Harvard for another twelve years. The next paragraphs summarize some of his research interest in those years.

FURTHER RESEARCH ON BAT NAVIGATION

Research on bat echolocation (Griffin's term) expanded in a number of different directions with an increasing number of collaborators. (Indeed by the time of his death most of the now numerous bat researchers everywhere in the world saw themselves directly or indirectly, implicitly or explicitly, as his collaborators.) One such direction was to determine the limits of the avoidance and object detection abilities afforded by echolocation. It was clear early that Myotis could discriminate wires down to a quarter of a millimeter, but could they actually echolocate moving-insect prey in the dark? Field experiments suggested that they could. This was confirmed by combining acoustic recording with ultra-high-speed strobe photography in an enclosure with released fruit flies and then weighing the bats before and after a short period of catching flies. Furthermore, the bats could quickly learn to discriminate pebbles and other inedible objects from flying insects. These experiments were carried out with Alan Grinnell, Fred Webster, and others.

Another direction initiated by Griffin with his collaborators Alan Grinnell and Nobuo Suga (and encouraged by Galambos) was the neurophysiology of bat echolocation. Today, largely due to the work of Suga and his students, more seems to be known about the organization of auditory cortex in the bat than in any other animal.

Whereas the North American bats initially studied by Griffin emitted brief frequency-modulated (FM) signals, in 1950 F. P. Mohres discovered that the European horseshoe bat used longer-duration constant-frequency signals for echolocation. This inspired Griffin, Alvin Novick, and other collaborators to survey the signals produced by different species of bats. As most bats species are tropical, this led Griffin, Novick, and their collaborators to a number of exciting Latin American expeditions and the discovery of many different modes of echolocation, including one specialized for fishing and others in cave-dwelling birds.

In the last weeks of his life Griffin was out "night after night" on Cape Cod "still trying to learn more about bats."

BIRDS AND OTHER CREATURES

Griffin continued to work on the mysteries of bird navigation. What made this a difficult problem was that although it became clear that birds (or some birds under some conditions) were using such cues as the elevation of the sun, the pattern of stars, their circadian rhythms, the earth's magnetism, and spatial memory, it was difficult to sort out the interaction and relative roles of these cues. Griffin pioneered in the use of airplanes, radar, and high-altitude balloons to study this problem. [My first publication, on bird navigation, arose out of a paper I wrote for an undergraduate seminar with Griffin. I then researched in his lab on the subject. My most vivid, if irrelevant, memory was the time he asked me to get the car battery from the next room for use as a power supply, and I answered, "What does it look like?" He gave this Brooklyn boy a brief strange look, and then went and picked it up himself.]

Griffin's discovery of a "new sense" in bats probably influenced, at least in part, the discovery of other "new animal senses" such as infrared vision in snakes, infrasonic signals in elephants, and orientation and discrimination in electric fish. He played a more direct role in the story of the dancing language of bees. During World War II the Austrian zoologist Karl von Frisch had discovered that honeybees could communicate the distance, direction, and desirability of food sources by a dance-like behavior. This work was hardly known in America in 1949 when Griffin arranged for him to give a series of lectures at Cornell, and then across the country, and shepherded their publication through Cornell University Press. Griffin had initially been skeptical until he replicated some of the critical experiments himself. (At the age of 72, Griffin published his last experimental paper; it was on bees.)

Griffin was interested in how beavers communicate. The last weeks of his life found him introducing microphones into beavers' nests near the Harvard Field Station in Concord. Indeed, the number of anecdotes about the field studies he carried out in his last, and eighty-eighth, year that I collected while preparing this memoir is a measure of the man.

THE ROCKEFELLER INSTITUTE AND BACK TO HARVARD

In 1965 Griffin left Harvard to organize a new Institute for Research in Animal Behavior jointly sponsored by the Rockefeller University and the New York Zoological Society. It eventually included a field station in Millbrook, New York. Joined by the leading ethologist, Peter Marler, and Ferdinando Nottebohm, the well-known investigator of bird song and adult neurogenesis, the institute became one of the leading U.S. centers for the study of animal behavior. Among Griffin's collaborators and students at the institute were Roger Payne, discoverer of acoustic hunting by owls and of whale songs and now the leading advocate of whale conservation; Jim Gould, who extended von Frisch's bee studies; and Carol Ristau, pioneer in the study of intentionality in the piping plover. From 1979 to 1983 Griffin was president of the Henry Frank Guggenheim Foundation, and he used this position to encourage research on animal behavior.

When Griffin retired from Rockefeller in 1986 he spent a year at Princeton University and then returned to Harvard, where he worked at the Concord Field Station and occasionally taught undergraduates. In this final period of his life he continued his experimental work on bats, birds, and beavers, as well as his cognitive ethology advocacy described below.

COGNITIVE ETHOLOGY

For about the first 40 years Griffin's career had been that of the very hard-nosed empiricist and skeptic, typified by the following oft told tale (attributed to Griffin's students Donald Kennedy, former president of Stanford and FDA commissioner; and Roger Payne, among others). "When passing a flock of sheep while traveling in a car, his companion noted that among the flock of sheep there were two that were black. Griffin replied, 'They're black on the side facing us anyway.'" Then in 1976 Griffin began to publish a series of books and papers that contained no new data, no figures, but a host of citations and arguments from philosophers as well as scientists that challenged the contemporary worldview of animals. He claimed that animals (and not just chimpanzees or even mammals) were aware and conscious and these properties of their mind should be the subject of scientific study, a field he named "cognitive ethology."

At least at the beginning, these claims and exhortations were usually greeted by harsh and angry criticism (one critic called them the satanic verses of animal cognition) or the sadness of seeing a great experimenter supposedly slipping into premature senility. (He himself even called this interest an example of "philosopause.") To better understand why imputing awareness, or even minds, to animals was considered outrageous, or at least, extra-scientific, by most of those who studied animal behavior, we need to go back to Charles Darwin and the beginning of modern biology.

One of Darwin's central points was the continuity of humans and other animals. As evidence of mental continuity Darwin cited examples from animals of humanlike emotions of joy, affection, anger, and terror, as well as of what we now call cognitive functions, such as attention, memory, imagination, and reason. George Romanes continued this tradition in what became know as the anecdotal school. C. Lloyd Morgan reacted against this approach and formulated what became known as Lloyd Morgan's canon, essentially the application of the law of parsimony to animal behavior: "In no case may we interpret an action as the outcome of the exercise of a higher psychological faculty, if it can be interpreted as the outcome of one which stands lower in the psychological scale." This quickly came to imply the rejection of animal consciousness and awareness, and a wariness to impute any complex cognitive function to animals. This tendency was reinforced by Jacques Loeb's theory of tropisms and the Russian school of reflexology, which also downplayed or denied consciousness in animals as well as humans. All these "objectivist" tendencies came together in the behaviorist movement, founded by J. B. Watson. The dominant figure in behaviorism, indeed in all of U.S. psychology until the rise of cognitive psychology, was B. F. Skinner. Skinner and the other "radical behaviorists" flatly denied the validity of the scientific study of consciousness, attention, awareness, thought, and other mental phenomena in humans, as well as other animals.

The other principal group studying animal behavior was the ethologists deriving from a European zoological tradition. They tended to stress the role of innate wiring in animal behavior, in contrast to the behaviorists who stressed the role of experience; however, they too obeyed Morgan's canon and were generally uninterested in the role of consciousness, intention, and mental experience in animal behavior. The cognitive revolution against behaviorism starting in the 1960s brought consciousness, attention, and awareness back into human psychology but left other animals still essentially mindless and unaware.

Thus Griffin's plea for studying the question of animal awareness (1976) was fiercely counter to the prevailing ideology in both psychology and zoology. Griffin used a variety of arguments, coming from different directions and different fields, to attack this view. One central argument was that it was simply anti-intellectual and antiscientific to deny any subject an objective and experimental inquiry. A second argument was Darwin's original one: the continuity of humans and other animals. Another argument was that animal communication, albeit admittedly fundamentally different from human language, might provide "a window on the animal mind."

In his next two books, *Animal Thinking* (1984) and *Animal Minds* (1992), these arguments were amplified and supported by a Romanes-like compendium of experiments and observations that greatly enhanced the case for animal consciousness and awareness. They included studies of tool construction and use, communication, planning, deception, blindsight, cooperative hunting, and intentionality. Two new lines of evidence came into prominence. The first was a host of neurophysiological experiments seeking mechanisms of consciousness. Since most of these were invasive, such as single neuron recording, they could only be done in animals and thus, with all due respect to Morgan, they assumed animals were conscious, reflecting the change in the intellectual air that Griffin had helped bring about.

The second line of new evidence, increasingly prominent in Griffin's last book and papers on cognitive ethnology, was of studies done by Griffin's students, such as Gould, Payne, or Ristau, and by the increasing number of quasi students, investigators who were never formally his graduate students but readily acknowledge him as their mentors, such as Dorothy Cheney and Robert Seyfarth (who studied communicative alarm calls and deception in vervet monkeys) and Irene Pepperberg (who trained a grey parrot to answer cognitive questions in English). (Even his formal students are not readily identified, as he rarely attached his name to their work.)

Although many biologists and psychologists are still skeptical or uneasy about Griffin's attribution of consciousness to nonhumans, particularly invertebrates, there is no question that he has radically opened up the field of animal behavior to new questions, ideas, and experiments about animal cognition. Because of his own towering achievements as a meticulous and skeptical experimental naturalist, his cogent and repeated arguments about studying the animal mind and his support and encouragement of others, coupled with his unusual modesty and soft-spoken nature, Donald Griffin was able to affect a major revolution in what scientists do and think about the cognition of nonhuman animals.

Griffin was elected to the American Philosophical Society, the National Academy of Sciences, and the American Academy of Arts and Sciences. He received the Eliot Medal of the National Academy of Sciences for *Listening in the Dark* (1959) and the Phi Beta Kappa science prize for *Bird Migration* (1964), and was awarded honorary degrees by Ripon College and Eberhard-Karls Universität.

Griffin leaves two daughters, Janet Abbott and Margaret Griffin, and a son, John, from his first marriage to the late

Ruth Castle. His second wife was Jocelyn Crane, an expert on crab behavior and biology, who died in 1998.

THE ACCOUNT OF Griffin's early life comes from his own memoirs (e.g., 1998). Previous drafts of this essay received valuable comments from Robert Galambos, Alan Grinnell, Marc Hauser, Byron Campbell, and Elizabeth Gould. In addition, the following provided helpful comments on Griffin's life and contributions: Robert Galambos, Alan Grinnell, James Simmons, Roger Payne, Marc Hauser, Greg Auger, Jim Gould, Janet Abbott, and Herb Terrace.

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Wassily Hoeffding

WASSILY HOEFFDING

June 12, 1914–February 28, 1991

BY NICHOLAS I. FISHER AND WILLEM R. VAN ZWET

Wassily Hoeffding was one of the founding fathers of nonparametric statistics, the science of analyzing data without making unnecessarily restrictive assumptions about their origin. His great strength was his deep understanding of statistics that told him which problems to attack, at what level of generality, and with what mathematical means. He was not interested in generality for generality's sake, and he always kept his mathematics as simple as possible. When he had dealt with a problem he left it to others to examine the consequences of his results. It is striking how often he picked the "right" problems, the ones that much later turned out to have consequences that went far beyond his deceptively simple results, and opened up entirely new fields of research. He was the statistician's statistician in the sense that he supplied what his colleagues needed today or would need in future.

Wassily was the most unassuming person imaginable with a mild but permanently present sense of humor. Throughout his life he suffered from diabetes and bad eyesight and hearing, but his indomitable spirit made these handicaps seem like minor inconveniences. As our colleague Ildar Ibragimov wrote[1] to us from St. Petersburg, Wassily Hoeffding was a very intelligent and broadly educated person with lively interests and a truly noble soul that we rarely come across even in great people.

THE EARLY YEARS, 1914-1947

Wassily was born in Mustamäki, Finland (Gorkovskoye, Russia since 1940), although his place of birth is registered as St. Petersburg on his birth certificate. His parents had traveled from their home in Tsarskoye Selo (now Pushkin) to spend the summer in Mustamäki. In his autobiographical article (1982, p. 100) Wassily provided some family history.

> My father, whose parents were Danish, was an economist and a disciple of Peter Struve, the Russian social scientist and public figure. An uncle of my father's was Harald Hoeffding,[2] the philosopher. My mother, née Wedensky, had studied medicine. Both grandfathers had been engineers.

His younger brother, Oleg,[3] became a military historian in the United States, and Oleg's life interleaved in interesting ways with Wassily's, as we shall see.

In 1918 the family left Tsarskoye Selo for the Ukraine and, after traveling through scenes of civil war, finally left Russia for Denmark in 1920, where Wassily entered school.

> In 1924 the family settled in Berlin. In high school, an Oberrealschule which put emphasis on natural sciences and modern languages, I liked mathematics and biology and disliked physics. When I finished high school in 1933, I had no definite career in mind. I thought I would become an economist like my father and entered the Handelschochschule . . . in Berlin. But I soon found that economics was too vague a science for me. Chance phenomena and their laws captured my interest. I performed series of random tossings and recorded their outcomes before I knew much about probability theory. One of the few books on chance phenomena that I found in the library of the Hochschule was Die Analyse des Zufalls by H. E. Timerding, and it fascinated me. In 1934 I entered Berlin University to study mathematics . . .
>
> The meager fare in mathematical statistics that I was fed in my lectures in Berlin, I tried to supplement by reading journals. But somehow I did not

fully absorb the spirit of research at the frontier of the subject in my student days. My Ph.D. dissertation . . . was in descriptive statistics and did not deal with sampling . . . My Doktorvater or Ph.D. supervisor was Klose. I chose the topic of the thesis and worked on it largely by myself, with some suggestions and encouragement from him. He was a Baltic German and had his own ideas about Russians. He warned me to refrain from making exaggerated claims in my thesis that I could not substantiate as, he thought, Russians were prone to do. (1982, p. 100)

Wassily's first published papers (1940) appeared in a series of the University of Berlin and reported (in German) the results of his Ph.D. investigation into the correlation phenomenon. Characteristically he at once saw the "right" formulation of this problem. He studied correlation properties of bivariate distributions that are invariant under arbitrary monotone transformations of the marginals. The work is related to later work on dependence by Fréchet and Lehmann. But the concept of invariance under monotone transformations is relevant in a much broader setting and lies at the root of nonparametric or rank methods, the major development in statistics in the years following World War II. In fact, in Hoeffding's correlation papers one encounters Spearman's rank correlation coefficient, which also pre-dates the formal development of rank statistics. It is not surprising that his first published work in an international journal deals with rank correlation (1947).

The war, and the relative obscurity of university series to statistical researchers elsewhere, meant that his results were known only to the few people outside Germany to whom he had sent copies of his thesis. It is only recently, with the publication of English translations of these papers in his *Collected Works,*[4] that he is starting to receive credit for results rediscovered many years later by others.

However, he did not proceed immediately to a research career.

On completing my studies in 1940, I accepted two part-time jobs: as an editorial assistant with the *Jahrbuch über die Fortschritte der Mathematik* and as a research assistant with the inter-university institute for actuarial science . . . I held both jobs until almost the end of the war. I never applied for a teaching position in Germany: I had been stateless since leaving Russia and did not wish to acquire German citizenship, which was necessary to hold a university teaching job. (1982, pp. 101-102)

Meanwhile, brother Oleg had spent the war in England, working in the Economic Division of the U.S. Embassy. Toward the end of the war Wassily and his mother experienced a very heavy bombing raid. He subsequently discovered that Oleg had been involved in planning it.[5]

In February 1945 I left Berlin with my mother for a small town in the province of Hanover to stay with a Swiss friend of my father's . . . My father stayed behind and was captured by what was later to become the KGB. He had been employed for many years at the office of the American Commercial Attaché and then had been the economic correspondent of American and Swiss periodicals. This made him a "spy" in the eyes of the KGB. (1982, p. 103)

During this time, Wassily asked Oleg to send him a copy of M. G. Kendall's recently released volume 1 of *The Advanced Theory of Statistics.* "It read to me like a revelation," he wrote. And it was also in Hanover that he wrote "my first statistical paper in the modern sense of the word," establishing the asymptotic normality of Kendall's rank correlation coefficient τ in the general case of independent identically distributed random vectors. However, the significance of the paper resided not so much in its content but in the path down which it led him to his groundbreaking theory of *U*-statistics. But first he had to leave Germany and find a job.

[W]e left Germany for Switzerland and arrived in New York City in September 1946 . . . As I was unemployed, I attended lectures at Columbia University by Abraham Wald, Jack Wolfowitz, and Jerzy Neyman, who was then visiting Columbia. I was in the thick of contemporary statistics . . .

In 1940 I had sent a few copies of my Ph.D. thesis to statisticians in other countries, including the United States . . . Thus my name was not entirely unknown in the USA. My brother Oleg, whose arrival in New York preceded mine by three months and who now was an economics instructor at Columbia, helped me to get invitations from the Cowles Commission for Economic Research . . . and from Harold Hotelling, who had just established the Department of Mathematical Statistics at the University of North Carolina at Chapel Hill.

I first went to Chicago to give a talk on what I later called *U*-statistics . . . Soon after, a letter from Hotelling offered me a position as research associate. He did not ask for a preliminary visit; later he said he had been impressed that a Ph.D. thesis in mathematical statistics had come out of Germany. In May 1947 I arrived in Chapel Hill. (1982, pp. 103-104)

Wassily had arrived at his academic home. Apart from conference travel, and the occasional short visit (six months or less) to other scientific institutions, he remained in Chapel Hill for the rest of his life.

I was to remain in Chapel Hill until my retirement and beyond. Congenial colleagues, a relaxed, informal academic life style, the attractive nature of the town, the relative closeness of the sea and the mountains, combined with an inborn inertia, made me resist the temptations of moving to other campuses. Being somewhat reserved by nature, I cherish all the more the friendships and contacts I have had with my colleagues and students in the department and their families. (1982, p. 105)

ACADEMIC CAREER AT THE UNIVERSITY OF NORTH CAROLINA, CHAPEL HILL, 1947-1979

The Department of Statistics at Chapel Hill indeed provided a congenial professional environment. Hotelling had attracted several leading researchers, including R. C. Bose, S. N. Roy, P. L. Hsu, Herbert Robbins, and Ram Gnanadesikan. Wassily immediately embarked on an active research career. A year after his arrival he published a paper on *U*-statistics (1948) that has become a landmark in the development of asymptotic statistics. A *U*-statistic is of the form

$$U_n = \Sigma \ldots \Sigma\ h(X_{i(1)}, \ldots, X_{i(k)}),$$

where $X_1, \ldots, X_n$ are independent and identically distributed (i.i.d.) random variables and the summation extends over all k-tuples of distinct elements of $\{1, \ldots, n\}$. U-statistics were introduced by Halmos as unbiased estimators of their expectation, but are found to play a role in almost any statistical setting. From a probabilistic point of view, U-statistics of degree $k = 2, 3, \ldots$ are successive generalizations of sums of i.i.d. random variables (the case $k = 1$), the study of which has formed the central part of probability theory for centuries. Hoeffding performed the essential moment calculations for U-statistics, established their consistency as well as their asymptotic normality as $n \to \infty$, and dealt with the case where the X_i are not identically distributed as an encore. Wassily liked to think of this paper as his "real" Ph.D. dissertation. It is fair to say that this paper started and at the same time finished the probabilistic study of U-statistics until Hoeffding himself took up the subject once more some 13 years later.

In an important unpublished paper in 1961 (which did not appear in his *Collected Works* but was discussed by others) he introduced a decomposition of an arbitrary function of $X_1, \ldots, X_n$ in terms of U-statistics. This is now known as Hoeffding's decomposition and has proved to be a powerful tool in asymptotic statistical theory.

At the time Wassily was sharing an office with Herbert Robbins and they soon became close friends. They collaborated on a paper (1948) on a problem of common interest, the central limit theorem for m-dependent variables. For Wassily there was an obvious link with his work on U-statistics and the paper may be considered as a starting point for the vast literature on the central limit theorem under mixing conditions. Three years later Hoeffding returned to the central limit theorem under nonstandard conditions when he proved a combinatorial central limit theorem (1951): If $(R_1, \ldots , R_n)$ is a random permutation of the numbers $1, 2, \ldots , n$ that assumes every permutation with the same probability $1/n!$, then

$$T_n = \Sigma_j \, b(j,R_j) \tag{1}$$

is asymptotically normal as $n \to \infty$ under simple conditions that were later proved to be minimal. This result has turned out to be the workhorse of the asymptotic theory of rank tests that often have statistics that are either of the form (1) or that can be approximated by such statistics.

Having contributed one of the major tools for studying of rank tests, it was time for Hoeffding to take part in this study. His first paper in this area (1951) was a major one. The appealing property of rank tests is that the probability of an error of the first kind (i.e., rejecting the null-hypothesis when it is true) is known and constant whenever the hypothesis is satisfied. However, the power of the test (i.e., the probability of rejecting the hypothesis if the alternative is true) is generally unmanageable even for parametric alternatives. Except in very simple cases it is not possible to construct a rank test that is most powerful for a specific parametric alternative. To get around this problem Hoeffding introduced the concept of a locally most powerful rank test (i.e., a rank test that is most powerful in a typically parametric arbitrarily small neighborhood of the null hypothesis). Hoeffding showed that in many cases such tests could be easily derived and often performed quite well in general. But the meaning of this result was much deeper than that. It later became clear that asymptotically as $n \to \infty$, these local alternatives are the only ones that are relevant for the asymptotic power of a test, as a consequence of which a direct link was established between locally most powerful and asymptotically optimal rank tests. The way to an asymptotic optimality theory was clearly mapped out, and Wassily would expound his ideas on asymptotic efficiency of tests three years later in a joint paper with Joan Rosenblatt (1954).

Permutation tests share the property of rank tests that the probability of an error of the first kind is constant whenever the hypothesis is satisfied. They are typically modeled after parametric tests performed conditionally on some aspect of the data that is irrelevant for the truth or falsehood of the hypothesis to be tested. With the techniques available at the time, analyzing the performance of nonparametric tests in general was not yet possible, but Hoeffding achieved the tour de force of showing that permutation tests were asymptotically as powerful in the parametric setting as the parametric tests on which they were modeled (1952).

There followed a remarkable series of papers concerning bounds on expected values of functions of sums of independent random variables, of which Hoeffding (1956) generated most attention. For independent random variables $X_1, \ldots, X_n$, let X_j assume the values 0 and 1 with probabilities p_j and $(1 - p_j)$, respectively, and let $\Sigma p_j = np$ for given $p \in (0,1)$. It is shown that according to several criteria, ΣX_j is most spread out if all $p_j = p$. In other words, the binomial distribution is the least concentrated among all Poisson-binomial distributions with the same expectation. This result has many intriguing ramifications.

Hoeffding (1963) established probability inequalities for sums of bounded random variables. This paper has been truly influential in many areas of statistics, including empirical process theory. Perhaps more surprisingly, it is one of the most cited papers in computer science literature; for example, it is used in algorithms simulating pinball machines and is associated with the term "a Hoeffding race." Hoeffding

(1965, 1967) was devoted to probabilities of large deviations and a related optimality property of likelihood ratio tests for multinomial distributions.

An extensive discussion of the full breadth and impact of Wassily's scientific contributions is given in four papers by his colleagues, which were included in his *Collected Works*.

WASSILY AS TEACHER

As with all other aspects of his professional life, Wassily approached his interactions with students with very careful thought. In his later years in the department his teaching was confined to advanced graduate courses,[6] of which he taught five: Estimation and Hypothesis Testing, Nonparametric Statistics, Sequential Analysis, Decision Theory, and Large-Sample Theory. For this level of student the courses were a delight: as elegantly and meticulously prepared as his research papers, always totally up-to-date, and above all designed to promote understanding and learning. Furthermore, his examinations were a revelation. He had the view that students taking his courses wanted to learn, and that it wasn't his job to seek to catch them out by probing for areas of confusion or lack of knowledge. Rather, his examinations, which took the form of one-week take-home projects, simply extended the learning process, by leading the student through an area of the subject not covered in the course. For example, on one occasion he chose not to cover *U*-statistics in presenting his Nonparametric Statistics course, but the take-home exam resulted in the students becoming familiar with the area. Of course, when he taught the course on another occasion, a different topic would be omitted.

One delightful classroom story was handed down from one generation of students to the next. There tended to be little dialogue in Wassily's lectures; it was mainly a question of copying the material that he wrote on the board in chalk.

However, legend had it that on one occasion a student interrupted him to ask, "Excuse me, Professor Hoeffding, but can you give us an example of this theorem, please?" and that Wassily thought briefly and then wrote on the board,

Example: set $\theta = \theta_0$.

Wassily supervised the Ph.D. theses of 17 students. As an advisor he was supportive but nonintrusive. Indeed, his natural modesty made the early phase of Ph.D. research somewhat daunting. When seeking advice about the current difficulty with which one was confronted, it was essential to end a description of the problem with a direct question. Otherwise (and typically) Wassily did not presume that his counsel was being sought and so would offer no comment.

LIFE OUTSIDE STATISTICS

Although not disposed to lengthy sojourns away from Chapel Hill, Wassily very much enjoyed the shorter trips that he made to other parts of the United States and of the world, usually in association with his work. An early Berkeley symposium provided him with the opportunity to meet statisticians and probabilists from the West Coast and from all over the world. He recorded his delight at the opportunity to go for a three-day hike in Yosemite Park with some colleagues. Walking with colleagues in the hills around Ithaca was a highlight in a later visit to Cornell.

Attendance at international meetings took him to Sydney, Warsaw, Stockholm, and other foreign parts; a six-month stint in India funded by UNESCO included one month of touring around the country, giving lectures in Benares, Lucknow, Delhi, Agra, Bombay, Bangalore, Mysore City, Trivandrum, and Madras. After India the National Academy of Sciences sponsored a one-month visit to Russia, where he visited Moscow, Leningrad, Kiev, Tashkent, and Novosibirsk. Subsequently, while attending a conference in Warsaw he was able to visit Vilnius.

> The visit to Akademgorodok, the seat of the Siberian section of the Academy of Sciences of the USSR, near Novosibirsk, was of special interest. Few Westerners had been there before me. I was warmly received by A. A. Borovkov and his co-workers, mostly younger people. I had arrived directly from Tashkent, where the flowers were in bloom in the public squares. Here, when we were walking on the ice-bound Ob River in a chilly wind, I put on the only head covering I had with me, an embroidered Uzbek scull cap which had been presented to me in Tashkent. (1982, p. 108)

Apart from Wassily's enjoyment of hiking, his longtime friend and colleague, Ross Leadbetter, records[7] that "operating his sailboat brought him much pleasure, even though as he liked to recall, he was once arrested by the coastguard out in Kerr Lake for the heinous crime of having nonstandard life jackets. A rather amusing train of correspondence ensued, which it appears the authorities tired of first, dropping the matter."

He was always well informed on world affairs and was deeply interested in cultural matters. Ildar Ibragimov wrote[1] to us of Wassily's interest in Russian literature, particularly Russian poetry. Wassily used to send Ibragimov Western editions (in Russian) of the Russian poets Akmatova and Gunilev, and Ibragimov subsequently sent him very good Russian editions of these poets. On one occasion when Wassily and his mother visited Tashkent, they were invited to a dinner. Someone proposed that they all toast their motherlands: the U.S.S.R. and the U.S.A. Wassily and his mother stood up and proclaimed the famous lines from Pushkin.

Nam celyi mir chuzhbina
Otechestvo nam Carskoe Selo

(For the whole world is a strange country,
Our motherland is Tsarskoe Selo)

Tsarskoe Selo was, of course, Wassily's hometown.

PROFESSIONAL HONORS

Wassily was the recipient of many of the highest honors available to statisticians, in the United States and overseas. These included

1967	Wald lecturer
1969	President, Institute of Mathematical Statistics
1973	Appointed Kenan Professor, University of North Carolina
1976	Elected a member of the National Academy of Sciences
1985	Elected to the American Academy of Arts and Sciences
	Elected a fellow of the American Statistical Association
	Elected a fellow of the Institute of Mathematical Statistics
	Elected a member of the International Statistical Institute
	Elected an honorary fellow of the Royal Statistical Society

Shortly after his retirement at the age of 65, the University of North Carolina's College of Arts and Sciences established the Wassily Hoeffding Professorship in his honor.

RETIREMENT, 1979-1991

In reflecting on his career, Wassily concluded that

> ever since I switched from economics to probability and statistics in my early student days, this area has continued to absorb my interests. The very idea that the seeming chaos of chance obeys mathematical laws is immensely attractive. It gives me great satisfaction to have made a few contributions to the understanding of this field.
>
> The successes I had did not come easy to me. They were the fruits of long hours of work which often led to dead ends. I am well aware that with advancing years my capacity to work has diminished. The lure of the subject persists. Whether I will contribute more to it, only time will tell. (1982, p. 108)

Just before his sixty-fifth birthday an international symposium was held in his honor.[8] Sadly, he was suddenly taken ill during the symposium dinner and was rushed to hospital, where "my right leg had to be amputated. (The reason was an infection related to my diabetes.) Since then, I have been getting used to a new kind of life" (1982, p. 108).

He remained active and alert. His new life still involved research, notably a number of typically elegant contributions to the *Encyclopedia of Statistical Sciences.* In describing Wassily's retirement activities Ross Leadbetter commented,[7]

> Those of us privileged to enjoy his friendship in later years must be struck by his extraordinary strength of character, his extreme generosity—almost to a fault—towards causes, and people expressing needs, and his amazing ability to contend with the seemingly endless medical complications, not too infrequently life threatening, and yet to get very significant enjoyment from life.
>
> His later life was burdened severely by the necessities of medical attention but he would make the fullest use of intervening time with avid reading, with writing made laborious by poor circulation in his fingers, watching TV and listening to his shortwave radio. He would scan the *Annals of Statistics* for any articles involving *U*-statistics. He was fond of Russian literature and poetry and was busy reading a new and extensive biography of Tolstoy during his recent stay in hospital. A *New York Times* reader without peer, he took endless delight in finding the unusual new items which appealed to a very real sense of humor that could surprise those only casually acquainted with him.
>
> He clearly enjoyed his later years in spite of the struggles and limitations. He enjoyed his team of nurses, his watchful doctor . . . his neighbors . . . his local friends and international department visitors who would want to go and see him. He kept his dignity to the end, and never gave any hint of self pity. As a basically theoretical person he gave many practical lessons to those about him, not the least of which was that real friendship is something beyond the clatter of small talk, and is better demonstrated by action rather than endless words.

Tributes from friends, colleagues, and even from those who had never met him, provide a clear and consistent picture of this remarkable human being. In a memorial session held at the Joint Annual Statistical Meetings in Boston in August 1991, Richard Vitale, who fell into this last category, said,

I never had the opportunity to meet Professor Hoeffding personally but am glad to have encountered him through his work. Several times in conducting a literature search, I found that he was the one who pioneered a certain problem area. He always did it with a penetrating understanding of the fundamental questions and elegant, lucid analysis. Or, to be more direct, he always seemed to understand and do a problem right, the way it *ought* to be done. He was for me, in this way, a gifted and generous teacher.

In the same session a tribute was read from his collaborator from his early days, Herbert Robbins.

Although he was gentle and courteous in manner and fragile in health, he was lion-hearted in spirit and completely original in his scientific work. His character was truly noble; I never heard from him a complaint about his chronic illness or the difficulties of living in an alien environment, or a disparaging remark about any fellow human being.

The Statistics Departments of Columbia and Rutgers join in expressing their sorrow at the death of Wassily Hoeffding, one of the great creative forces of statistical thought of our time. His work will long continue to provide an unequalled example of mathematical elegance and manifold application. His presence here from 1946 until his death has greatly enriched the intellectual and cultural life of this country.

The preface to Wassily's *Collected Works* concludes with the following words:

A few months before Wassily's death, we visited him to request permission to produce this book. He expressed surprise (and pleasure) that the enterprise might be considered worthwhile. Then he offered us a drink. We asked what was available, He thought for a few moments, trying to recall the name of the liqueur (Benedictine), then said, "Er . . . I forget. My memory is bad – but the liqueur is good." We shall treasure the opportunities we had to learn from Wassily and to appreciate his gentle humour.

We have been greatly assisted by many people in preparing this memoir. In particular, we would like to express our gratitude to Virginia Hoeffding; Stamatis Cambanis, Ross Leadbetter, the late June Maxwell, and Gordon Simons of the University of North Carolina; Ildar Ibragimov; and John Kimmel of Springer-Verlag for arranging permission to quote material from *The Collected Works of Wassily Hoeffding*.

NOTES

1. E-mail message from Ildar Ibragimov dated November 25, 1997.

2. *The Columbia Encyclopedia* (6th ed., 2001) records the following information: "Harald Høffding 1843-1931, Danish philosopher. He was professor at Copenhagen (1883-1915). His histories of philosophy have been enjoyed by a large audience, especially his *History of Modern Philosophy* (1894-95; tr., 2 vol., 1900, reprinted by Dover 1955)."

3. E-mail message from Oleg's daughter, Virginia Hoeffding, dated January 23, 2002: "Oleg spent the war in England, and wound up attached to U.S. Army Intelligence during the liberation of Europe; among other things, he was the first Allied intelligence officer to interview Albert Speer (he tends to appear in biographies of Speer as Captain "Otto" Hoeffding—much to his annoyance). He and my mother and brother came to this country in 1946 . . . the American intelligence community felt he was a valuable resource, and essentially sponsored his hiring by Columbia University as an instructor in the Economics Department . . . He left Columbia for the Rand Corporation when the latter was founded in 1953, and remained there until his retirement in the mid '70's. His field was Sino-Soviet economic relations, so that a great deal of his work was and remains classified. He was one of several people associated with Daniel Ellsberg during the Pentagon Papers episode, and was one of the co-signers of Ellsberg's famous letter to the *New York Times*. His opposition to the war was, I think, largely pragmatic rather than ideological; one of his papers which is in the public domain was an analysis of the ineffectiveness of strategic bombing."

4. N. I. Fisher and P. K. Sen, eds. *The Collected Works of Wassily Hoeffding*. New York: Springer, 1994.

5. Hanover became part of the British zone of occupation, and Wassily and his mother stayed there for more than a year, trying in vain to secure the release of his father. Then they lost all trace of

him. Some time after they had left Germany, Hoeffding *père* escaped from prison in Potsdam.

6. With one outstanding exception: On one occasion he was assigned (by some unknown and unfathomable process) to teach an introductory service course in statistics to a class of undergraduates comprising basketball players, footballers, and other assorted attendees, none of whom was attending UNC, Chapel Hill, with the primary intent of learning statistics. It is not clear who received the greater shock from the semester's experience: the students or Wassily.

7. Memorial service for Wassily Hoeffding, March 7, 1991, Person Recital Hall, University of North Carolina, Chapel Hill.

8. I. M. Chakravarti, ed. *Asymptotic Theory of Statistical Tests and Estimation*. New York: Academic Press, 1980.

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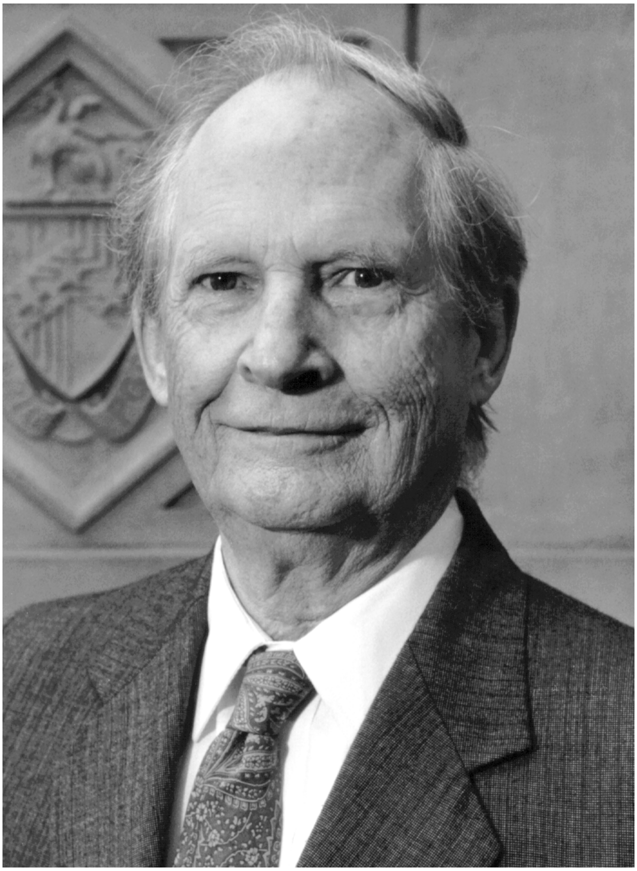

D. GALE JOHNSON

July 10, 1916–April 13, 2003

BY VERNON W. RUTTAN, JAMES J. HECKMAN, AND G. EDWARD SCHUH

D. GALE JOHNSON was a scholar of exceptional breadth who made original and important contributions to economics and to public policy. In his early work he contributed to our knowledge of the sources of instability in national and international commodity markets. In his later work he addressed the sources and consequences of the failure of socialized agriculture in the former Soviet Union and in China.

Gale was born on July 10, 1916. His parents, Albert and Myra, operated a farm near Vinton, Iowa. He attended grade school at a one-room country school near his parents' farm and high school in the town of Vinton. His family experienced the effects of the instability in agricultural prices and income that characterized the post-World War I era: the boom of the 1920s and the Great Depression of the 1930s. Gale received his undergraduate degree from Iowa State College in 1938. He married Helen Wallace on August 10, 1938. They had two children, David Wallace and Kay Ann. He received an M.S degree from the University of Wisconsin in 1939 and a Ph.D. degree from Iowa State College in 1945. His graduate study also included two years at the University of Chicago in 1939-1941.

Gale Johnson's interest in agricultural economics began when he was still in high school, when he participated in an agricultural policy debate contest sponsored by the Future Farmers of America. In preparation for the debate he wrote to Professor Theodore W. Schultz at Iowa State College asking about resource materials. Schultz (who later became the chairman of the Economics Department at the University of Chicago, a member of the National Academy of Sciences, and a Nobel Prize laureate) responded by sending him two books and an extended letter. Alan Klein, then president of the Iowa Farm Bureau, was so impressed with his presentation in the debates that he invited Gale to accompany him and participate in debates on agricultural policy at a series of meetings throughout Iowa.

Gale Johnson began his professional career as a research assistant at Iowa State in 1941. He was promoted to assistant professor in 1942, before receiving his Ph.D. He moved to the University of Chicago as assistant professor in 1944. He was promoted to associate professor in 1949 and to the rank of professor in 1952, the youngest ever to have achieved this rank in the Chicago Economics Department. In 1970 he was appointed the Eliakim Hastings Moore Distinguished Service Professor.

Gale Johnson's transfer from Iowa State University to the University of Chicago was the result of an incident involving academic freedom at Iowa State that became known as the "Iowa Margarine Incident."[1] At the beginning of World War II Iowa State University received a grant from the Rockefeller Foundation to prepare a series of monographs on how to enhance the contribution of agriculture to the war effort. One of these monographs, prepared by O. H. Brownlee, a fellow graduate student, demonstrated that a shift from butter to margarine consumption (made from soybean oil) would result in significant resource savings.

The monograph recommended that the taxes and other restraints on the sale of margarine be removed. Iowa dairy interests demanded that the monograph be revised or withdrawn. Gale had prepared several important, though less controversial, monographs in the same series. When the president of Iowa State acceded to the dairy industry demands, Johnson's mentor Theodore W. Schultz, then chairman of the Iowa State Economics Department, resigned and accepted an appointment as professor in the Economics Department at the University of Chicago. Schultz arranged a research associate appointment for Gale Johnson at the University of Chicago.

Gale Johnson's contributions to the University of Chicago were enormous. He served as associate dean (1957-1960) and dean (1960-1970) of the Division of Social Sciences. He served as chairman of the Economics Department during 1971-1975 and again during 1980-1984. He held the positions of vice-provost and dean of faculties in 1975 and provost during 1976-1980.

While still an assistant professor, Johnson initiated the graduate research workshop in agricultural economics. The workshop met weekly. Graduate students were expected to present reports on the progress and results of their research two or three times while working on their theses. Faculty and visiting scholars also made presentations at the workshops. The consistent participation of Schultz, Johnson, and other faculty assured vigorous discussion. For more than three decades the workshop was an incubator for a series of leading scholars in the field of agricultural and development economics. It served as a model for other workshops in the Chicago Economics Department and at other universities. While chairman of the Economics Department, he substantially strengthened an already leading department by recruiting outstanding scholars into a department that has produced 22 of 49 Nobel laureates in economics.

Gale Johnson has also been of great service to both the academic community and the broader society. He has received numerous honors, including election as a fellow of the American Agricultural Economics Association and the American Academy of Arts and Sciences and to membership in the National Academy of Sciences. He served as consultant and advisor to the Tennessee Valley Authority (1950-1955), the National Advisory Committee on Food and fiber (1965-1967), the National Commission on Population Growth and the American Future (in 1972), and on several National Research Council study committees. He served as editor of the leading interdisciplinary journal in the field of development, *Economic Development and Cultural Change* from 1985 to 2003.

Gale Johnson has made a series of important scientific and professional contributions that have changed the way economists think about economic instability and economic development. His initial contributions focused on the impact of price instability on the efficiency of resource allocation and use in agriculture. The importance of this work can only be understood in the context of the unstable economic environment of the 1920s and 1930s and the attempts to develop policies to reduce the effects of price and income instability in agriculture. Johnson's research demonstrated the importance of separating analytically the use of commodity price policy as an effective means of addressing efficiency problems associated with price distortions and instability from the use of price policy to transfer income to farm people to address the poverty problem in U.S. agriculture. His first article on this issue, "Contribution of Price Policy to Income and Resource Problems in Agriculture," published in 1944 had an immediate impact on economic thought concerning post-World War II agricultural policy. His second article on the same topic, "A Price Policy for Agriculture, Consistent with Economic Progress That Will Promote an Adequate and More Stable Income from Farming" (1945), was awarded second prize in an American Agricultural Economics Association competition.

Johnson brought the analysis and the policy implications of his work on price instability together in 1947 in a widely acclaimed book, *Forward Prices for Agriculture.* In this book he showed for the first time how farmers form expectations to guide production decisions, how these expectations contribute to instability in agricultural commodity markets, and how agricultural commodity policies could be designed to address these problems.[2] By the early 1930s Johnson was recognized as one of the most innovative and influential scholars in the field of agricultural policy.

Gale insisted that the income problems of agriculture were rooted in disequilibrium in intersector factor markets. This view was articulated most rigorously in an article, "Nature of the Supply Function for Agricultural Products," published in the *American Economic Review* in 1950, in which he located the failure of agricultural production to adjust to secular growth in agricultural productivity, to instability in commodity prices, and to rigidities in agricultural labor markets.[3] The implications for the reform of land tenure institutions were analyzed in a 1950 study, "Resource Allocation Under Share Contracts." This series of studies led Gale Johnson, together with Schultz, to focus major effort on the adjustment problem of Southern agriculture, particularly issues of labor productivity and income distribution. His innovative article on the "Functional Distribution of Income in the United States" (1954) stimulated an important body of theoretical and empirical research.[4]

By the early 1950s Gale was beginning to give considerable attention to international trade in agricultural commodities. This research culminated in an influential book published in 1973, *World Agriculture in Disarray.* The same insights on the behavior of agricultural commodity and factor markets that he had developed in his studies of U.S. agricultural policy provided powerful insight into the effects of government interventions in agricultural commodity markets on distortions in agricultural resource use and commodity trade at the global level.

In a brief 2003 biographical statement appended to his list of publications Johnson summarized the perspective that had emerged from his work in the area of factor and product markets.

> Among the major themes I have emphasized are that output prices have little or no effect on the returns to mobile resources engaged in farming (capital and labor), that it is through the factor markets that returns between farming and other sectors of the economy are equalized, that share renting as actually practiced is efficient. The primary effect of subsidy programs for agriculture, such as higher prices, is to increase the returns to and price of land and to expand agricultural output and to induce governments to interfere with international trade.

During the 1980s and 1990s, issues of agricultural development in the centrally planned economies of the Soviet Union and China occupied a good deal of Johnson's attention. His interest in Soviet agriculture was initially stimulated by an extensive visit to the Soviet Union in 1955 as a member of a U.S. agricultural delegation. His interest in China was stimulated by his daughter, Kay Ann Johnson, a China scholar, who took Gale along with his longtime colleague, Theodore W. Schultz, to visit a Chinese village for the first time in 1980. In the 1980s and early 1990s Gale made frequent visits to China. In his visits to both the Soviet Union and China, Gale always insisted on visiting farms and villages. His experience growing up on an Iowa farm in the 1920s and 1930s gave him a strong empathy with rural people. In one of the many papers and articles that he wrote on agriculture in the centrally planned economies, he insisted that socialized agriculture had been associated with more human suffering than any human institution other than slavery and war.

In the early 1970s, service on the National Commission on Population Growth and the American Future turned Johnson's attention to the relationship between population growth and economic development. In 1987 he edited with Ronald Lee a highly influential book, *Population Growth and Economic Development*. The book, which drew attention to what the editors interpreted as an easing of the pressure of population against the world food supply, was both controversial and influential in shaping the direction of population research and policy. Although not opposed to public support of family planning, Gale was highly uncritical of Chinese government attempts to regulate family size.

In a series of papers beginning in the mid-1990s Johnson was among the first to question the alarmist projections of a food crisis in China during the early twenty-first century. He addressed the food-population issue more broadly in a masterful presidential address to the American Economic Association on "Population, Food and Knowledge" (2000) and again in his last major article, "Globalization: What It Is and Who Benefits" (2002).

A continuing concern of Gale Johnson's research was the role of intersector and international factor and product markets in the process of economic development. He continued to advance our knowledge and to insist on the importance of advancing knowledge, up until the last months of his life. "Not only are people better fed than ever before but they also acquire their food at the lowest cost in all history. . . . The greatest achievement of the twentieth century is that the majority of the poor people of the world have shared in the improvements in well being made possible by the advancement of knowledge" (2000, pp. 12-13).

IN PREPARING this memoir we benefited from correspondence with Kay Ann Johnson and have drawn on the following articles: G. E. Schuh. A tribute to D. Gale Johnson. In *Papers in Honor of D. Gale Johnson,* eds. J. M. Antle and D. W. Sumner, pp. 30-43. Chicago: University of Chicago Press, 1997. D. M. Hoover and P. R. Johnson. D. Gale Johnson's contribution to agricultural economics. In *Papers in Honor of D. Gale Johnson,* eds. J. M. Antle and D. A. Sumner, pp. 30-43. Chicago: University of Chicago Press, 1997. J. M. Antle, B. L. Gardner, and D. Sumner. Contributions of D. Gale Johnson to the economics of agriculture. *Economic Development and Cultural Change,* in press.

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Robert T. Jones

ROBERT THOMAS JONES

May 28, 1910–August 11, 1999

BY WALTER G. VINCENTI

THE PLANFORM OF THE wing of every high-speed transport one sees flying overhead embodies R. T. Jones's idea of sweepback for transonic and supersonic flight. This idea, of which Jones was one of two independent discoverers, was described by the late William Sears, a distinguished aerodynamicist who was a member of the National Academy of Sciences, as "certainly one of the most important discoveries in the history of aerodynamics." It and other achievements qualify Jones as among the premier theoretical aerodynamicists of the twentieth century. And this by a remarkable man whose only college degree was an honorary doctorate.

Robert Thomas Jones—"R.T." to those of us fortunate enough to be his friend—was born on May 28, 1910, in the farming-country town of Macon, Missouri, and died on August 11, 1999, at age 89, at his home in Los Altos Hills, California. His immigrant grandfather, Robert N. Jones, after being in the gold rush to California, settled near Macon, where he farmed in the summer and mined coal in the winter. His father, Edward S. Jones, educated himself in the law and practiced law in Macon; while running for public office, he traveled the dirt roads of Macon county in a buggy behind a single horse. R.T. later contrasted this with his own experience flying nonstop from London to San Francisco over the polar regions behind engines of 50,000 horsepower.

Writing later about his days in Macon High School, R.T. paid tribute to "a wonderful mathematics teacher, Iva S. Butler, who took us along the intricate path through exponents, logarithms, and trigonometry." Like most boys of his time, he and his friends strung wires from the house to the barn, wound coils on oat boxes, obtained Model T spark coils from junkyards, and made spark-gap transmitters. With these, he said, "We showered the ether with noisy dot-dash signals that could be heard clear across the country and beyond." "My consuming interest," R.T. wrote, "however was aviation." He built rubber-band-powered model airplanes from the kits of the Ideal Model Airplane Supply Company and "devoured eagerly" the technical articles in aeronautical magazines and the research reports from the National Advisory Committee for Aeronautics (NACA), little suspecting that for most of his life it (and its successor, NASA) would be his employer.

Following high school, R.T. attended the University of Missouri, but found it unsatisfying and dropped out after one year. Returning to Macon, he joined the locally based Marie Meyer Flying Circus, a stunt flying group typical of the time. As an employee of the circus he received flying lessons in exchange "for carrying gas and patching wing tips," though he would not solo until more than 50 years later. He never would return to the university.

In 1929 the fledgling Nicholas-Beazley Airplane Company in the nearby town of Marshall found itself without its one engineer. One of the owners of the flying circus, aware of R.T.'s self-education with the NACA reports, recommended R.T. for the job. As R.T. wrote, "I was hired immediately; 19 years old, a college dropout, and chief (or only) engineer at a salary of 15 dollars a week." Later the company hired an experienced engineer who taught R.T. about airplane design, especially stress analysis. R.T.'s job concerned mainly the production of the Barling NB3, a new type of three-place, low-wing, all-metal monoplane, and he also "worked from early morning until midnight" on the design of a small racing plane for the 1930 air races. These experiences apparently fostered his ambition to become a skilled engineer. Nicholas-Beazley was successful for a time, but in the early 1930s in the Great Depression the company, like many others, went out of business.

R.T. returned home to Macon, where he used his time to study books on aerodynamics, such as Max Munk's *Fundamentals of Fluid Mechanics for Aircraft Designers*. Needing work, he obtained a ride with neighbors to Washington, D.C., where his local congressman provided him with a "wonderful" job as an elevator operator in the House Office Building. With typical dry humor, he wrote that the "ups and downs of this job" gave him an opportunity to observe the inner workings of the government. Realizing that he would need to know considerable mathematics to be a successful engineer, R.T. used his spare time in the nearby Library of Congress studying original works on various mathematical topics. He also struck up an acquaintance with A. F. Zahm, a well-known aerodynamicist in charge of the library's aeronautics collection and an ex-member of the NACA. One day a Maryland congressman, David J. Lewis, who also knew Zahm, got on R.T.'s elevator and asked whether R.T. would tutor him in mathematics. Congressman Lewis was then 65 years old and completely self-taught, with no formal education of any kind. R.T. brought him through algebra and up to calculus, "learning a lot from him on the way."

After arriving in Washington, R.T. also attended graduate evening classes in aerodynamics at Catholic University from the brilliantly creative but difficult Max M. Munk. Munk had received his doctorate in aerodynamics with the great Ludwig Prandtl at Göttingen and worked for the NACA at its laboratory at Langley Field. When told that R.T. had studied his book on fluid mechanics, Munk suggested that R.T. take his classes. R.T. did so for three years. They would have profound influence on his later achievements.

In 1934 the new Public Works program, started by President Roosevelt to help combat the Depression, made available a number of nine-month positions as a scientific aide at the NACA's Langley Aeronautical Laboratory in Virginia. With the recommendations of Zahm, Munk, and Congressman Lewis, R.T. obtained one of these positions. By the time his nine months were up, his exceptional talents had become apparent, and his supervisors retained him at subprofessional levels by temporary and emergency reappointments. A permanent professional appointment as an engineer at the initial civil-service grade, however, required a bachelor's degree. A professional appointment thus seemed impossible until someone noticed that the next higher grade, usually attained by promotion from the initial grade, had no such stated requirement. In 1936 he was therefore promoted directly to that level and became officially an engineer. Thus began R.T.'s career with NACA and its successor NASA, which, except for a period in the 1960s, would occupy him until his retirement in 1982 from the Ames Research Center in California.

R.T.'s work in his first 10 years with NACA dealt mostly with airplane stability and control, in which he became a recognized authority. In the process his lack of knowledge of applied mathematics rapidly disappeared. He quickly became a pioneer in the application of Oliver Heaviside's operational methods in the theoretical analysis of the transient motion of airplanes following a transient disturbance. In this he introduced some perceptive, ingenious, and mathematically sophisticated procedures. By 1944 he published, alone or occasionally in collaboration, about 20 reports on stability and control, mostly theoretical but some involving discussion of related wind-tunnel and flight results. One of these was an exhaustive résumé and analysis of NACA lateral-control research written in collaboration with Fred Weick, assistant chief of aerodynamics at Langley (1937). Weick also asked R.T. to see whether he could design a satisfactorily maneuvering airplane with simplified controls on the assumption that it shouldn't require both hands and feet to move them. R.T.'s analysis showed that two-control operation would best be achieved by controlling the ailerons instead of the rudder, preferably with a small amount of rudder movement linked directly to the aileron motion. After leaving Langley, Weick used the concept to design the famously successful two-place low-wing Ercoupe, which went into production in 1940.

In his first 10 years with the NACA only three of R.T.'s publications had to do with aerodynamics—in those days low-speed (i.e., incompressible) flow. In the mid-1940s that would quickly change. He would spend the remainder of his career mostly in high-speed (i.e., compressible) aerodynamics, coming up at the outset with a fundamental concept for the aerodynamic design of aircraft to fly at supersonic or high subsonic speeds.

This idea originated in 1944 in the course of a wartime assignment to help develop guided missiles. It derived from the design by the Ludington-Griswold Company of a dart-shaped glide bomb having a wing of narrow triangular plan-form. The company's engineers had calculated the aerodynamic lift of the wing using the usual lifting-line theory; they had misgivings because the theory was devised for wings wide relative to the flight direction. In a visit to Langley the president of the company asked R.T. whether he could think of a better way to calculate the characteristics of a long and narrow triangular wing, pointed in the direction of flight. R.T. remembered a paper from 1924 "by my teacher Max Munk" in which the forces on a long, narrow body of revolution (an airship hull) at angle of attack had been analyzed on the assumption that the flow in planes *perpendicular* to the flight direction could be treated as two-dimensional. With this assumption, the only novelty for the flat wing, though far from a trivial one, was how to satisfy the necessary condition at the trailing edge of a wing (i.e., the condition that there be no flow around the sharp edge). R.T. devised a way to do this, and the rest was relatively simple. So simple, in fact, that he thought "nobody would be interested in it," put the analysis in his desk drawer, and temporarily forgot about it.

R.T.'s slender-wing analysis, like the lifting-line theory, used the linear equations of incompressible flow. A few months later while exploring the more complex nonlinear equations of compressible flow, he realized that introducing the approximation of the long, narrow wing gave him the same results as his incompressible analysis. This result implied that for slender wings there was no effect of compressibility, that is, no effect of Mach number (the Mach number being the speed of flight divided by the speed of sound, a measure of compressibility). In particular, there was none of the undesirably large increase in drag characteristic of straight wide-span wings at flight speeds approaching and exceeding the speed of sound.

In trying to understand physically why the slender wing should show such Mach-number independence, R.T. wondered whether it might have to do with the large sweepback of the wing's leading edge. Again he remembered another paper by Max Munk, this one dealing with the effect of dihedral and sweepback on the performance of wings at low speeds. In it Munk assumed that the air forces on a swept wing of large span and constant chord depend only on the component of flight velocity perpendicular to the leading edge and are independent of the component parallel to it. R.T. wondered whether this independence principle might also apply to the components of the flight Mach number (i.e., in high-speed flight) and decided that it did. Thus, the effective Mach number, on which the air forces depend, decreases continuously with increasing sweep; it follows that even at supersonic flight speeds the air forces can be made to have the advantageous properties found at low subsonic Mach numbers simply by introducing sufficient sweepback, in particular, that the enormously increased drag of conventional unswept wings at supersonic speeds can be reduced to subsonic levels. R.T. thus discovered the theory of high-speed sweepback, which William Sears described as "certainly one of the most important discoveries in the history of aerodynamics."

With his new insights R.T. quickly resurrected his incompressible slender-wing analysis, modified it to start from the compressible-flow equations, and added his reasoning about sweepback. The resulting report was then submitted in early 1945 to the customary editorial committee, chaired in this case by Langley's top senior theoretician. To R.T.'s surprise the committee accepted his special slender-wing theory—which was published as a separate unrestricted NACA Report dated May 1945 at Langley Field (1946,1)—but rejected his general finding about sweep. Subsonic and supersonic flow were conceived at that time as of entirely different nature, and the committee chairman could not accept that an essentially subsonic result could be obtained in a supersonic free stream. NACA management therefore held up publication of the sweep theory until transonic experiments, conducted at R.T.'s suggestion, showed 45-degree swept wings to have much less drag than straight wings. The sweep analysis appeared in circulation-restricted reports later in 1945 and as an unrestricted report in 1946 (1946,2). R.T.'s reports on slender wings and sweep are among the most consequential in the history of aerodynamics.

At the time of R.T.'s work no one in the United States appears to have been aware that the eminent German aerodynamicist Adolf Busemann had used the independence principle to examine the theoretical high-speed possibilities of sweep as one of a number of topics in his lecture to the Volta Congress in Rome in 1935. His idea received little notice perhaps because Busemann's thinking considered only supersonic flight speeds and sweep angles for which the effective Mach number remained supersonic—speeds that seemed far beyond practical attainment at that time. In early May 1945 while the events at Langley were taking place, a group of U.S. engineers investigating German wartime research came upon a large collection of unpublished swept-wing data from high-subsonic-speed wind tunnels at Busemann's institute at Braunschweig. R.T.'s idea of high-speed sweepback occurred independently of German thinking, and he and Busemann are credited jointly with the concept.

In early 1946 R.T. transferred from Langley to the NACA's Ames Aeronautical Laboratory south of San Francisco. It was there that I came to know him, when we spent hours together in the next few years laying out what we hoped would be optimum swept-wing configurations for testing in Ames's first supersonic wind tunnel. Except for seven years in the 1960s when he went elsewhere and worked outside aeronautics (as described later), R.T. was employed at Ames until his formal retirement in 1981. After that he served until 1997 as a consulting professor in the nearby Department of Aeronautics and Astronautics at Stanford University, at the same time maintaining a close, informal relationship with Ames.

R.T.'s work at Ames and later at Stanford dealt mostly with sweepback. His nonsweep concerns, however, also have fundamental importance. One of these dealt with a basic but previously unnoticed mathematical singularity that occurs when thin-airfoil theory is applied to an airfoil having a rounded leading edge (1950). Another put the "area rule" for flight speeds near the speed of sound, which transforms the pressure-drag problem for a wing-body combination into that for an equivalent body of revolution, on a firm theoretical foundation and extended it to supersonic flight speeds (1956,1). Both these topics appear in any complete text on aerodynamic theory.

R.T.'s dealings with sweep after his move West show a wide range of concerns. For the first 10 or so years his papers focused mostly on the theory, elaborating its foundations and techniques and exploring its results (1947, 1951, 1952). In 1957 he and Doris Cohen incorporated these and the findings of others into an important 241-page section with the title "Aerodynamics of Wings at High Speeds" in the Princeton series *High Speed Aerodynamics and Jet Propulsion* (1957,2).

In the second half of the 1950s R.T.'s concerns began to shift from sweep theory itself to the implications of sweep and other theoretical findings for the design of airplanes for high-subsonic and supersonic flight. He voiced his ideas especially in papers at several meetings (1955; 1956,2; 1959). In several of his writings in this period and earlier, including the section in the Princeton series and the papers from the meetings, he mentioned the idea of an oblique (or yawed) wing, though mainly as a matter of theoretical interest rather than a practical configuration. Following his return to Ames from an absence in the 1960s this changed, and he devoted himself wholeheartedly to the startlingly unconventional concept of the oblique wing and its potential for a high-performance airplane.

The planform of a conventional swept wing, as we see it flying overhead, has bilateral (mirror) symmetry, that is, it is swept back on both sides of the fuselage (or plane of symmetry). Sweep can equally well be embodied in a wing that is swept back on one side and forward on the other, that is, a wing that is oblique to the line of flight. R.T.'s theoretical work showed, moreover, that such an oblique wing would have superior aerodynamic performance at high speed to a swept wing of conventional planform. Because both the conventional and the oblique wing present problems at the much-reduced speed of landing, there may be virtue in having a wing adjustable at landing to the zero sweep of low-speed aircraft. Such adjustment is mechanically and structurally easier for an oblique wing with a single pivot atop the fuselage than for a conventional swept wing with its required pair of pivots, one on each side. Along with its potential aerodynamic and mechanical virtues, the oblique wing raised questions about stability and control and about aeroelastic deformation and hence structural design.

Whether the concept of the oblique wing was original with R.T. is not clear. The idea was current in the sweep developments in Germany during the war. Evidence exists of stability-and-control tests, under R.T.'s inspiration, of an oblique-wing airplane model in the Langley low-speed free-flight wind tunnel in 1946, but whether he had heard of the German work we do not know. The idea was and still is startling because, as R.T. wrote, "Artifacts created by humans show a nearly irresistible tendency for bilateral symmetry."

R.T. and people associated with him produced a considerable body of work, mostly in the 1970s, on problems of the oblique-wing airplane. This included (1) transonic wind-tunnel tests that showed clear drag superiority of the oblique wing over a conventional sweptback wing; (2) comparative design studies, one under contract with the Boeing Company, giving careful consideration to stability-and-control and aeroelastic problems as well as aerodynamics; (3) low-speed flight tests of radio-controlled models (designed and built by R.T.) for which the sweep angle of the pivoted wing could be adjusted in flight; and (4) low-speed tests of the control response and pilot feel of a full-scale single-seat aircraft at the NASA Dryden Flight Research Center in southern California.

This work concerned an oblique wing with a fuselage and tail. R.T.'s interests following his formal retirement at Ames centered on the even more startling concept of the oblique flying wing. This consists simply of an oblique wing without fuselage or tail and large enough to carry its load internally. Here the variable sweep must be achieved by aerodynamic means, and the wing-mounted engines must be pivoted so they can be pointed in the direction of flight. The concept had been in R.T.'s mind for some years; he included it in his discussions of sweep in his papers at meetings. This he accompanied with a striking demonstration of the low-speed stability of an oblique flying wing by means of a balsa-wood glider.

R.T.'s work on the flying wing took place mainly in the late 1980s and early 1990s, much of it in his association with Stanford. Mostly it was as an advisor to people at Ames and other local research groups and to Professor Ilan Kroo and his doctoral students at Stanford. Kroo and his students, with R.T.'s inspiration and help, studied the aerodynamic-design and control-system problems in detail, using a radio-controlled model, in order to become familiar with the low-speed flight characteristics. Kroo and engineers at local research companies did a comprehensive design layout to examine the packaging within the aircraft of the payload, fuel tanks, retracted landing gear, and other internal components and their strong coupling to the exterior aerodynamic geometry. The resulting Mach 1.6 aircraft would accommodate 440 passengers inside a wing with a span of 400 feet.

Thanks to R.T.'s impetus and vision, there now exists a large body of practical knowledge of possible oblique-wing airplanes in both the pivoted and flying-wing versions. Sears, writing in 1976 with regard to the pivoted wing, said, "I, for one, fully expect to see future transport airplanes with 'Jones oblique wings.'" Though aircraft companies have studied the possibilities, what Sears expected has not, for a complex of reasons, come to pass with either version. The crystal ball for the future is unclear.

As the foregoing materials suggest, R.T.'s interests ranged widely. Within aerodynamics itself his concerns went far beyond the problems discussed above. This range appears in papers shortly before and after his retirement from Ames on such miscellaneous topics as the motion of ultralight aircraft in vertical gusts, the dive recovery of hang gliders, the aerodynamics of flapping wings, and the efficiency of small transport aircraft. And in his previously mentioned absence from Ames from 1963 to 1970 he worked in a field of fluid motion far removed from aerodynamics. This he did at the Avco Everett Research Laboratory in Massachusetts, where he chaired the laboratory's Medical Research Committee. In this work he studied the characteristics of blood flow in the human body and application of such knowledge to the design of cardiac-assist devices and development of one of the earliest artificial hearts. These efforts led to a number of articles in medical and biomechanical publications (e.g., 1970). His studies also ranged far outside fluid mechanics. Pieces by him appeared at various times in physical journals under such titles as "Analysis of Accelerated Motion in the Theory of Relativity" (1960) and "Relativistic Kinematics of Motions Faster Than Light" (1982).

Those who worked with R.T. marveled at how he arrived at his ideas, seemingly intuitively and frequently in terms of physical models and analogies. He could use highly sophisticated mathematics deductively when necessary, but he did so mostly to support his ideas and explore their consequences. In the initial report on his concept of sweepback he began conventionally with a mathematical derivation followed by three physical arguments and explanations. Events at the time suggest that the mathematics actually came to R.T.'s mind after the physical concepts and had been put into the report in response to editorial-committee objections. Whatever the situation, the fact is that things that seemed clear and obvious to him in his physical explanations often caused the rest of us difficulty and struggle to master. As Sears wrote, "Lesser aerodynamicists often find his arguments too concise and the literature of the field includes papers in which authors re-do Bob's work providing longer proofs, and discover again Bob's results."

As part of his association with Stanford, R.T. offered an occasional quarter-long lecture course on problems in aerodynamic theory. Connected with this, he published in 1990 an exceptional book entitled simply *Wing Theory*. Sears, on the flap of the dust jacket, calls it "surely . . . one of the most important books on aerodynamics written in our time." In 200 pages with numerous figures and frequent comparison with experiment, R.T. focuses on the basic principles and principal findings of the theory at both subsonic and supersonic speeds. To do so he uses a minimum of mathematics and a great deal of the intuitive physical thinking characteristic of his creative work. It is a book that only R.T. could have written.

As his construction of radio-controlled flying models of pivoted-wing airplanes might suggest, R.T. also had a talent for craftsmanship. In his spare time in the 1950s he devised and constructed (grinding the mirrors himself) an improvement on a type of reflecting telescope and published a number of related articles (e.g., 1957,1). In 1957 he and his wife formed the Vega Instrument Company, which made and sold some 40 six- and eight-inch telescopes of this kind.

In the 1950s, as well, R.T.'s daughter Patty reached the point in her violin studies where she needed a better but discouragingly expensive instrument. With characteristic resourcefulness R.T. undertook to make her one. After putting together equipment for electronic acoustic testing, he made a first attempt that turned out to be disappointing. His second effort was a notable success. Patty has since played it in recitals and as a member of the La Jolla Civic Symphony. During the years R.T. built more than a dozen fine violins and violas.

R.T. also had a passion for airplanes that became unmistakably visible in the mid-1980s (R.T. was in his mid-seventies). It was then that he obtained a pilot's license and bought the two-place Ercoupe mentioned earlier. He also went frequently (though not in his Ercoupe) to the annual Experimental Airplane Association Fly-In at Oshkosh, Wisconsin, where he gave occasional talks on airplane aerodynamics.

Besides his interest in telescopes, violins, and airplanes, R.T. read widely and thought seriously about human affairs. This is exemplified by an extraordinary piece entitled "The Idea of Progress" that he contributed to the journal of the American Institute of Aeronautics and Astronautics on the fiftieth anniversary of the institute (1981). In it he recounts something of the advances in his lifetime of technology plus the concurrent increases in food production and living standards and the related and troubling increase in population and appearance of the nuclear bomb. He cites Gerard O'Neill as telling how a scientist brought to life from 200 years ago would be completely bewildered by what he saw, whereas a politician would recognize perfectly well the kind of thing that was going on in politics and the general conduct of human affairs. In the discussion R.T. mentions or draws upon the ideas and writings of such a diverse group as (in the order in which he refers to them) R. A. Millikan, Frederick Soddy, Malthus, Hendrick Willem van Loon, Jay Forrester, Paul Ehrlich, Marx, J. B. Bury, Giordano Bruno, Gerard O'Neill, Voltaire, Machiavelli, and Max Born. He ends with the conclusion that "the idea of progress, in the form responsible for the revolution in science, must somehow find its way into political thought." The mainly technical audience of the institute must have been startled by what they encountered.

R.T. was elected to both U.S. national academies: the National Academy of Engineering in 1973 and the National Academy of Sciences in 1981. His many other honors included the Sylvanus Albert Reed Award of the Institute of the Aeronautical Sciences in 1946, the Prandtl Ring of the Deutsch Gesellschaft für Luft und Raumfahrt in 1978, the Langley Medal of the Smithsonian Institution in 1981 (an award shared with such aviation notables as the Wright brothers and Charles Lindbergh), and the Fluid Dynamics Prize of the American Physical Society in 1986. His lone college degree—the honorary doctorate mentioned earlier—came from the University of Colorado in 1971.

I cannot think of a more fitting way to close than to repeat what I have written elsewhere: R.T.'s friends knew him as a modest, considerate person of absolute integrity.

According to Ilan Kroo of Stanford, "Those of us privileged to call him a colleague . . . were continually surprised and inspired by this maverick scientist who contributed so much to our understanding of flight. In addition to his well-known technical contributions . . . he captivated a generation of students with fresh insights and new ways of looking at problems ranging from hang-glider dynamics and optimal bird flapping to supersonic aircraft." Most important for his various activities, he seemed to have a quiet confidence that he could accomplish whatever he set out to do—even if it was to make a fine violin. We do not see his like very often.

THIS IS A CONSIDERABLY shortened and revised version of a piece I wrote for the *Annual Review of Fluid Mechanics* (vol. 37, 2005); passages are reproduced here with the permission of Annual Reviews, Palo Alto, California. In preparing the work I have relied upon the writings of Jones, John D. Anderson, Jr., James R. Hansen, and William R. Sears as listed under References below, together with various unpublished patent-related materials. I have also drawn on my own and others' memories, especially for R.T.'s later career at Ames and Stanford, and on studying a number of R.T.'s technical reports listed under Selected Bibliography below and cited in the text.

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Norman C Rasmussen

NORMAN CARL RASMUSSEN

November 12, 1927–July 18, 2003

BY KENT F. HANSEN

NORMAN CARL RASMUSSEN died on July 18, 2003, at the age of 75. He succumbed to complications of Parkinson's disease from which he suffered for many years. Norm was a remarkable scientist, engineer, and educator who made additions to nuclear physics, nuclear engineering, health physics, and risk analysis. In each of these fields he was a creative researcher who made important, lasting contributions. He first achieved recognition for his work in gamma ray spectroscopy and the quantitative determination of the nuclear composition of materials. Subsequently he worked on the analysis of radiation doses in survivors of the U.S. nuclear weapons testing programs of the 1950s and 1960s. His most influential work was in directing the Atomic Energy Commission study on nuclear safety, published as WASH 1400 but better known as the Rasmussen Report. This pioneering effort has evolved into the principle tool of risk assessment in the nuclear industry. His public service included the National Science Board, numerous National Academy of Sciences panels, and the Defense Science Board. However, to those of us privileged to know him well, our sense of loss is dominated by the loss of a wonderful colleague and friend who possessed a rich collection of delightful human characteristics.

EARLY YEARS

Norm was born on November 12, 1927, in Harrisburg, Pennsylvania. He grew up on a dairy farm as the fifth of six brothers. He attended public schools in the Hershey, Pennsylvania, school system. Besides being a student he had the multiple chores of a farm boy, an experience that greatly influenced his career. As a farm boy in the depths of the Great Depression he had to learn how to care for animals, how to service and maintain farm equipment, and how to build or repair farm buildings and facilities. The result was that he became very proficient in using his hands, and very motivated to use his intelligence. Finally, the experiences of his youth gave him a lifelong habit of hard work. His father died when Norm was in the eighth grade, and the family moved to near Gettysburg, where his grandparents helped in caring for the family. He graduated from high school in June 1945 and enlisted in the Navy. He was sent to the Great Lakes Naval training school, where he became an electronics technician. He served on active duty until August 1946, when he was honorably discharged.

With the help of the GI bill he enrolled in Gettysburg College in the fall of 1946. He majored in physics because his interest had been stimulated in high school. He came under the guidance of Prof. George Miller at Gettysburg College, who intensified Norm's interest in physics, and also encouraged Norm to go to graduate school. Upon graduation (cum laude) in June 1950 Norm enrolled in graduate school in physics at the Massachusetts Institute of Technology. Before leaving Gettysburg he met a young coed, Thalia Tichenor, who subsequently became his wife (in 1952) and lifelong soul mate.

At MIT he worked for Prof. Robley Evans in the Radioactivity Center, which Evans created and led. The work was concerned with the field of experimental low-energy nuclear physics, including the determination of nuclear energy levels, radiation dosimetry, and the biological effects of radiation. It was in the fall of 1952 that I first met Norm. He was a teaching assistant in Prof. Evans's two-semester subject "Nuclear Physics," which I took as a senior in physics. Norm was always available to the students to help with understanding the material and in working the devilishly long homework assignments. One of my classmates and close friends subsequently became a research assistant in the Radioactivity Center, and I began to see Norm frequently outside the classroom. He was an avid sports enthusiast, both as a player and as a fan. We frequently shared despair over the fate of the Red Sox and the impact of the curse of the Bambino. (For readers not familiar with the curse, it began in 1920 when Harry Frazee, owner of the Red Sox, sold his star pitcher, Babe Ruth, to the New York Yankees for cash. Frazee subsequently used the cash to promote a Broadway flop, whereas the Yankees converted Babe Ruth to a hitter. And the rest is a well-known history of triumph for the Yankees and tragedy for the Red Sox.) Even in our later years together as faculty colleagues we would occasionally sneak off in the afternoon to go watch the Red Sox together.

ACADEMIC CAREER

Norm completed his Ph.D. in 1956, and his thesis was entitled "Standardization of Electron Capture Isotopes." This was a very creative experimental thesis involved in determining absolute nuclear decay rates. After graduation, Norm remained in the MIT Physics Department as an instructor. He also continued his experimental work in the Radioactivity Center. Norm's hands-on experience as a child made him an extremely versatile and creative experimentalist. In the 1950s the tools available for detection and measurements were primitive. Norm was in the forefront of developing coincidence-counting techniques to measure decay schemes, which was the focus of his early papers.

At this time, the mid-1950s, MIT was building the MIT research reactor and expanding the program in nuclear engineering into a full department. Norm was invited to become an assistant professor in the new department to help in the creation of a curriculum that included experimental methods. He also became an important experimentalist using the new reactor. He was a key participant in the building of a 6-meter bent crystal spectrometer that was used for gamma ray spectroscopy studies for many years. He migrated from the determination of decay spectra to the use of spectra for measuring nuclear composition. This led him to a major program for the measurement of spent nuclear fuel composition, a matter of significant importance to the nuclear weapons programs where both tritium and plutonium were created in production reactors. This work also brought him international renown, as the International Atomic Energy Agency adopted his techniques for use in proliferation studies.

Although a magnificent experimentalist, he was also exceedingly creative in applying new technologies to nuclear spectroscopy problems. He was among the leaders in adopting the use of solid-state devices for photon detection and measurement. He was an important contributor to the development of lithium drifted germanium detectors. He also recognized the importance of data analysis and was the first spectroscopist to adopt the then-new fast Fourier transform to data analysis.

Part of his training and background was an appreciation of the importance of statistics to the analysis and interpretation of data. Robley Evans was very firm in training all his students to be careful and thorough in their analyses. This training was reflected in Norm's work and laid the foundation for his subsequent appreciation of probabilistic risk assessment. It also made Norm an excellent poker player, a pleasure he pursued regularly and profitably.

One of Norm's closest colleagues and collaborators was Prof. Theos J. Thompson. Tommy came to MIT in 1957 to design the MIT research reactor. In 1966 Tommy began a special summer program in nuclear power plant safety. This program brought together leading experts in all aspects of safety, including reactor physics and engineering, materials problems, instrumentation and control issues, plant operations, modeling and simulation, and plant licensing. Norm was a participant in this program, and in 1969 he became the director when Tommy left to become an AEC commissioner. As a result Norm was in the position of being an experienced analyst with a deep understanding of most of the issues involved in nuclear power technology.

THE REACTOR SAFETY STUDY

The first civilian nuclear power plant, Dresden 1, went online in 1959. This was followed by Yankee Rowe in 1960. The electric utilities began a rapid increase in plant orders and construction. The first large unit was at Oyster Creek in New Jersey and was a very large plant, over 650 MWe. The plant was ordered in 1963, construction was approved in 1964, and the plant went into commercial service in 1969. Another large plant, Nine Mile Point, also went into service in 1969. Thereafter growth was very rapid; four plants in 1970, four more in 1971, and eight plants in 1972. In 1973 U.S. utilities ordered 41 nuclear plants. Clearly the industry was growing, and attracting attention.

Opposition to nuclear power began to take shape in the 1960s, with the initial concern focused upon radiation from the plants and the effluents. Then the concern shifted to safety and the consequences of large accidents. Interveners began to attack the licensing process and create expensive delays in plant construction and licensing. The plant designs were based upon the concept of the "maximum credible accident." Usually this took the form of a large rupture in a main coolant pipe, depriving the core of cooling water. Arguments in the courts and in the public arena were complicated because of the lack of quantitative assessments of the real risks associated with the plants. Senator John Pastore (Rhode Island) was the chairman of the Joint Committee on Atomic Energy, and in 1972 he wrote to James Schlesinger, head of the AEC, encouraging the AEC to undertake a study that addressed the issue. Schlesinger agreed and went about creating a large-scale project for that purpose. Because of the significance of the study it was felt that it should be led by someone outside the AEC itself. Norm's name emerged as a likely leader of the project based upon his association with the issue, his neutrality as an academic, and his scientific reputation. Norm agreed to head a multiyear, multimillion-dollar study.

He was very fortunate to have as a close collaborator, Saul Levine, who was then the deputy director of the Office of Research at the AEC. Together they began to review potential tools for risk analysis and encountered some classic work by Chauncey Starr and F. R. Farmer that suggested probabilistic approaches to address licensing and siting. Their work also considered the use of event trees to identify how things could go wrong, and then in using fault trees to develop quantitative evaluations of the likelihood of an accident. This was then to be followed by an assessment of the consequences of every failure (e.g., radiation release quantities, pathways to the environment, and effects on population). Together Norm and Saul created a program to examine the risk associated with both major types of U.S.

reactors (i.e., the pressurized water reactor and the boiling water reactor). Their team ultimately involved a large number of analysts at the national laboratories, the utilities, and several universities.

The activities of the AEC were overseen by the Joint Committee on Atomic Energy (JCAE) of the U.S. Congress. The committee had a deep interest in the future of nuclear energy and in the findings of the study under way. Norm was frequently called upon to testify before the JCAE. He was an extraordinary witness due to his great depth of knowledge, his ability to put complex issues in a comprehensible form, his obviously forthright presentations, and his wonderful sense of humor. At one hearing Senator Pastore was presiding. Norm was explaining the concepts of event trees and fault trees and how they were used. In the midst of his testimony the quorum bell rang. Senator Pastore interrupted Norm and explained that the committee members would have to leave in about 10 minutes. He asked Norm how much longer he would need to complete his remarks. Norm replied, "Senator, that depends upon how smart you are!" The staffers in attendance were all aghast, and Senator Pastore roared with laughter and suggested that the committee should adjourn promptly.

The study report, WASH 1400, was released in draft form in 1974, and the final version in October 1975. It was received with appreciation from the industry because it concluded that the risks to nuclear power were very low. Conversely the opponents attacked the report vigorously because the conclusion was unacceptable to them. There followed an extensive period of review, debate, and reassessment. Appreciation for the report grew after the Three Mile Island (TMI) accident. The report had suggested that small breaks in piping were much more significant than the large break accident. TMI was in fact a small break. In the aftermath of the accident the Kemeny Commission suggested that the methodology be used in risk assessment. The Nuclear Regulatory Commission had replaced the AEC as the regulator in 1975. After TMI the Nuclear Regulatory Commission began to use probabilistic risk assessment (PRA) for specific safety issues; for example, the issues regarding loss of offsite power to a station were analyzed and found to be significant, leading to new regulations. The commission went even further in the 1990s by deciding to use PRA for judging the impact of the usefulness of various safety regulations. Today the industry operates under what are called “risk informed regulations,” which allow utilities to use PRA to adjust their service and maintenance activities. Partly as a result of these changes U.S. plants are now among the most productive in the world.

Norm received well-deserved recognition for this pioneering work. He was elected to the National Academy of Engineering in 1978 and to the National Academy of Sciences in 1979. In 1985 he was awarded the Department of Energy Enrico Fermi award, the most prestigious of all its awards. The Fermi award had a cash stipend of $100,000. A few weeks after receiving the award, Norm told me of his adventures with his new riches. He deposited the check at his bank and waited a few days to inquire at an ATM about his balance. He said he just wanted to see that much money in his account. The balance did not reflect the deposit. He waited another few days and tried again, and again the deposit wasn’t shown. After a third trial and several weeks after making the deposit, he went to the bank personally to ask what had happened. The teller listened to his story and then patiently explained that the ATM screens only showed 5 digits before the decimal.

With the release of WASH 1400 Norm was involuntarily committed to becoming a public figure. He spent an incredible amount of time traveling the world explaining the methodology, defending nuclear power, and helping develop the applications. He was always fair in his debates, never indulging in distortion, misrepresentation, or exaggeration. He was deeply appalled by the poor quality of some of the actions of some opponents. Most of all he was distressed by the unwillingness of some opponents to discuss issues offstage and off camera. He always tried to understand the nature of the opposition and how together the industry and the opponents might find constructive resolution. He kept on his wall a cartoon showing two figures separated by a deep, symmetric chasm. One character is saying to the other, "Come over to my side, the view is much clearer." He always tried to maintain a balanced perspective on the nuclear issue and did his best to convince others to do the same.

While maintaining his activities in the nuclear power arena he continued an active academic career. He was named head of the Nuclear Engineering Department in 1975 and served in that position for seven years. In 1983 he was named the McAfee Professor of Nuclear Engineering. During these years he continued an active research program but with the focus now on risk assessment. He was highly sought after by students to be their thesis supervisor. The student grapevine was, and is, well attuned to the merits of various faculty members as advisors. Norm was one of the best in giving his students lots of time, attention, and moral support. He supervised more than 60 graduate theses, and each of his graduates became a lifelong friend.

He was appointed by President Reagan to the National Science Board in 1982 and served for six years. He also served on the Defense Science Board from 1974 until 1978. He continued as a consultant to the Defense Science Board until his retirement in 1990. He retired from active teaching in 1994 in part due to his health.

THE MAN

Norm maintained a remarkably wide set of personal interests and activities. He was very good with his hands and pursued crafts with diligence and skill. He made much of the furniture for his home just for the shear joy of craftsmanship. He and his wife purchased land in New Hampshire on a small lake, and he cleared the land and by himself built a small home. He would visit barn sales throughout New England to find old beams and boards and incorporate these into his home. As part of his land clearing he purchased an abandoned bulldozer and restored it to operating condition. He then used the bulldozer to improve the road into his property and prepare a site for a sauna, which he again built by hand. He loved spending time in the summer on the lake. In the fall he would go up on weekends to cut wood for the stove and fireplace. And in the winter he used the home whenever he could arrange a ski trip to the mountains.

Perhaps my favorite tale of Norm has to do with his wood chopping one fall. He cut wood for most of a chilly October Saturday. After enough effort, he fired up his sauna to relax. After he had been inside long enough, he thought he might prove his Scandinavian roots by leaping into the lake. Knowing that this late in the season no one would be at the lake he ran out of his sauna in the buff, ran down the path to his dock, and pounding his chest and yelling like Tarzan he leaped into the lake. Only after becoming airborne did he note that two very frightened women were sitting in a rowboat fishing just off the end of his dock.

Norm was also very athletic and participated in all kinds of sports. He was particularly fond of skiing, and we always arranged our teaching schedules to have common days off to go skiing in the middle of the week. We also served together on the Scientific Advisory Board of the Idaho National Engineering and Environmental Laboratory. We frequently managed to find time to ski in Utah or Wyoming on those trips.

Beyond sports Norm had a real passion for bird-watching. Wherever he traveled he took binoculars with him in the hopes of having a few minutes to see new species. As part of his duties on the National Science Board he traveled to the South Pole. He made arrangements to be helicoptered over to the ice shelf in order to see emperor penguins. He was particularly fond of penguins and found this trip one of the most exciting of his life. After completing the trip, he gave a seminar in the Nuclear Department with a slide show that included the penguins. He appeared at the seminar dressed in a penguin costume, which created one of the lasting moments in the department's history. He also took a vacation to journey to the Priboloff Islands in order to see the unique species present there.

There is no doubt that the greatest individual inspiration in his life was his wife, Thalia. Together they shared the raising of two children: son, Neil, and daughter, Arlene. Subsequently they enjoyed together four grandchildren. Norm was blessed with intelligence, a strong work ethic, and a wonderful family life that was apparent to all who knew him.

Norm will be most remembered by the scientific community for his remarkable achievements in the area of nuclear power plant safety. Every nuclear plant around the world now has a tool that allows for the assessment of risks, and of means for improving the safety of plant design and operations. The Nuclear Regulatory Commission has used the results of his methods to assist in identifying new regulatory processes and procedures. The results are much greater insights into system design and performance. All new reactor concepts are influenced by the ability to examine their safety in a quantitative way. Other technical areas are beginning to adopt the probabilistic risk assessment approach.

I WOULD LIKE TO THANK several colleagues and friends for their assistance in preparing this biography. Gordon Brownell, Frank Masse, and Costa Maletskos were with Norm in his early years in the Radioactivity Center and provided much valuable information. Prof. George Apostolakis was very generous in reviewing material regarding WASH 1400 and its impact on the industry.

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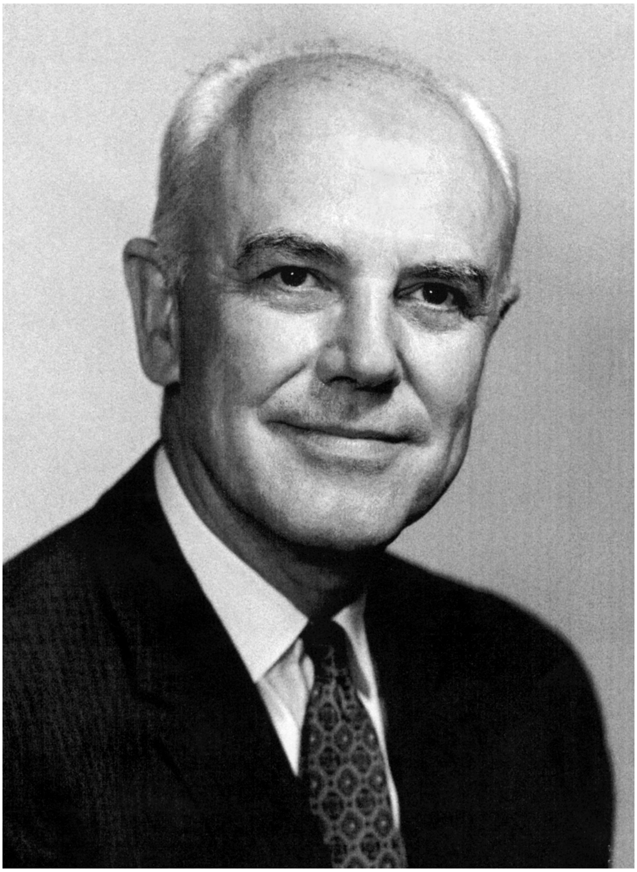

G. W. Salisbury

GLENN WADE SALISBURY

June 2, 1910–February 3, 1994

BY ROBERT H. FOOTE

EARLY IN HIS CAREER Glenn Salisbury became the foremost researcher in the world in testing ideas and applying results that revolutionized the breeding of dairy cattle through the development of sound principles and procedures for artificial insemination with semen from genetically superior sires. This has become the most powerful biotechnology used worldwide for the improvement of cattle. The guiding principles exemplified by Salisbury were good basic research, integrity, and superior accomplishment. He expounded on these in a superb mimeograph, *An Approach to the Scientific Solution of Problems in Biology*, in 1948. These principles were passed on to students and others through example and active mentoring by Salisbury. Only the best in any endeavor was acceptable for his own efforts as well as those of his colleagues.

His exemplary leadership, as head of the Dairy Department at the University of Illinois, resulted in a department that rose in 20 years from near the bottom of the list of published research by agricultural experiment stations to near the top. He instilled in his associates the importance of reflecting on the moral aspects of their research, and the social and economic consequences nationally and internationally. He fought fiercely for what he believed was important and right, regardless of administrative inertia and any political backlash that might occur. He believed strongly that the higher the position one held, the greater the responsibility for service to others. Consequently he did not pull punches when the obstacles were self-serving individuals. As a consequence, Salisbury made a major contribution in training outstanding young leaders, as well as in the improvement of animal agriculture, with the objective of feeding a hungry world better.

PERSONAL

Glenn Wade Salisbury was born on June 2, 1910, on his maternal grandfather's farm in Sheffield Township, Ashtabula County, Ohio. His dad's ancestors also were farmers. Thus, he was surrounded in his early life with a family that had a zest for life, a great appreciation of the land, of working hard, and of caring for one another—all characteristics of Salisbury in his adult life. His father's academic career took him to many places and schools during his precollege days. Summers were spent on his grandfather's farm, enjoying the Jersey cows, trotting horses, and pigs.

Salisbury entered Ohio State University in 1927. He was a leader in student affairs and a member of the Student Senate and several honorary societies. He was an articulate member of the dairy cattle judging team. Upon graduation in 1931 he was selected as the outstanding senior in the College of Agriculture, based on his leadership and academic achievement. Following graduation in June 1931 he visited the famous Mt. Hope Farm in Massachusetts, where progressive genetic studies were in progress with cattle and poultry. These experiments stimulated Salisbury's interest in the genetic potential for improving animal agriculture.

Few jobs were available at the height of the Depression, so Salisbury accepted a half-time assistantship at Cornell University. This financial support of $700 per year enabled him to take a major step in his life. In August 1932 he married Dorothy Jane Cross, who became his lifelong, supportive companion and devoted mother to children Laird Wade Salisbury and Susan Lynne Salisbury. Glenn and Dorothy had dated regularly four years earlier, until her family moved. The romance was continued by correspondence. After the wedding it was immediately back to work for Salisbury, in the middle of his Ph.D. studies. The honeymoon was a bicycle trip through Europe in 1937.

At Cornell University Salisbury studied nutrition, physiology, and genetics (1931-1934), receiving his Ph.D. degree in three years. The chairman of his committee was Professor F. B. Morrison, world famous in agriculture for his book *Feeds and Feeding.* Professor S. A. Asdell, one of the world's great animal biologists, trained by Professor F. H. A. Marshall at Cambridge University, was Salisbury's mentor in physiology. His genetic exposure was to a superb teacher, Professor A. F. Fraser, in a department that had recently trained Barbara McClintock and George Beadle, both Nobel laureates.

PROFESSIONAL CAREER AT CORNELL UNIVERSITY

Salisbury was hired in 1934 as an instructor in the Department of Animal Husbandry upon completion of his doctoral program. This was the beginning of a long and illustrious career. In only 10 years he rose through the ranks to become a full professor. The year he became a full professor he was the youngest scientist to receive the Borden Award, a prestigious national award for outstanding contributions to reproductive physiology and production of dairy cattle. This was especially remarkable, considering that his appointment was for 45 percent of his effort in research and 55 percent for teaching and extension.

Initially he assumed responsibility for teaching an introductory course in nutrition of farm animals to animal husbandry students. Noting that veterinary students received little training in nutrition, he developed a course designed to meet their needs. He also developed an introductory course in animal breeding, comprising both comparative reproductive physiology and genetics. Soon he was recognized as a master teacher. This course became one of the more popular undergraduate courses at Cornell.

He was a popular speaker among dairymen, bringing news of college programs to them and soliciting their ideas. He was equally comfortable and conversant with dairymen in the barnyard and scientists at national meetings.

Salisbury developed the program in animal breeding at Cornell University, which he administered until he left for the University of Illinois in 1947. This program required graduate students to obtain basic training in biochemistry, physiology, mathematics, and genetics. Salisbury also was a lifelong student, hungering after knowledge, which he consumed in great quantities.

How did he accomplish all this? Partly he sought out and developed associations with the best minds in each field at Cornell and elsewhere. He continued to collaborate with S. A. Asdell, author of the gold standard book *Patterns of Mammalian Reproduction*. He took a summer off to study at Iowa State University with Jay L. Lush and with George W. Snedecor, respectively, a foremost animal breeder and a biological statistician. Salisbury considered both Lush and Snedecor as among the best teachers in the world.

In 1937 he bicycled 4,000 miles with his wife, Dorothy, through the British Isles, the Channel Islands, and France, visiting research laboratories, experiment stations, and farms.

An appendectomy in the middle of the trip slowed him down only temporarily. In 1936 and 1938 the Salisbury couple drove to many agricultural experiment stations in the Midwest to become better acquainted with leading scientists, their research, and their approach to solving major problems. They did not stop at Illinois, because what was being published there was of little interest to him.

At the same time he was in contact with famous colleagues in the department at Cornell, including Nobel laureate J. B. Sumner. Sumner was the first person to crystallize an enzyme, which was catalase. He gave a sample of catalase to Salisbury. Salisbury added this catalase to bull spermatozoa and found that it prevented the damaging effect of oxygen by stopping the accumulation of hydrogen peroxide (1949).

Salisbury was not shy in emphasizing that the new paths he wanted to explore in reproductive physiology required basic research to underpin applied studies in artificial insemination. He found ways to do it despite the lack of administrative support. One of his friends, I. C. Gunsalus, had laboratories in another department where the bacteriology of bull semen could be studied (1941), as infectious disease transmitted through semen was one of the probable major causes of low fertility in dairy cattle.

Simultaneously Salisbury teamed up with Professor Stanley Brownell, one of the most able, articulate, and forceful extension professors in the United States, to generate support from leading dairymen and key members of the Cornell Board of Trustees to develop a major artificial insemination program in dairy cattle. Governor Dewey had a dairy farm, and he supported the idea. Timing was right to develop this program. Some members of the veterinary profession were opposed to the development of artificial insemination; however, a strong advocate was William A. Hagan, dean of the New York State Veterinary College. In 1945 the artificial insemination program moved to Ithaca, New York, where Cornell University made land available. Dairymen in the New York Artificial Breeders Cooperative, Inc., and New York State each contributed $25,000 to build facilities close to the Cornell campus. An enormously successful cooperative program of research and application followed. Salisbury now had his laboratories where the biochemistry of bull sperm could be studied without interference.

Salisbury also was a member of several important committees, including the Graduate School. He was a faculty member on the Cornell Board of Trustees. He had made substantial progress in all aspects of his career. However, he was rankled by the lack of support by the dean of the college and the criticism that his group was conducting biochemical research outside of the Department of Biochemistry. He voiced his objections to this criticism. Furthermore, in 1945 he was not chosen to become head of the Department of Animal Husbandry, a position that he had been interested in exploring. Consequently, as inquiries came to Salisbury from several universities, inviting him to consider heading various departments, he began to think, "What if the right situation came along?"

In 1947 he was invited to come to the University of Illinois, as a new head was being sought for the Department of Dairy Husbandry. During his first visit Salisbury pointed out many deficiencies that existed. Subsequently, after several changes were made at Illinois, Salisbury was invited to visit a second time. This time he visualized that he could have a dairy husbandry group that would contain highly qualified individuals to form sections of dairy chemistry, dairy bacteriology, and dairy cattle genetics, in addition to the more traditional physiology, nutrition, and management sections. All they needed was stimulation to move ahead with their research more aggressively. Events moved swiftly. Glenn W. Salisbury resigned his position at Cornell University, effective October 31, 1947.

PROFESSIONAL CAREER AT THE UNIVERSITY OF ILLINOIS

The new job at the University of Illinois was not without problems. There had been considerable disarray in the Dairy Husbandry Department during the war. Many faculty members were pleased to see a new head arrive who could resolve problems by appropriate reorganization. Others were concerned that Salisbury would promote the development of the basic sciences and weaken the animal production component. Fortunately, Salisbury had several factors going for him, which he skillfully managed.

As a new department head he had been given the charge of revitalizing the department. This he did initially by convincing the administration to change the name of the department from Dairy Husbandry to Dairy Science. Then he obtained additional prestige for the professors in the department by receiving permission to appoint them with the specific titles of professors of biochemistry, physiology, genetics, microbiology, or nutrition. Salisbury was able to add seven new faculty members to his staff during the first four years of administration. He attracted superb individuals trained at several excellent universities and with a mixture of special talents. The result was a department ready to lead into the future, facing agriculture changing rapidly after World War II.

He, and others, fought for new facilities, campaigning within the university and by personally contacting influential farm leaders throughout the state. This resulted in a new Animal Sciences building that housed the entire faculty and provided all with excellent laboratory facilities. The extension group was included, as Salisbury recognized the importance of the Land Grant mission.

Stimulated by these new resources and by Salisbury's dynamic leadership, the department attracted more outstanding graduate students. Research flourished. When Salisbury left as head of the Department of Dairy Science in 1969, his department was one of the leaders among dairy and animal science departments in number of research publications per year.

Teaching also flourished. Salisbury was a great disciple of the principle that knowledge is power. He believed all undergraduates should be exposed to the basic sciences within the dairy field, as well as the more traditional courses of feeding, breeding, and management. The new faculty added courses for advanced undergraduates and graduate students in physiology of reproduction, rumen microbiology, biochemistry of nutritional processes, endocrinology, biometry, and quantitative genetics. The emphasis on basic training equipped undergraduate students to fill technical positions in agriculture and the pharmaceutical industry, and to pursue advanced studies, as the number of dairy farms was decreasing rapidly. The graduate students were competing successfully for excellent postdoctoral positions, university faculty positions, and technical positions in industry.

Salisbury was criticized by those who resisted change from the traditional department dealing primarily with management problems of producers. This aspect was reduced but not neglected. Furthermore, he could point to a large increase in funding and research productivity through competitive extramural grants awarded to faculty in the department. This enabled the faculty to provide excellent research exposure for undergraduates and graduate students. The department became nationally recognized, resulting in increased opportunities for faculty to assume leadership roles nationally as well as within the university. With this recognition, the faculty received an increasing number of prestigious awards for teaching and research. Salisbury accomplished this by acquiring a complement of competent interactive faculty. Then he sought ways to assist staff members to develop to their full potential, and was intolerant of those who did not.

CAREER RESEARCH ACCOMPLISHMENTS AND AWARDS

Salisbury followed the logical concept that an inquiring mind was necessary for development. Furthermore, any procedures applied broadly in agriculture should be based upon as much understanding as possible of the principles, mechanisms, advantages, and weaknesses of the system. Thus, careful research was always an essential component of any program in his department. He personally continued to be involved directly in research as well as teaching during his two decades of leadership in the Department of Dairy Science.

Salisbury is best and rightfully remembered for his foresight and research contributions in reproductive physiology that provided the framework for the successful development of artificial insemination of cattle worldwide. He recognized that in addition to trained people, a successful artificial insemination program required (1) superior healthy, fertile bulls, (2) methods of semen collection that protected the sperm, (3) techniques for evaluating semen quality, (4) preservation of the viability of sperm until used for insemination, and (5) a system to evaluate the fertility of sperm inseminated. His 265 publications addressed all these components and more, but only a few examples can be highlighted here.

Extensive research had been conducted on feeding dairy cows, but little was known about the nutrition of bulls. With his graduate students he was the first to study the energy and protein requirements of mature bulls. Feeding standards were established for maintaining healthy bulls that produced high-quality semen. Objective measurements of semen quality were needed as indicators of sperm fertilizing ability. Salisbury and his students published many papers on the morphology, motility, and metabolism of sperm. Basic studies on sperm metabolism elucidated mechanisms by which sperm gained energy to remain fertile. Suppression of the more efficient oxidative biochemical pathway, and forcing sperm to use the glycolytic pathway, increased sperm motility during storage and subsequent fertility. Studies on mineral components of fluids from the excurrent ducts of the male reproductive system revealed that the sodium-potassium ratio markedly affected sperm metabolism. Later studies showed the importance of phosphate and pCO_2 in depressing sperm respiration. A variety of tests of sperm function were established and standards set to assure that semen used for insemination would be highly fertile.

Salisbury's discovery of the importance of the citrate ion combined with egg yolk led egg yolk-citrate to become the standard medium worldwide for preserving bull sperm. His studies with Gunsalus, and later with Foote, on the bacteriology of bull semen and control of pathogens through the inclusion of antibiotics in the egg yolk-citrate medium led to a major improvement in fertility in dairy herds. This greatly accelerated the adoption of artificial insemination.

Salisbury and Thompson, in 1947, developed an ingenious, simple method of accurately estimating fertility to evaluate results of all semen processing and insemination procedures. The "non-return" rate method developed is still in use worldwide to assess fertility.

Semen from superior sires became available to inseminate thousands of cows per bull as a result of Salisbury's pioneering studies that the billions of sperm per ejaculate of semen could be diluted to millions of sperm per insemination without reducing fertility. Critics scoffed when they heard of these planned experiments, saying it was like "watering the milk." Instead, the high fertility led successors in the Salisbury laboratory (Foote and Bratton) to coin the term "extending semen," which is used throughout the industry today.

Salisbury observed that extensive data from the artificial insemination industry indicated that aged sperm held at 5°C could fertilize the oocyte, but embryo death often appeared to follow. Laboratory studies of DNA and DNA-protein complexes also showed changes as bull sperm aged. Aged frog sperm also produced defective embryos. Furthermore, from analysis of large batches of cryopreserved sperm used in the field, he concluded that similar trends occurred with frozen sperm. However, storage temperature in the field was confounded with multiple manipulations by inseminators. Controlled laboratory studies by others have revealed that sperm cells are highly stabile when stored continuously at –196°C.

Salisbury shared his information through scientific publications, presentations at national and international meetings, and in various classroom settings. Together with N. L. VanDemark he prepared a seminal reference book *Physiology of Reproduction and Artificial Insemination of Cattle*, which was published in 1961 and was revised in 1978. His wide range of interests are indicated by his membership in the following societies: American Association for the Advancement of Science (fellow), American Dairy Science Association, American Society of Animal Science, American Genetics Association, American Physiological Society, Society of Cell Biology, Society for the Study of Fertility, Society for Experimental Biology and Medicine, and Society for the Study of Reproduction. He was also elected to five honorary societies, including Sigma Xi, becoming president of that society at the University of Illinois in 1966.

Salisbury was the recipient of many honors and awards. Previously mentioned was his receipt of the Borden Award at age 35 for outstanding contributions to physiology and production of dairy cattle. The American Society of Animal Science presented its highest award, the Morrison Award, in 1964 for research contributions to animal science. Several honors from foreign governments were received in 1964 and 1965. In 1971 the University of Illinois recognized his outstanding achievements in research and contributions to agriculture with the Funk Award, and national acclaim resulted upon his election to the National Academy of Sciences in 1974. This was followed by a Distinguished Service Award from the American Dairy Science Association in 1978 for his lifetime contributions to science and the dairy industry. The highest world prize in agriculture, the Wolf Prize, was awarded to Salisbury in 1981. This award was timely, as it occurred when he was closing his remarkable academic career.

PUBLIC SERVICE

Salisbury started his public service while at Cornell University. In 1944 Salisbury and others were asked by the Near East Foundation to propose a plan to restore Greece's livestock population, which had been decimated by the war. There was a great opportunity to rebuild the dairy cow population with superior animals by artificially inseminating cows with semen from selected bulls. This rekindled a dream that Salisbury had first visualized when he visited Mt. Hope Farm in 1931. This program really was democracy in action, making the best genetics available not only to wealthy bull owners in the United States but also to all cattle farmers regardless of their economic status.

In 1946 Salisbury went to Greece for several months, working with Greek authorities to implement the plan he had helped to design. In 1963 he was commissioned, as part of a team, to develop plans for agricultural research and higher education in Greece. This experience had a great influence on Salisbury, as shown in his dedication of a whole volume to Greece in his memoirs. I can appreciate this impact, because I was a graduate student in Salisbury's laboratory at this time, and listened to some of the discussions afterward.

At the University of Illinois, Salisbury served on his share of housekeeping committees, but more importantly his foresight and insight were shared on many university, national, and international committees and commissions. He was elected to the Executive Committee of the Graduate College, the Long-Range Planning Committee, the Council on Program Evaluation, the Faculty Senate, the Committee on Academic Freedom, the Committee to Determine Policy on Classified Research, and many others. These positions, committees, and others benefited from his brilliant mind and the wisdom that came from experience.

From 1961 to 1963 he served on President Kennedy's Science Advisory Committee. In 1965 Salisbury was a Fulbright scholar at the Agricultural University in Wageningen, The Netherlands. The same year he was a consultant to the U.S. Agency for International Development, studying and recommending programs for improving agriculture in India, and in 1969 for Indonesia. From 1974 to 1977 he was a consultant to the Office of Technology Assessment of the Congress of the United States. He served on the Board on Agriculture and Renewable Resources of the Commission on Natural Resources of the National Research Council from

1975 to 1978. He participated in writing the report *World Food and Nutrition Study: Enhancement of Food Production for the United States.* From 1977 to 1978 he cochaired the Joint U.S. Department of Agriculture/State Agricultural Experiment Station Commission to recommend safety procedures to the USDA. These are only examples of many groups that sought his advice, which he willingly gave.

Simultaneously he was involved in many community organizations. He was active in the Methodist church, a board member of the University of Illinois YMCA, served as president of the Urbana Rotary International organization, and was a Paul Harris fellow.

In 1969 Salisbury became associate dean and director of the Illinois Agricultural Experiment Station, a position he held until his retirement in 1978. During those nine years Salisbury continued to publish research. In addition, he became more interested in and deeply concerned about the efficiency of the agricultural experiment stations in the United States and the effectiveness of the Cooperative Extension Service in delivering timely information to the needy anywhere. He despised excessive bureaucracy, with its effect on decreasing the efficiency of research and distribution of the results. The important goal was to feed the hungry, wherever they were.

As he viewed advances in the technology capable of manipulating the reproductive process in animals, he became increasingly concerned with *Homo sapiens* having the moral wisdom and courage to control or withhold application of this technology to our own species.

FAMILY AND HOBBIES

Salisbury was intensely devoted to his job, his staff, and his students. The older graduate students at Cornell remember the young Professor Salisbury as a brother. All of us graduate students and our families looked forward to the annual Thanksgiving invitation to share the day with Dorothy and Glenn Salisbury. We gave thanks for our wonderful academic family. At the same time he was a loving personal family man. He was devoted to his wife, Dorothy, and supportive of their children. He took great pride in their achievement. As time passed he joined the ranks of doting grandparents.

He loved the outdoors and the good earth. He was an avid gardener. His garden continued to grow larger during retirement. Salisbury loved to till the soil, plant the tiny seeds, and watch a variety of nourishing vegetables grow, especially if someone else would weed the garden and harvest the abundant supply.

Family camping trips to scientific meetings or to Wisconsin vacationlands were an adventure during rain or sunshine. He enjoyed the serenity—the time to relax away from it all. He enjoyed fishing and the kids reported that the fish enjoyed their Dad, because many of them got away. He refinished furniture, painstakingly scraping off multiple layers of paint that obscured the beautiful wood underneath. He enjoyed cherry wood especially. One refinished cherry table served as the family dinner table for many years.

Salisbury had an insatiable appetite for reading novels as well as scientific publications. He was particularly fond of military history. When he was a young boy, his grandfather had spun tales about service in the Civil War. It is likely that Salisbury was as much interested in the study of soldiers and their leaders as he was in the bloody outcome of battles. He was an astute judge of human beings.

While he was excellent in repairing and building people, he had no hankering for repairing cantankerous equipment or assembling do-it-yourself items. This was a family joke, likely to the annoyance of Salisbury, who did everything in his professional life as near perfection as possible. His kids reminisced that they had to remind their Dad to read the directions.

In retrospect, Glenn Wade Salisbury combined intellect, vision, and unlimited enthusiasm with integrity, qualities that inspired others. As a result his group was the dominant factor in developing artificial insemination, the most powerful biotechnology for genetic improvement of livestock. He was a builder of people, as he saw the potential of individuals, and nurtured each one to the fullest.

THIS BIOGRAPHY is based on personal reflections as I began graduate work with Glenn Wade Salisbury, on files obtained from the Department of Animal Science at Cornell University, and especially on voluminous files kindly sent to me by his wife, Dorothy Cross Salisbury. Drs. George and Sarah Seidel reviewed the text. Their helpful suggestions are appreciated.

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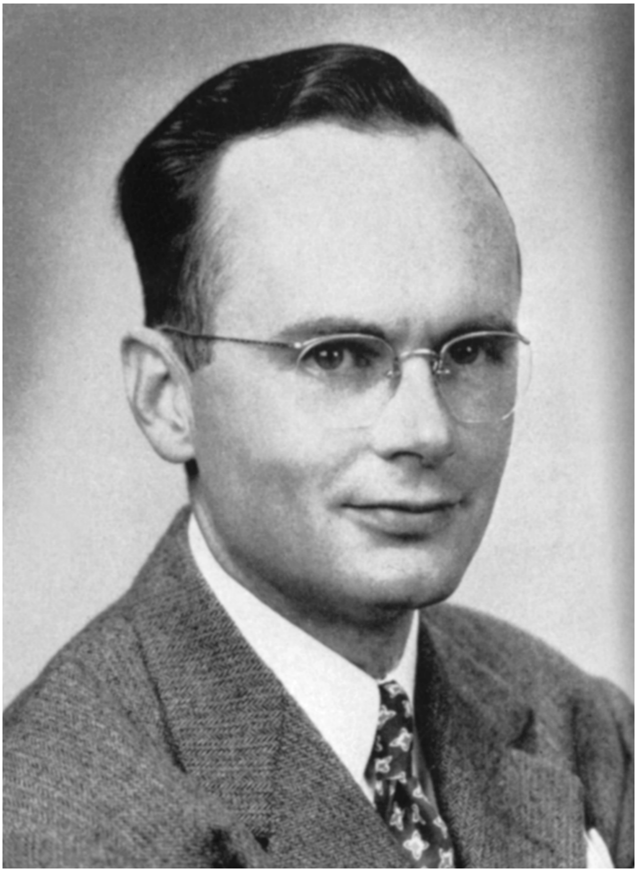

Courtesy of the American Institute of Aeronautics and Astronautics

W. R. Sears

WILLIAM REES SEARS

March 1, 1913–October 12, 2002

BY NICHOLAS ROTT

William ("bill") sears was born in Minneapolis, son of William and Gertrude Sears (née Rees). As described in his recollections, Bill's youthful interests included music and literature, but by the time he was ready for college his talents in the mathematical and technical sciences determined his choice. He enrolled as a student in the University of Minnesota and earned his bachelor of aeronautical engineering degree in 1934. After that he moved for graduate studies to the California Institute of Technology in Pasadena, where he became a student of Theodore ("Todor") von Kármán.

Bill Sears was deeply impressed by Kármán's scientific powers and also by his warm and humane personality. Bill's thesis on the theory of oscillating airfoils in an ideal flow, submitted in 1938, turned out to be a classic that summarized and enriched substantially all previous efforts in this important field. In 1940 Bill went on to publish a paper, based on the results of the von Kármán-Sears theory, that pioneered the application of operational methods for the solution of problems arising when an impulsive change occurs in the velocity and the shape of an airfoil (1940). Problems that were reduced to a search for the solution of an integral equation were solved here, for the first time, by analytic continuation in a complex plane.

The student years brought Bill not only scientific fame but also extraordinary experience as a teaching assistant and associate. He adapted many of Kármán's teaching methods, and married Kármán's secretary, Mabel Rhodes, who became Bill's steady companion in a long, happy, and brilliantly successful life.

Bill was appointed instructor in aeronautics in 1937, before he got his Ph.D. in 1938. He was appointed assistant professor of aeronautics in 1940. He left Caltech in 1941 because of the impending war.

In 1940 Bill received his pilot's license, and flying became a hobby that led him during the years to the ownership of several types of private airplanes. By the time he retired from flying in 1990 he had logged 8,000 hours of airtime, both for business and for pleasure. On major expeditions he was regularly accompanied by Mabel, his trusted navigator.

In 1941, with the entry of the United States into World War II becoming more evident, Sears accepted the offer of Jack Northrop (a friend of von Kármán) to became chief of aerodynamics and flight testing for the Northrop Aircraft Corporation. His main responsibilities were the design of the P-61 (the Black Widow) and, in particular, the aerodynamics of the "Flying Wing." His name is firmly connected with these projects in the history of aviation. In 1945 he also served in the Naval Reserve in Germany, debriefing German scientists and engineers. More details about this very interesting chapter in Bill's life can be found in his autobiography, which was published in 1994, and in an article (obituary) written by Frank Marble of Caltech, published in *Engineering and Science* (vol. LXVI, Nov. 1, 2003).

The years of working for the Northrop Company did not interrupt Sears's involvement with the fundamental problems of aerodynamics theory. Within days after he left the company in 1946 his paper on compressible flow past bodies of revolution appeared in print. The question of how to generalize compressibility corrections known in two dimensions to three dimensions had haunted aerodynamicists for decades, and Bill's contributions were essential in leading to the final correct solutions. They also led to the design of minimum wave drag projectile shapes known as the Sears-Haack bodies.

A new era in Bill's life started when in 1946 he became the director of the newly created Graduate School of Aeronautical Engineering of Cornell University in Ithaca, New York. Because of his experiences at Caltech and in the industries of Southern California, he was able to assemble a faculty for this school that jump-started its national and international fame. His appointee in the field of general aerodynamics was Y. H. Kuo, who was a student of H. S. Tsien, an early and eminently successful student of von Kármán. The return of H. S. Tsien to China in 1955 was a big scientific and political event. His student Y. H. Kuo followed him in 1956.

With his extraordinary background both in academic achievements and in industry, Bill brought a revolution in teaching methods to Cornell. The students of the graduate school gathered weekly at a research conference under his guidance, where they reported mostly on what they had "just found." Bill knew how to respond to such reports with gentle humor and how to move on to high-level technical discussions.

He also became a member of the editorial committee of the *Journal of the Aeronautical Sciences* for the field of aerodynamics. Under his leadership this journal became the top U.S. publication in its field. (He also headed the meetings committee, which was responsible for the regular annual meetings in New York City.) He became editor in chief of the journal in 1955, and saw its name and scope changed from "Aeronautical" to "Aero/Space" in 1959.

If one searches for a theme in the most important aeronautics publications of this era, one would find it is the incorporation of solutions for compressibility problems that involve the third dimension in their boundaries. Typical is a paper of 1948 by Bill entitled "The Boundary Layer of Yawed Cylinders." He occasionally mentioned jokingly that he had to share priority on this subject with Ludwig Prandtl and Robert T. Jones. Articles of this type in the journal had titles like "the theory of not-so-slender bodies and wings," and were often authored by graduates from the Cornell school.

The most important theme in Bill's research remained, however, in direct continuation of his thesis work with von Kármán, the incorporation of the time dependence in the description of the performance of airfoils. From the consideration of single airfoils he proceeded to the theory of airfoils in grid configurations, proceeding further from simple configurations to arrangements that are directly applicable to rotating machinery. Complicated phenomena like the rotating stall in axial compressors became thesis problems at the aero school, which maintained its top position in the aeronautical sciences. Graduating students stepped into leading positions in industry.

A further remarkable sign of the school's prestige was that it hosted a great number of scholars who spent their sabbatical leaves in Ithaca. The international list included Itiro Tani from Japan and George Batchelor from England. Theodore von Kármán became a frequent visitor and spent a term at Cornell in 1952.

Interest in a new subject in aeronautics evolved in the years leading to the Second World War, and it led after the war years to discussions on an international level. The original problem was to find a meaningful model for the flow past a slender and sharp-edged triangular (delta) wing, where vortex sheets originate at the edges. A successful model that emerged replaced a vortex sheet with a concentrated vortex singularity, which was tethered and fed by a thin sheet emerging at a sharp edge. Thanks to the superior guidance of Bill Sears, the most important U.S. contributions to solutions appeared in his journal.

The theme of Bill's lectures and of his writings showed a growing interest in the fundamental problems of airfoils in real fluids, where the Kutta condition provides the critical link between the ideal and the real fluid. Bill's paper, "Some Recent Developments in Airfoil Theory," which summarized the state of the art, appeared in a special issue of his journal in 1956, in honor of von Kármán's seventy-fifth birthday.

The launch of *Sputnik* in 1957 challenged the formerly undisputed leading role of the aeronautical sciences, and a new partnership with the aero/space sciences resulted. The change found Cornell well prepared. The first faculty of the aero school assembled by Bill Sears in 1947 already had a member who was a Columbia-educated physicist, Arthur Kantrowitz. He actually left Cornell in 1956 for a position in industry, but his student Edwin L. Resler, Jr., who was a research associate at the aero school from 1948 to 1951, received a doctorate from Cornell in 1951 and started teaching there in the same year as an assistant professor. Later in 1952 Resler moved to the University of Maryland but returned to Cornell in 1957. A close partnership developed between Bill Sears and Ed Resler, and a series of groundbreaking papers emerged from their collaboration. The authors finally agreed on the name "magneto-fluidynamics" for the new field that they pioneered. They had the opportunity to reformulate classical aerodynamic theory to solutions that are applicable to plasmas (i.e., to conducting fluids).

In the meantime, Bill's thoughts returned to the basic question of the efficiency of airfoils, and he developed a new idea that found its first written expression in a joint paper with his student Demetri P. Telionis in 1971. They maintained that a necessary prerequisite for the explanation of the time-dependent lift of airfoils was the understanding of the detailed structure of the flow connectivity (one could also say "topology") in the separation and transition region of the laminar boundary layer. The original paper can be found reproduced in a volume edited by one of Bill's outstanding students, Nelson H. Kemp (1927-1986), for the Cornell University Press. This volume also bears testimony to the details in the evolution of Bill's ideas and to the struggles of an indomitable spirit. A second version of the Sears-Telionis paper is much more easily accessible (1975). Although the final steps needed to connect the ideas of separation and lift remained elusive, further efforts based on these ideas are still alive and well.

In 1974 Bill and Mabel Sears moved to Tucson, where Bill joined the Department of Aerospace and Mechanical Engineering of the University of Arizona. He brought along the high prestige of a well-established reputation and official (Air Force and Navy) support for a large-scale research project. The project aimed at accurate testing in the wind tunnel by use of adaptive walls. Their shapes were calculated assuming the model in a free atmosphere, and a method was outlined that led to theoretical solutions in iterative steps. The idea is recognized as an important step on the way toward totally numerical methods.

The lifestyle of the Sears family continued on an even keel after the move. At the University of Arizona, Bill, who was an accomplished musician (percussionist, drummer, timpanist), played the recorder with the Collegium Musicum for 20 years. Flying remained the main hobby of the family.

Bill's successor as head of the Cornell school, Ed Resler, initiated (with great help from the school's administrative assistant Alice ["Toni"] Anthony) the establishment of the William R. Sears Distinguished Lecture. Beginning in 1985, this yearly event became an important scientific and social event for Cornell, and members of the Sears family flew to Ithaca for the occasion.

The collection and edition of the Sears papers remained a tradition in Cornell that survived after Bill's move to Arizona. Actually, however, the celebration of "Fifty Years of Aerospace Engineering at Cornell" in the fall of 1996 was preceded by a symposium honoring W. R. Sears on his eightieth birthday on March 1, 1993, thanks to the organization by a colleague of Bill in Arizona, K.-Y. Fung (who later also returned to China). This collection of papers by admirers, friends, colleagues, and students is now also part of the Sears legacy. It can be found with the title *Symposium on Aerodynamics & Aeroacoustics,* published in 1993 by the World Scientific Publishing Co. of Singapore and New Jersey. Also included in this collection are evaluations of some early ideas of Bill Sears that he had while working for Jack Northrop.

The collected papers of W. R. Sears after 1973 were published by the Cornell University Press in 1997. They also contained—in the tradition established by the first publication—a short biographical sketch.

Bill's retirement from flying in 1990 led ultimately to profound changes in the scheduling of his regular visits to Ithaca. He became ill in the fall of 2002 and died on October 12, 2002.

The personality of Bill Sears, and the guiding principles of his actions, remind his many admirers, friends, and colleagues of the heroic and idealistic characters of the American Revolution. Memorials in celebration of his life were held at Cornell and at the University of Arizona in Tucson. For the aero school in Ithaca (which merged in 1972 with the Sibley School of Mechanical and Aerospace Engineering), Director Sid Leibowitz decided to revive the tradition of the William R. Sears Distinguished Lectures at Cornell. In 2003 Mabel Sears from Tucson attended as honored guest, together with the Sears children, David (from Bethesda, Maryland), Susan (from Indianapolis), and several grandchildren. Mabel and the Sears children also initiated a series of Sears Memorial Lectures in Tucson, Arizona; the first was given in October 2003. On April 24, 2004, the Sears family reunion was repeated on the occasion of the Sears Distinguished Lecture at Cornell. On April 26 Mabel Sears died peacefully in her sleep at the age of 91, back in Ithaca, New York.

The autobiography of William Rees Sears, *Stories from a 21st Century Life*, was published in 1994 by Parabolic Press, Inc., of Stanford, California, and received favorable reviews of its literary contents. In particular, important parts of Bill's actions as chairman of the NATO (AGARD) Fluid Dynamics Panel committee were reported by an eyewitness: Dr. William J. Rainbird, a panel member who originally hailed from New Zealand. He published his experiences in a paper that appeared in the *Canadian Aeronautics and Space Journal* in 1994.

The memories of the creative work of Bill Sears in the years 1941-1946 for the Northrop Company's Lifting Wing are kept alive in the writings (mentioned earlier in this memoir) of Professor Frank E. Marble of the California Institute of Technology.

HONORS AND AWARDS

William R. Sears was the recipient of numerous honors and awards and was invited to present the most prestigious memorial lectures of the aeronautical sciences in the United States, in Britain, and worldwide. In particular, he delivered the Von Kármán Lecture of the American Institute of Aeronautics and Astronautics in 1968 and the Frederick W. Lanchester Memorial Lecture of the Royal Aeronautical Society of London in 1973. Invitations for lectures to be presented came from around the world: Israel, Japan, Italy, England, and the People's Republic of China.

Sears became a member of the National Academy of Engineering in 1968 and of the National Academy of Sciences in 1974, the most prestigious scientific society of this country. Further honors included the Von Kármán Medal of the Advisory Group for Aeronautical Research and Development (AGARD) in 1977, the Prandtl Ring in 1974, and the Daniel Guggenheim Medal in 1996.

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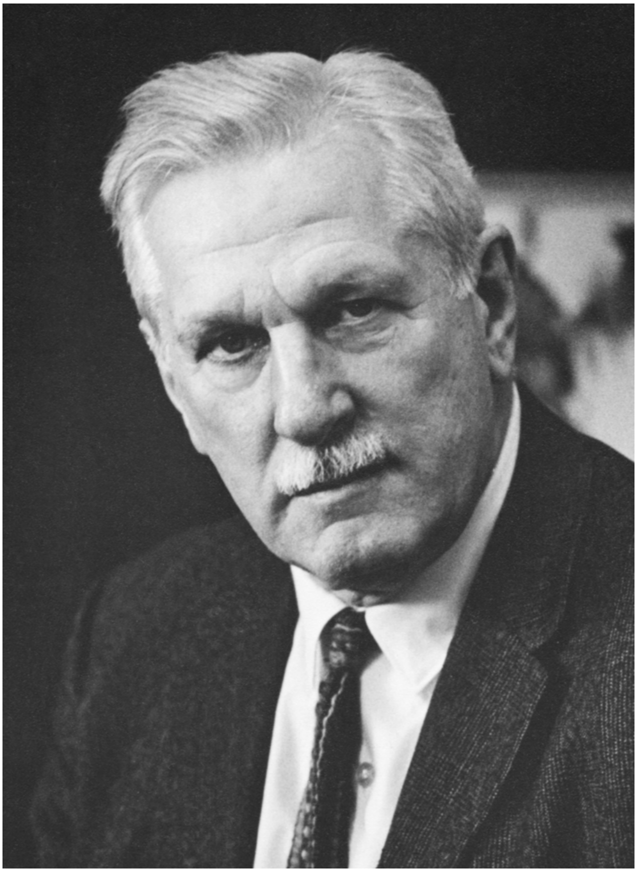

University of Wisconsin Archives

FOLKE KARL SKOOG

July 15, 1908–February 15, 2001

BY DONALD J. ARMSTRONG AND
ELDON H. NEWCOMB

FOLKE KARL SKOOG WILL be remembered as one of the twentieth century's major figures in plant biology. He was the last surviving member of the small group of investigators who began plant hormone research in this country, and his death in Madison, Wisconsin, at the age of 92 marked the end of an era. His personal contributions to the field of plant hormone research were monumental. Few single discoveries have had such a major impact on a field of plant science as did the isolation and identification of kinetin by Skoog and associates at the University of Wisconsin-Madison in 1955. This discovery was the founding event in the recognition of a major new class of plant hormones, the cytokinins, and it shaped and conditioned research in plant growth regulation for decades. Earlier Skoog had made several pioneering discoveries that helped to establish the general importance of auxin in plant growth regulation. During his multifaceted career he also investigated aspects of plant nutrition, advanced the science and art of plant tissue culture, and addressed a number of important questions in plant morphogenesis. His contributions to our understanding of the regulation of plant growth and development constitute a legacy that is rivaled by few others in the field. This legacy should also include the names of the many people whose lives he influenced and enriched.

Folke Skoog was born in Halland, Sweden, on July 15, 1908. His father was trained as an agronomist, and Folke's early years, when he was not in school in Uppsala, were spent on the large agricultural estate his father managed. Folke had a younger brother, Toord, who became a prominent reconstructive surgeon in Sweden. In 1925, at the age of 17, Folke came to the United States for what was originally intended as a one-year stay with an aunt residing in California. After enrolling in high school, he became interested in chemistry and decided to stay in the United States to pursue an undergraduate degree at the California Institute of Technology. Although he graduated from Caltech with a B.S. in chemistry in 1932, by that time his professional interests had shifted to plant biology. Nevertheless, his scientific thinking through the years was strongly influenced by this early training in the physical sciences.

The learning environment in which Skoog was immersed as an undergraduate at Caltech was truly remarkable. He had a chemistry course from Linus Pauling, physics from Robert Millikan, and philosophy from Bertrand Russell, but it was the association with a number of superb biologists that shaped his career. During his graduate student years at Caltech he was mentored by the geneticist Thomas Hunt Morgan, who had recently become head of the new Department of Biology; by Carl Lindegren, a graduate student who was later to become well known for his work on the genetics and biochemistry of *Neurospora* and yeast; and notably by Hermann Dolk, who had been induced to join Caltech to initiate research in the new field of plant hormones. Dolk was one of a number of gifted young scientists, including Robert Emerson, George Beadle, Kenneth Thimann, and Boris Ephrussi, who had been persuaded to join Caltech by T. H. Morgan. Skoog began working with Dolk on the regulatory effects of auxin on plant growth, but Dolk was killed in an auto accident before the project was completed.

Following Dolk's death, Skoog began working on auxin physiology and biochemistry with Kenneth Thimann, a young plant biochemist from England who was also to become renowned for his research on plant hormones. This early association with Thimann was the beginning of a close lifelong friendship between the two men. When Thimann left Caltech for a position at Harvard, Skoog finished his graduate work under the direction of Dolk's successor, Frits Went, who through the discovery of auxin had launched the field of plant hormone research. During the years Skoog always remembered the kindness Went had shown him when he was a young graduate student trying to survive and carry on his studies during the Great Depression.

Skoog became a naturalized citizen of the United States in 1935. On receiving his Ph.D. in biology from Caltech in 1936, he was awarded a National Research Council Fellowship to work with Dennis Hoagland in the Division of Plant Nutrition at the University of California, Berkeley. In Hoagland's laboratory Skoog worked most notably on the effects of zinc on auxin metabolism, an experience that provided him with bench knowledge of research on mineral nutrition, which he applied repeatedly in later years. At Berkeley he also investigated the regulation of bud dormancy in woody perennials under the direction of J. P. Bennett.

Skoog accepted a position with Thimann as a research associate and instructor at Harvard University in 1937. With the exception of a six-month sabbatical in 1938 as a visiting scientist at the Pineapple Research Station of the University of Hawaii, he continued at Harvard until 1941, when he accepted a faculty position at Johns Hopkins University. At both Harvard and Johns Hopkins he focused on auxin metabolism and action and initiated experiments with plant tissue culture systems. While at Johns Hopkins, he contributed to the war effort by participating in a National Institutes of Health study of medical problems arising from the handling of TNT in munitions manufacture and developed assays to test the efficacy of various antifungal compounds. From 1944 to 1946 he served with the quartermaster general's office in the Defense Department as a chemist and technical representative attached to the U.S. Army in Europe. At war's end he was assigned to investigate Germany's wartime research on the production and use of yeast as a food source.

Skoog returned to academic life in the United States at the end of 1946 with a brief stay in Carl Lindegren's laboratory at Washington University in St. Louis. In 1947 he accepted a faculty position at the University of Wisconsin-Madison. There he began the work with tobacco tissue culture systems that led ultimately to the discovery of kinetin and the founding of the field of cytokinin research. He remained at the University of Wisconsin as a faculty member of the Department of Botany until his retirement in 1979.

During his career Skoog authored more than 170 scientific publications and trained more than 60 graduate students and more than 40 postdoctoral associates. His laboratory was always an international center of intellectual activity, attracting students, postdoctoral associates, and visiting scientists from around the world. Throughout his long and distinguished career he received numerous honors and awards, including the Stephen Hales Award of the American Society of Plant Physiologists in 1954 and the Award of Merit of the Botanical Society of America in 1956. He was elected to the National Academy of Sciences in 1956. He served on numerous national panels and study sections, and as president of several professional societies, including the American Society of Plant Physiologists in 1957, the American Society of General Physiologists in 1957, the Society for Developmental Biology in 1970, and the International Plant Growth Substances Association from 1979 to 1982. Many other honors followed in later years, including memberships in foreign academies, several honorary degrees, and award of the National Medal of Science in 1991.

INVESTIGATIONS OF AUXIN METABOLISM AND PHYSIOLOGY

The Stephen Hales Prize was awarded to Skoog for "outstanding contributions to research in the physiology of auxins, the development of plant tissue cultures, and the physiology of fresh water algae." The revolutionary discovery of kinetin was yet to come, but he had already made major contributions to research on the regulation of plant growth and development. As an undergraduate and graduate student working with Thimann at Caltech, he had shown that auxin activity could be extracted from plant sources other than the tips of coleoptiles, and that auxin applied to the cut surfaces of decapitated dicotyledonous seedlings could maintain apical dominance by substituting for the effect of the terminal bud in inhibiting the outgrowth of lateral buds. This result provided evidence that in addition to promoting elongation in cereal coleoptiles, auxin had more general effects, and also led eventually to recognition that this hormone was broadly involved in the regulation of plant growth and development.

At Harvard Skoog had worked with Thimann on the development of methods for the quantitative extraction and estimation of free and bound auxin levels in plant tissues. He demonstrated that cultured tissues from the plant tumors that arise spontaneously on certain hybrid tobacco plants (derived from *Nicotiana langsdorfii x N. glauca*) produced high levels of auxin in culture. Subsequently, at Johns Hopkins, following up on the observation of Philip White that cultured tissues derived from the *Nicotiana* tumors sometimes produced shoots when the tissues were grown submerged in liquid culture, Skoog demonstrated that addition of auxin to the culture medium completely suppressed shoot formation. Additionally, he was able to root some of the shoots that developed in culture, providing the earliest demonstration of the regeneration of a complete plant from a callus tissue.

DISCOVERY AND STRUCTURAL STUDIES OF CYTOKININS

In 1947, shortly after his arrival at the University of Wisconsin, Skoog developed a plant tissue culture system that he derived from stem tissues of *Nicotiana tabacum* cv Wisconsin #38. With Cheng Tsui he demonstrated that stem segments from this cultivar formed abundant callus tissue on a medium containing auxin; however, the callus tissue that formed on the segments could not be subcultured on the same medium. Moreover, if pith tissue from the center of the tobacco stem, instead of complete stem segments, was placed on the medium, no cell division or callus formation would occur. By the early 1950s Skoog's laboratory had demonstrated that cell divisions could be induced in tobacco pith tissue and an indefinite proliferation of callus tissue could be achieved by adding certain complex natural products to the medium. Coconut milk, malt extract, and yeast extract each induced cell divisions in tobacco pith tissue when added separately to a medium containing auxin.

The isolation of the substance(s) in yeast extract responsible for stimulating cell division in plant tissues was undertaken by Skoog's postdoctoral associate, Carlos Miller. Beginning with evidence that the active substance in yeast extract had the properties of a purine, Miller found that an old commercial preparation of herring sperm DNA was highly active in promoting the cell division. Although new preparations of DNA were inactive, Miller soon found that cell division could be induced if the DNA preparations were autoclaved in weakly acid solutions. Late in 1954 Miller succeeded in purifying the responsible compound in these partially degraded DNA preparations. The compound was identified as 6-furfurylaminopurine (i.e., N^6-furfuryladenine), and its structure was confirmed by synthesis in collaborative work with Frank Strong and his associates in the University of Wisconsin Department of Biochemistry. Although kinetin (the trivial name given to the compound) has never been shown to occur naturally, it was nevertheless the first example of a major new class of plant growth substances that came to be known as cytokinins. The first analog of kinetin (N^6-benzyladenine) was quickly synthesized in Strong's laboratory and proved to be even slightly more active in inducing cell division than kinetin itself. Another 40 compounds were subsequently synthesized by Strong and his associates and were tested in Skoog's laboratory. Twenty-one of these brought about some degree of cell division in the tobacco pith bioassay system. The generic name "kinin," originally proposed for this new class of plant growth regulators, was later changed to "cytokinins" to avoid confusing the plant compounds with the kinins that stimulate smooth muscle contraction in animals.

The discovery of kinetin sparked intensive efforts to isolate and identify a naturally occurring compound with equivalent activity in promoting cell division. It is illustrative of Skoog's creative approach to problems that in the attempt to isolate a cell division factor from pea seeds, he arranged to obtain 2,000 gallons of blanch water from a nearby Green Giant cannery and had it transported to the university in a milk truck and taken to dryness in the powdered milk facility in the university's Dairy Department. However, success was finally achieved in 1964, when D. S. Letham in New Zealand and, independently, Carlos Miller at Indiana University, isolated from corn kernels a compound that possessed equivalent activity in promoting cell division. The compound, termed "zeatin," proved to be N^6-(*trans*-4-hydroxy-3-methyl-2-butenyl)adenine, a close relative of kinetin.

Discovery of the cytokinins led to a long and highly productive collaboration and friendship between Folke Skoog and Nelson J. Leonard, the eminent natural products chemist at the University of Illinois at Urbana. During many years Leonard's group synthesized hundreds of possible cytokinins and antagonists that were then tested for cytokinin activity in Skoog's laboratory in order to establish the principles governing the relationship between structure and activity. Several active compounds were isolated from the plant pathogen *Corynebacterium fascians*, one of which was identified as N^6-(Δ^2-isopentenyl)adenine. Much of the structure and activity work in Skoog's laboratory was carried out by Ruth Yates Schmitz, one of Skoog's first graduate students, who returned as a research associate in 1967 and remained until his retirement. Her careful work contributed to a steady stream of publications describing in detail the structure-activity relationships of cytokinin-active compounds and antagonists.

HORMONAL REGULATION OF PLANT MORPHOGENESIS

Through experimentation with tissue cultures Skoog and his associates established that contrary to the opinion prevailing at the time, plant growth and morphogenesis are controlled by complex interactions of multiple plant hormones in which both the relative and absolute amounts of these substances are important. This theme first emerged in Skoog's early work on the control of shoot formation in plant tumor cultures. As early as 1951 he and Tsui noted that although Sachs's old concept of specific organ-forming substances had enjoyed a recent revival, "The results we have obtained are in disagreement with such concepts. On the contrary, our findings suggest that both organ formation and subsequent development are brought about by quantitative changes in amounts and interactions between nutrients and growth factors which are essential for growth of all cells, so that the pattern of development is determined by the relative supplies . . . of these materials at particular loci."

After the discovery of kinetin Miller and Skoog showed that it was possible to control organ formation in tobacco tissue culture by manipulating the levels of auxin and cytokinin in the culture medium. Subsequent studies in the Skoog laboratory demonstrated that by appropriate sequential manipulations of the medium it was possible to control to a remarkable degree the formation of organs and the complete regeneration of whole tobacco plants from undifferentiated callus tissue. This result also proved to be applicable to a number of other tissue culture systems and finds important applications today in many of the strategies used in plant genetic engineering. Murashige and Skoog examined and optimized the inorganic nutrients required by tobacco tissue cultures. They showed that much of the growth stimulation observed when extracts from various natural sources are added to the culture medium are attributable to relatively nonspecific effects arising from the use of suboptimal levels of inorganic nutrients. The Murashige and Skoog medium, published in 1962, is now a standard commercial product widely used for plant tissue culture.

Customarily a number of different systems and morphogenetic problems were under simultaneous investigation in

Skoog's laboratory. During his career he worked with algae, mosses, ferns, bacteria, fungi, a variety of seed plants, and even some animal systems, in addition to the tobacco tissue culture system for which he is best known. The list of authors on his publications falls far short of reflecting all who were trained in his laboratory. For a variety of reasons a number of interesting studies by his students failed to result in manuscripts, many surviving only in thesis form, but all of the work was an important part of the internal heritage of the group and was discussed and passed on to newcomers as part of the intellectual framework upon which the group relied.

Skoog was one of the first to suggest that plant hormones might be affecting growth and morphogenesis by mechanisms associated with nucleic acid metabolism and protein synthesis, and during the 1950s he and his coworkers published a number of papers relating auxin and cytokinin to cell division, nucleic acid content, and DNA synthesis. When the cytokinin-active nucleoside N^6-(Δ^2-isopentenyl)adenosine was reported to occur in hydrolysates of yeast and calf liver tRNA and found adjacent to the anticodon in serine tRNA from yeast, he mobilized the laboratory to investigate. Bioassays of hydrolysates of tRNA prepared from a wide range of plant, animal, and microbial sources revealed that cytokinin-active nucleosides were almost universally present. In collaboration with Nelson Leonard the cytokinin-active constituents of a number of tRNA preparations were isolated and identified. The results, together with collaborative studies with several other laboratories and with the independent work of S. Nishimura in Japan, established that the distribution of cytokinin-active nucleosides with respect to individual tRNA species was related to the genetic code. Cytokinin-active nucleosides were found to be restricted to tRNA species that responded to codons beginning with U. Subsequent results with tRNA preparations from plants, yeast, and *Drosophila* established that the distribution of cytokinin-active nucleosides within the U group of tRNA species was more restricted in eukaryotes than in prokaryotes, but the cytokinin modification was always present in serine and leucine-tRNA species responding to U codons and always occurred adjacent to the anticodon. Although attempts to link these observations to the mechanism of hormonal action of cytokinins in plant systems were not successful, the work contributed substantially to our knowledge of the occurrence and distribution of hypermodified bases in tRNA molecules.

CONTRIBUTIONS TO THE UNIVERSITY OF WISCONSIN-MADISON COMMUNITY

At the University of Wisconsin the World War II years had left a depleted Botany Department no longer at the forefront of botanical research. Following the war, with the encouragement of the university administration, the department sought to appoint an outstanding young scientist who would provide leadership through a program of basic research in plant physiology and also establish strong ties between botany, located in the College of Letters and Science, and the numerous related departments in the College of Agriculture. Folke Skoog's appointment as an associate professor in 1947 proved to be an ideal decision, as his impact on the Madison campus was immediate and huge. He quickly developed personal relationships with leading figures in both colleges, the Medical School, and the university administration, and provided advice and established numerous collaborations that often ranged considerably beyond his own primary research interests. It is no exaggeration to say that he was responsible for rejuvenating and modernizing the Botany Department and giving it strong campus leadership in basic research in the plant sciences.

He contributed to the scientific life of the university in many ways, and sometimes on issues that required considerable courage. He was the initiator and prime mover in the establishment on the campus of the Biotron, one of the very few facilities in the country for the study of plants and animals under controlled conditions. Shortly after his arrival on campus he undertook the overall direction of a comprehensive long-term investigation of the nutritional requirements and possible methods of control of the noxious blooms of blue-green algae in the local lakes resulting from lake eutrophication. Under his supervision G. C. Gerloff and G. P. Fitzgerald successfully pursued algal studies from 1950 to 1957. One notable outcome of the work was the demonstration in 1954 by Holm-Hansen, Gerloff, and Skoog that cobalt is an essential element for the growth of blue-green algae.

It was also owing to his initiative and persistence that the highly successful Biocore program was established on the campus in the mid-1960s. In his passionate advocacy of the program he argued persuasively with numerous colleagues across the campus that biology majors would be much better educated if they first received an adequate background in physics and chemistry and then, building on this foundation, took courses in biology in a logical, structured sequence. The enduring success of this program on the Madison campus, for its initiation he was solely responsible, remains a lasting tribute to the soundness of his convictions and his leadership.

Shortly after his arrival in 1947 Skoog began to urge curricular reform in the Botany Department itself (e.g., in the approach and emphasis used in presenting the major introductory course in botany and in the course requirements for M.S. and Ph.D. candidates). He also took a strong stand regarding the kinds of faculty appointments and the criteria to be used in judging candidates. These vigorously argued proposals understandably did not sit well with several members of the old guard, who felt their disciplines threatened, and resulted literally in years of acrimonious staff meetings. Although Skoog in time came to feel that the internecine warfare was too hard on his health, and in the early 1960s permanently stopped attending staff meetings, in fact, he did achieve his objectives; all the positions he advocated were eventually adopted, resulting in a much stronger and more competitive department.

PERSONAL REMINISCENCES

Folke Skoog was 38 years old and already had an international reputation when he arrived on the Wisconsin campus in January 1947. As one of us (E.H.N.) well remembers, he was energetic, intense, and inquisitive, and showed a warm personal interest in virtually every student or associate who came his way, drawing us all into his circle of influence. If he hadn't seen us for a day or two, he would look us over quizzically and ask how we were doing, and would often respond to our replies with lightning-quick witticisms, his good-humored banter conveying genuine interest in our well-being. He was remarkably accessible, especially in those early years, and totally devoid of airs. Although I was not his student, I felt welcome to drop by his office at any time and receive a sympathetic ear and perceptive advice. As an example of this accessibility, and incidentally of his mental powers, I recall visiting him in his office as a graduate student in 1948. I was taking a course entitled "The Characterization of Organic Compounds" at the time, and in response to his interest, showed him some of the more difficult textbook problems, where the structures of unknown compounds had to be determined from their molecular formulas, solubilities, and reactions with reagents. Folke rose to the challenge and before long had worked out several of the identifications, although it had been at least 15 years since he had taken his courses in organic chemistry.

Folke was an active man possessed of a rather tall and athletic frame. In his youth in Sweden he had played soccer and a Scandinavian game similar to ice hockey. In college he became interested in track and enjoyed considerable success as a runner. At age 24 he represented Sweden in the 1932 summer Olympics, finishing sixth in his heat in the 1,500-meter race. In later years he claimed, with a twinkle of the eye, that he should have won, since he had beaten the winning time in trials. Nevertheless, he felt it was probably best that he had not won an Olympic medal; otherwise, he might have become a "track bum." Amusing as well is a story he told of missing the question "Who is Babe Ruth?" on the Caltech entrance exam. Being a recent immigrant to the United States, he hadn't known the answer at the time, but he thought this was "a good question, considering the number of bookworms who applied for admission."

He retained his interest in track and other athletics throughout his life, and became a close follower of the football fortunes of the University of Wisconsin Badgers and the Green Bay Packers. In the late 1940s and for several years into the 1950s he regularly swam for some distance along the shore of Lake Mendota during the noon hour. For years he also participated vigorously in the football games at laboratory picnics. He took great pleasure in the laboratory's social get-togethers, and entered into the sports and general badinage with zest, adding much to everyone's fun. His recreational interests, especially in later years, also included regular poker sessions with a select group of university friends.

Humor was always close to the surface with Folke. His quick mind was never short of quips and humorous associations. As a native of Sweden, he had a particular fondness for obscure ethnic jokes about Norwegians, and he delighted in bringing these to the attention of Norwegian friends on campus. He was just as happy, however, to join in the laughter when the joke was on him, a situation his friends tried to achieve as often as possible.

As mentor, Folke Skoog on occasion could be an intimidating presence. One of us (D.J.A.) can testify from personal experience that it was always with some trepidation that a young graduate student or postdoctoral associate approached his office to report a disappointing result or some experiment gone awry. On these occasions he would listen carefully, with one hand on his chin, now and again commenting, "Is that so!" Finally, at the end of the recital the head would cock to one side, the reading glasses would come down on the nose, and for one long unnerving moment the intense blue eyes would peer at the uneasy student over the frames. Then, more often than not, a soft chuckle would emerge and he would lean back in his chair and offer some ambiguously reassuring comment, a favorite being "Results always seem to follow the principle of maximum human unhappiness."

The Skoog laboratory at Wisconsin was the site of continuous intellectual traffic, with most of the leading figures in plant physiology visiting at one time or another. They were always brought to the afternoon coffee breaks to meet and talk informally with everyone. These coffee breaks were social occasions that included also the personnel of the laboratories of Paul Allen, Jerry Gerloff, and one of us (E.H.N.). The Skoog group, usually comprising a dozen or so, had a distinctly international flavor, with students and postdoctoral associates from around the world. Each member had a coffee mug emblazoned with a personal symbol. When the member left the group, the cup was "retired" by hanging it from one of the overhead pipes in the laboratory, so that in time, scores of colorful mugs dangled from above, nostalgic reminders of the group's continuity.

The goal of the group's weekly research meetings was always to dissect reports on original research or on the literature to examine closely what had been done and to ferret out the flaws. These sessions could be punishing experiences for the unprepared, and the training in evaluating the literature and one's own experiments was invaluable. Folke himself was seldom one of the more aggressive participants, and he usually spoke in a low voice so that listeners had to strain to catch his words. But those who paid close attention were well rewarded; not only did he have a vast knowledge of the literature on plant growth substances but he was also exceedingly careful and rigorous in examining experimental data. With his standards and comments shaping and informing the discussions, it was a lively, exciting, and demanding environment in which to acquire a graduate education.

Folke Skoog was outspoken in defending his strongly held principles and opinions, and was widely known for his love of intellectual battle and his willingness to engage the opposition. Whether the topic was science, society, politics, or philosophy, he always brought a unique perspective and a penetrating analysis to the discussion. One could disagree with Folke's position, yet appreciate that his reasoning illuminated and deepened the discussion. He disliked lavish praise and was never effusive in commenting on others, even those whom he held in the highest regard, but associates knew they could place great confidence in a few terse, commendatory words. He had little patience for those in the profession he regarded as pompous or too highly impressed with their own stature, and he delighted in delivering a humorous or caustic remark that punctured the arguments of the pretentious. However, the repartee was almost always delivered in good humor.

Considering his own intellectual gifts, it might come as a surprise to those who did not know him well to learn that he was extraordinarily considerate of and patient toward the occasional student whose abilities were somewhat dubious, if he believed the aspirant was doing his or her best. The traits he absolutely would not tolerate were laziness of habit or carelessness in conducting research. He also frowned on any tendency to exaggerate claims of past accomplishments or the significance of research results. In such cases he could be blunt in assessing a student's deficiencies in behavior and performance.

Owing to his personal magnetism and the challenging nature of his research projects, throughout his career Folke Skoog proved to be highly successful in persuading talented individuals from many different disciplines to collaborate with him. When asked late in life to what he attributed his success, he generously acknowledged this, and was also characteristically self-effacing: "My success, to the extent I may have had some, and considering only professional aspects, must be ascribed mainly to my opportunities to have had a large number of very capable people in different disciplines helping me." In addition, he said modestly that he believed he had had "a fairly long nose in smelling out problems, and blind perseverance in trying to bring matters to a conclusion."

Throughout his years at Wisconsin Folke was fortunate to have the quiet strength and affection of his wife, Birgit. Dealing with the sometimes raucous group of students, friends and associates that surrounded him could not always have been easy, and the patience and good humor with which she dealt with this large and unconventional extended family and its somewhat eccentric head were admirable. Their daughter, Karin, occupied a special place in the lives of both, and in old age Folke took great interest and pride in the emerging intellects of his three grandsons. Though he suffered from vascular problems and was in poor health in his last years, he was sustained by Birgit's care, and remained clear and sharp of mind to the end of his life.

Folke's strength of intellect and character, his personal charm and special foibles, and the ongoing interest he showed in his friends created deep bonds of affection that linked him to his former students and endeared him to his many friends around the world. In the years after his retirement, many of these traveled to Madison to see him and Birgit, or phoned and wrote regularly. He was a remarkable person and an exceptional scientist who greatly enriched the lives of the many who were fortunate to be his friends.

PART OF THIS MEMOIR has been adapted from *Folke K. Skoog: In Memory and Tribute* by Donald J. Armstrong (*J. Plant Growth Regul.* 21[2002]:3-16). We thank Carlos Miller and Ruth Schmitz for their helpful comments on the memoir.

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Hiroshi Tamiya

HIROSHI TAMIYA

January 5, 1903–March 20, 1984

BY ANDREW A. BENSON

HIROSHI TAMIYA'S PILGRIMAGE IN BIOLOGY

HIROSHI TAMIYA WAS BORN in 1903 into a family of medical doctors, having been, since the sixteenth century, poets and physicians attending feudal lords in the Koochi (Tosa) Prefecture on the island of Shikoku. His father, Koreharu, a learned man, had received his western medical instruction from a Dutchman, Dr. C. Elmerence, one of the few knowledgeable sources at that time. Hiroshi's brother Takeo, 14 years his senior, was a professor emeritus of the University of Tokyo and president of the National Cancer Center. Their mother's ancestors can be traced back to the twelfth century as feudal lords in Iyo (Ehime Prefecture), who were defeated by Toyotomi, builder of the Osaka castle. They escaped to Tosa to become a samurai family. The young Hiroshi received much stimulus from Takeo as well as impetus toward the life of a scientist. As it was, and still is, the families of medical doctors want their sons to become medical men. Hiroshi Tamiya began his pilgrimage in science to become a medical man. Daunted, however, by the shock of seeing anatomical dissection of human bodies in the Faculty of Medicine, he gave up the study of medicine against the wishes of his late father and selected the course of botany (although he had no interest in the study of higher plants). Plant taxonomy and morphology bored him; he concentrated more on music, playing the cello, and preferred the study of physiological processes and functions. From his childhood he was interested in microorganisms or any cells that he could observe with his father's German-made microscope. This became the inspiration for him to study microbiology and cell physiology.

An unexpected fortune awaited Tamiya in his second year at the Department of Botany of Tokyo University. It was the opportunity to attend the lectures of Professor Keita Shibata, then the most outstanding plant physiologist in Japan. There was other timely good fortune. World War I ended in 1918, and there appeared, mainly from England and Germany, a stream of papers and books by leading scientists dealing with cell physiology and biochemistry. After his graduation in 1926, by virtue of his teacher's thoughtful recommendation, he was accepted as a member of the Tokugawa Institute for Biological Research, founded by Marquis Yoshitake Tokugawa. Apart from his ordinary university duties, he was able to spend much of his energy and time working freely in this relatively richly equipped and generously supported laboratory. After some preliminary physiochemical work, he decided to investigate phenomena that are fundamental and common to all living organisms, such as growth and cell proliferation, respiration, and energy metabolism. Thus, before and during the so-called "Chinese Incidents" followed by the Pacific War, he published a series of studies on respiration and energy metabolism during the growth of cells, using the mold *Aspergillus oryzae* as experimental material. His originality became apparent with his unique way of devising methods and apparatus adapted to gain his own experimental objectives. He constructed a special calorimeter combined with a respirometer with which he measured the heat produced and oxygen consumed during the growth of the fungus and demonstrated that the total heat liberated was greater than the heat produced by respiration alone. He concluded that the respiratory process consisted of two parts, one quantitatively related to the growth process and the other related to maintenance of the preexisting cells. It was just at the time when heme-containing respiratory enzymes were discovered by Otto Warburg in Berlin and cytochromes were found in various aerobic cells by D. Keilin in England.

PARIS, 1934

Hiroshi and Nobuko Tamiya lived in Paris during 1934. In the Institut du Biologie Physico-Chemique, of which René Wurmser was the director of the Départément Biophysique, Hiroshi pursued the subject of interrelations of oxidation and reduction pathways in bacteria and microalgae. He translated Wurmser's "Les potentials d'oxido-réduction" into Japanese. Nobuko earned the Diplom de Cordon Bleu. In the difficult years after the war when Hiroshi's salary, as a professor in the Botany Department of the University of Tokyo, was the equivalent of $50 a month, Nobuko became the major breadwinner for their little family by scheduling French cooking classes for 30 to 50 young ladies in their home, where her kitchen facilities could accommodate 6 to 10 students each day of the week.

Hiroshi's enthusiasm for the concepts of Heinrich Wieland in Freiburg and René Wurmser in Paris clearly influenced the direction of his research. From the period of his cytochrome studies (1928-1939) his attention moved from the heat of combustion balance sheet to the mechanisms underlying the oxidations and reductions involved. He applied reaction kinetics, his unique way of approaching a problem. Using a single-hand spectroscope, he measured kinetics of the observed changes in bacterial species and noted their differing patterns. This discovery was later followed by the works of his admired friend Martin Kamen: isolation and structure determination of bacterial cytochromes. His ardent admiration for Professor Otto Warburg remained an unalterable model for his spirit as a scientist. Warburg's development of the study of photosynthesis influenced him, and in 1941 Tamiya began his own investigations of photosynthesis. He applied inhibitors of photosynthesis as a rigorous quantitative tool. Six years earlier Constance Hartt in Hawaii had independently initiated use of such tools in deducing the nature of the compounds involved in photosynthesis of sucrose in the sugar cane leaf (Benson et al., in preparation).

Tamiya's 1942 paper revealed little change in approach to consideration of the energetic relation between growth and respiration applied in his 1935 paper in *Actualitiés Scientifiques et Industrielles.* For comparison, the report on generation and metabolic utilization of phosphate bond energy in the previous volume of F. Lipmann (1941) applied superbly the thermodynamic relationship, $\Delta F = \Delta H - T \Delta S$.

René Wurmser published the volume *Oxydations et Réductions* in 1930, wherein he referred to the classic 1923 work of Lewis and Randall, *Thermodynamics.* Wurmser's book utilized its concepts of atomic structure and thermodynamic energy expressed in the three laws of thermodynamics. It appears that Tamiya's 1935 and 1942 publications did not recognize the importance of thermodynamics in the study of biochemical transformations.

FLASHING LIGHT EXPERIMENTS

Tamiya continued his investigation of the mechanism of photosynthesis using *Chlorella,* the alga used by Otto Warburg. The most extensive study involved measuring photosynthesis under intermittent light (1948). Discrimination between the "light" and the "dark" steps of photosynthesis was carried out using “pre-illumination” techniques. These permitted elucidation of the modes of inhibition of photosynthesis by several “poisonous” substances, including oxygen gas. (Discovered by Warburg, 1919; confirmed by Wassink et al., 1938, by Tamiya and Husizige, 1949, and by Calvin and Benson, 1948.)

Since Warburg’s work, it had been shown repeatedly that the dark reaction of photosynthesis shows a characteristic temperature-rate relationship not in accordance with the well-known Arrhenius theorem. It had been inferred that the reaction involves two consecutive steps. Temperatures, pH, and pO_2 effects were used to identify sites of inhibition by inhibitors in each of the two steps (1948).

OXYGEN INHIBITION OF PHOTOSYNTHESIS

Tamiya’s masterful application of the kinetics of photosynthesis had delineated the three processes in “the dark reaction.” The 1941 kinetics study of the dark reactions of photosynthesis led to a major publication in 1949 by Tamiya and Huzisige. Here they clarified the inhibition of photosynthetic productivity by oxygen at high light intensity. They postulated that oxygen attacks the primary step in one of two dark reaction components not inhibited by cyanide, the mechanism in which carbon dioxide is combined with a certain component in the cell. They referred to this as the “Ruben enzyme.” It led to conclusions regarding the binding of CO_2 by a receptor compound. This was fortunate since Tamiya’s initial acceptance of Ruben and Kamen’s premise of reversibility of $CO_2 + RH \rightarrow RCOOH$ was actually misleading. The Rubisco-catalyzed reaction is irreversible (Cleland et al., 1998; Benson and Cleaves, unpublished).

Thus, Tamiya and Huzisige presented evidence for a Rubisco-type reaction involving CO_2 and O_2 fixation. Tamiya wrote, “It is our great regret that fate did not allow us to pursue this experiment owing to the unfortunate destruction of cyclotrons in our country." This conclusion led Hiroshi and Nobuko Tamiya to travel to Berkeley in 1952 to test it with an experiment with $C^{14}O_2$ in the old Radiation Laboratory. Thus, a plan conceived in 1940, for experiments with $C^{11}O_2$ to be produced by the Nishina cyclotron in Tokyo, was finally executed in this author's laboratory a few feet from the original 37-inch cyclotron of Ernest O. Lawrence. That was one year after ribulose diphosphate was identified (Benson, 1951) and found to be susceptible to oxidation by oxygen in vitro. The results corroborated the previously observed glycolate production by photosynthesis with $C^{14}O_2$ in the presence of air (oxygen).

TOWARD A RENEWAL OF JAPANESE SCIENCE

The problem of reorganizing science for the postwar world was universal. German science had collapsed under Nazi control and many scientists had fled. Japanese science, dominated by German influence, was in disarray. U.S. science was concerned with the relation between science and government and the questions of civilian control of atomic energy and the future of basic research. Maintaining government support for the scientific enterprise and keeping scientific research insulated from government control and protected from politics were incompatible goals. Vannevar Bush's report, "Science—The Endless Frontier," clearly stated the ideal.

MIT physicist Harry Kelly was assigned by the General Headquarters, Supreme Command of Allied Powers, under General MacArthur to its Economic and Scientific Section (Yoshikawa and Kauffman, 1994; Dees, 1997), which a few weeks before had been embroiled in the embarrassment of the destruction of the cyclotrons of Professor Nishina (laboratory of Niels Bohr) and Ryokichi Sagane (who had worked with Ernest Lawrence in Berkeley), deeply embittering

Japanese scientists and the public. Kelly deftly overcame local resentment to earn the respect of chemistry professor Juro Horiuchi, who recommended he meet his close friend Hiroshi Tamiya. They met with Kelly in the spring of 1946, and Tamiya advocated a carefully planned process, one that began with an open forum of scientists from all fields to discuss aspects of the Japanese scientific research system and specific problems affecting each discipline. The necessary first step, according to Tamiya, would be the organization of a conference to launch the initiative. "You're right," exclaimed Kelly. "So you must organize the conference. I leave everything to you." Intimidated at first, Tamiya later recalled that Kelly induced him to participate by offering him one rare U.S. cigarette after another. Finally, Kelly's "Three Musketeers" provided the basis for effective interactions with Japanese scientists and their government. At first there were problems. Kelly requested that Tamiya provide the names of 20 Japanese scholars who were active in the natural sciences to him within two weeks. Tamiya was offended at Kelly's impatience. "Please wait a minute," he told the American. "Suppose that Japan had won the war and I was assigned to Washington to sit as you are sitting here today. What would you do?" Kelly was upset by what he considered a brash response. Years later he told Tamiya, "I was truly vexed by your 'suppose Japan had won the war' comment. But later, you gave me food for thought. It made me like you and trust you."

Intent upon building lasting support, Tamiya proceeded cautiously. He organized a forum for Japanese scholars, primarily scientists to discuss representation in exchanges with the occupation forces. His creation of a liaison group provided an opportunity to affect permanent change in the organization of Japanese science. Kelly worked behind the scenes with Tamiya. He suggested that Tamiya write a letter from the Japan Association of Science Liaison to the U.S. National Research Council. The purpose of the letter would be to explain the situation in Japan and ask for the NRC's help. "I think American scientists know nothing of the problems except what is in the papers. I will go back to the NRC and ask them if they will help." Tamiya agreed, but only after intense discussion about which group represented Japanese science and its reorganization. They agreed that reorganization was necessary and could be carried out, even though the task was more difficult from the Japanese perspective than from Kelly's.

The next day Tamiya delivered to Kelly the draft of a letter addressed to the National Research Council. Signed by Kaya, Sagane, Tamiya, and 17 other members of the Japan Association for Science Liaison, the letter was sent on July 11. Tamiya spelled out the functions and mission of the association, including its desire to contribute to the rehabilitation of Japan and to improve its traditional systems for the organization of science. He pointed to the scientists' dissatisfaction with "the past impotency and clumsiness of our government authorities in utilizing and respecting scientific talents" as well as to the shortcomings of academic scientists' penchant for what he called "academic fogydom." He spelled out the deplorable material conditions under which the Japanese scientific community was struggling to resuscitate its research efforts and then to focus those efforts on improving the civilian standard of living. The letter sharply criticized Japan's traditional power structure, condemned its "thoughtless and erroneous war" into which these powers had led the nation, and expressed aspirations for a "new Japan which will contribute to the World's Peace and Humanity." Tamiya's letter reflected the views of many progressive intellectuals who believed that in the new democratic postwar order, Japan would demonstrate that a modern industrial nation could exist without arming itself. This view had been given expression in the country's newly adopted constitution, which renounced all war-making activity. By the middle of 1946 Kelly's mission had clearly changed from surveillance to mediation and friendly guidance.

Kelly succeeded in interesting Frank Jewett, president of the U.S. National Academy of Sciences, in organizing a committee of prestigious scientists and engineers to review the Japanese situation. Jewett obtained funding from the Rockefeller Foundation for a six-week mission and recruited six academy members who were experienced in dealing with "people and problems at top level": Roger Adams, head of the chemistry department at the University of Illinois, leader of the group; W. V. Houston, president of Rice Institute; W. D. Coolidge, director emeritus of research at General Electric; W. J. Robbins, director of the New York Botanical Garden; Royal Wasson Sorenson, head of the department of electrical engineering at the California Institute of Technology and past president of the American Institute of Electrical Engineers (honored by the Japanese Institute of Electrical Engineers who designated Professor Sorensen an honorary member); and Merrill Bennett, executive director of the Food Research Institute at Stanford University. Bennett, an economist representing the social sciences, wrote the group's final report. They arrived in Tokyo on July 19, 1947, and stayed until August 28, adding their goodwill and prestige to the reorganization effort. In their report to General MacArthur they advocated a program of reorganizing science in its broadest sense, including the social as well as the natural sciences. Kelly later made arrangements for a second science advisory group to be appointed by the National Academy of Sciences. The three-week mission was headed by Detlev W. Bronk, National Research Council chair and president-elect of Johns Hopkins

University. He was accompanied by Nobel Prize-winning physicist I. I. Rabi; organic chemist Roger Adams; metallurgist Zay Jeffries, a vice-president of General Electric and manager of that company's chemical division; and Elvin C. Stakman, chief of the division of plant pathology and botany at the University of Minnesota and president-elect of the American Association for the Advancement of Science. The mission left for Tokyo on November 2, 1948, and departed Japan on December 20.

Fluent in English and broadly educated in European science as well as in Japanese science and politics, Tamiya served in a crucial position as "native guide" for both committees. His sense of humor and ebullient enthusiasm instilled respect and appreciation in both groups. As a host he was also a delightful and knowledgeable entertainer. He was a skilled magician and a gifted caricaturist, delighting each member with his drawings.[1] Fortunately most of these have been preserved, though many were given to his guests. All members expressed their appreciation of Tamiya's culture, science, and hospitality.

VANNEVAR BUSH

Hiroshi Tamiya did not meet Vannevar Bush in Japan, but he was familiar with his status in the U.S. war effort. "However, I had the honor of meeting him in 1952 in his office in Washington when I was invited from the Carnegie Institution (Division of Plant Biology at Stanford) as a guest investigator. I was deeply impressed by his erudition and personality which was reminiscent of our Samurai. . . . When I told about my admiration to my friend, Harry Kelly—a physicist with whom I had become acquainted in Japan during the Occupation—he said, 'Vannevar Bush is really the *bravest* scientist we have in this country.'. . . The words 'brave scientist' moved me greatly," wrote Tamiya. "At that time, and even at present, Japanese people are using the adjective 'brave' only for such people as warriors, with such words as 'brilliant,' 'prudent,' 'scrupulous' or 'diligent.' Soon after coming back to Japan, I wrote in a popular scientific journal that Japan must have 'brave scientists' like V. Bush in the future." Bush later proposed a National Foundation for Scientific Research, writing, "In the last analysis the future of science in this country will be determined by our basic educational policy." Tamiya followed Bush's model by sending the best young Japanese scientists to the United States, an invaluable contribution to the scientific successes of the author of this memoir and the young scientists' own future in Japan.

CHLORELLA CULTURE

After termination of the war, Tamiya was asked by his old friend H. A. Spoehr of the Carnegie Institution of Washington to study the feasibility of mass culture of *Chlorella* with the purpose of using the alga as food or animal feed. After many trials and errors he developed a unique open circulation method with a device for intermittent sweeping. Nobuko Tamiya collaborated in developing culinary applications of the nutritious algae. Finally, Tamiya concluded that the idea was hardly feasible because of the high cost of the culturing techniques.

An important by-product unexpectedly emerged from the unsuccessful efforts of mass algal culturing. In laboratory experiments *Chlorella* was grown under diurnal alternation of light and darkness: 12 hours light and 12 hours darkness simulating natural outdoor conditions. It was found that the algal cells grew and divided almost synchronously in the light period, while cellular division took place during the dark period, so that almost all the cells were large at the end of the light period and small at the end of the dark period. By improvements of the culture techniques, it became possible to obtain complete and very uniform synchronous cultures of the alga. This technique, first reported in 1953, opened a new avenue of approach in microbiology to the elucidation of various cellular events occurring during the life cycles of microorganisms that could not be studied by conventional techniques (1966).

INTERNATIONAL BIOLOGICAL PROGRAM

In the beginning of the 1960s, Hiroshi Tamiya was invited to join an international cooperative research project, the International Biological Program. He assumed the chairmanship of the Japanese National Committee for the International Biological Program in 1964 and prepared its summary report in 1974. The studies recalled the lofty slogan of the program, "Biological Basis of Productivity and Human Welfare." His colleagues had warned him that as a laboratory biologist, working with the multitude of field biologists would be fraught with difficulty, for his colleagues rather scurrilously called the field biologists solipsistic wolves (formidable individualists). Tamiya commented on Chairman of the U.S. National Committee Roger Revelle's opening address, when Revelle remarked that one of the most gratifying and unexpected outcomes observed after their participation in the International Biological Program was that it made the otherwise strongly individualistic biologists willingly cooperate with each other in recognition of the international and human significance of the project. Tamiya felt that the same was the case in Japan.

Tamiya criticized the Japanese National Committee for the International Biological Program for having been negligent of fostering young scientists who would pursue and further develop the studies of a similar nature. He deplored the failure to include surveys in cooperation with countries in Southeast Asia and Korea, especially "with our colossal neighbor, the Republic of China."

ACCOMPLISHMENTS AND HONORS

On Tamiya's initiative the Japanese Society of Plant Physiologists was founded in 1958. The publication *Plant and Cell Physiology* owed much to his efforts. Tamiya cooperated with the occupation to establish the Science Council of Japan and the reestablishment of RIKEN, a Japanese research organization. Tamiya's research was supported by the Rockefeller Foundation and then by the Charles F. Kettering Foundation. The Tokugawa Institute for Biological Research was terminated on March 31, 1970, after 53 years of activity since its inception in 1917.

Tamiya was the recipient of the Ehrenmitglied der Deutschen Botanischen Gesellshaft and Mitglied der Kaiserische Deutschen Akademie der Naturforscher and was a corresponding member of the American Society of Plant Physiologists and the Botanical Society of America. He was elected in 1966 to foreign associate membership in the U.S. National Academy of Sciences. Tamiya was the recipient of the Order of Culture from the emperor of Japan in December 1977.

Tamiya was a man of superior intelligence and deep insight into every angle of human culture, especially in music and art, and his talent and vitality would have placed him in the first rank of any occupation he might have chosen. He stated that most literary scholars in Japan could recognize 20,000 kanji characters, while he could recognize 40,000. He did not hesitate to coin new words when he needed them in conversation. His use of the word "Kamenism" described phrases typically used by his friend Martin Kamen.

Tamiya's warm hospitality, which was shared by his wife, Nobuko, in their home, entertained a multitude of visitors from throughout the world. Behind the man's mild face, however, there dwelled a spirit of stern criticism and resistance against meanness and injustice, which he never hesitated to show before those who sold and bought in the sacred temple of his science. Nevertheless, he grew much in generosity and understanding while his inborn sense for keenly distinguishing between what is worthy in science and what is not became more and more sharpened with age. "Science is the god he serves and the ultimate source of his pleasure," wrote Atusi Takamiya in Hase et al. (1990).

Nothing will bear better evidence of his personality than the unaltered friendship he enjoyed throughout decades with his countless friends. His family, his students, and loyal friends meet each January 5 to celebrate his birthday and the memory of his life.

ACKNOWLEDGMENTS

The author acknowledges the importance of Hiroshi Tamiya to successes in his own academic and cultural life. Michio Seki, auditor of RIKEN, provided impetus for appreciating Tamiya's contributions for renewal of Japanese science after the war. Parts of this memoir were adapted from Atusi Takamiya's "Life and Work" in Hase et al. (1990) and from Tamiya's "Pilgrimage of a Man in Biology" lecture presented at the meeting of the Korean Association of Biological Sciences, in Seoul, on October 30, 1971. The Tamiyas' talented daughter, Takako Tamiya Horie, has helpfully contributed copies of her father's caricatures. Bowen C. Dees generously offered his collection of Tamiya's caricatures and personal recollections of his extensive work with Hiroshi Tamiya and the restructuring of Japan's national science organizations.

Material for this memoir was derived from published information in English from Takako Tamiya Horie, daughter

of Hiroshi and Nobuko Tamiya; from Bowen C. Dees (1997), successor to Harry Kelly as deputy chief of the General Headquarters, Supreme Command of Allied Powers, postwar administration in Japan and longtime friend of Hiroshi Tamiya; and from the author's letters exchanged with Hiroshi Tamiya from 1951 to 1984. The text of this memoir was enhanced by the valuable contributions of Bowen C. Dees, R. Clinton Fuller, Carole Mayo, Tatsuichi Iwamura, and Bunji Maruo.

The author's privilege of performing an important laboratory experiment with Hiroshi Tamiya in Berkeley was an important step toward understanding plant photorespiration. Traveling in Japan with the Tamiyas and appreciating often the hospitality of their home provided ample impetus for contributing this memoir of a great international ambassador of science.

NOTE

1. Hiroshi Tamiya's caricatures of the two committees' members are preserved as national treasures and copies have been provided by his daughter, Takako Tamiya Horie, and by Bowen C. Dees, successor to Harry Kelly at the Scientific and Technical Division, Economic and Scientific Section, General Headquarters, Supreme Command of Allied Powers, and close friend of Hiroshi Tamiya and his family. The caricatures are preserved in the membership files of the National Academy of Sciences and are accessible by members and historians.

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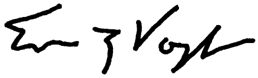

EVON ZARTMAN VOGT, JR.

August 20, 1918–May 13, 2004

BY JOYCE MARCUS

EVON ZARTMAN VOGT, JR.—"Vogtie" to his friends and countless generations of students—was a modest and unassuming scholar who nevertheless managed to transform the entire field of Maya ethnography, altering our views of both the ancient and modern Maya in the process. Vogt did so by spending 35 years among the Tzotzil Maya of Zinacantan in Chiapas, Mexico. His enormous dataset led him to generate new insights about how communities change over time while conserving and maintaining many traditions. His comprehensive analyses of Maya ritual, religion, kinship, social organization, and settlement pattern will link his name forever to Zinacantan and the Tzotzil.

Vogtie was a mentor and role model for me, and for many other students, during his 41 years of teaching at Harvard. In retirement (1989-2004) he remained generous, gregarious, gracious, and more prolific than most scholars half his age.

VOGT'S FAMILY AND HIS EARLY YEARS

Vogt's father, Evon Z. Vogt, Sr., was born into an American family of Swiss and German descent in Upper Sandusky, Ohio, in 1880. In 1892 the family moved to Dayton, Ohio. Vogt Sr. attended the University of Chicago until his senior year, at which time he contracted tuberculosis. His physician recommended a move to the Southwest to recover, and he chose New Mexico. By the time he had recovered his health, he had decided to stay in the Southwest, because he liked it so much.

In 1914 on his way back from a trip to France the elder Vogt stopped in Chicago to visit his brother, who had married a widow with two grown daughters. One of those daughters, Shirley Bergman, became his wife on July 17, 1915. They honeymooned in the Sangre de Cristo Mountains east of Santa Fe and then on the ranch house he had built near Ramah, New Mexico. There he and Shirley settled down to start a family. Their first child and only son, Evon Vogt, Jr., was born on August 20, 1918, in Gallup. His arrival was followed by the births of three girls: Barbara (Mrs. Richard Mallery of Santa Fe), Jo Ann (Mrs. Paul Davis of Ramah), and Patti (Mrs. Paul Merrill of Ramah). All four children enjoyed life on a sheep ranch among very diverse neighbors.

VOGT'S CHILDHOOD NEIGHBORS AND THE ROLE THEY PLAYED IN HIS FUTURE CAREER

The Vogts lived among Navaho, Zuni, Mormons, and Mexican Americans. Vogt Sr. spoke English, Spanish, French, German, and even some Zuni and Navaho. On occasion his Zuni and Navaho neighbors came to visit and were often invited to eat and spend the night. The younger Vogt loved these visits and later described his childhood home and its environs as a "rural microcosm of the United Nations lying within forty miles of the Vogt Ranch."

Vogt always said that his father stimulated his interest in other cultures by taking him (1) to the Zuni pueblo, first to see the summer Kachina dances and then to the ceremony in late November/early December, when 12-foot-tall masked Shalako gods came to visit new houses from midnight to sunrise; (2) to the less acculturated Navaho living in the Canyon de Chelly; and (3) to a performance of the Hopi Snake Dance at Walpi, where the snake priests danced with live rattlesnakes in their mouths. These exposures to ethnic and linguistic diversity influenced Vogt in several ways, surfacing when he began graduate school in anthropology. Vogt said that he developed a "burning curiosity about other ways of life" and that it was exciting "to study and try to understand them, even if you can't join them."

Vogt spent his childhood reading, studying, and helping his father with the sheep. He once described to me in some detail everything he knew about herding, shearing, and dipping sheep, but then quickly added, "It's very hard work and often quite lonely." At the height of his family's ranching operation they were responsible for 200,000 acres and 12,000 sheep. It was a huge job, and it all fell apart financially after the "Big Snow" of 1931, a devastating storm that led to the death of almost all the animals. That event made the younger Vogt yearn for a career with more security, one that was "not nearly as lonely, with only sheep to talk to." He did not know it at the time, but a job with tenure was in his future.

Vogt attended the nine grades taught at the village school at Ramah and then went to Gallup High School (45 miles from his ranch) to continue his education in the tenth grade and beyond. At Gallup High he became the senior class president and graduated first in his class.

Vogt's father encouraged him to apply to his alma mater, the University of Chicago. Vogt Jr. was awarded a full scholarship and entered Chicago in the fall of 1937. There his freshman advisor, Earl Johnson, encouraged him to major in anthropology, noting that Vogt already knew quite a bit about the Navaho, Zuni, and Mexican Americans of the Southwest. Vogt chose geography instead.

The gap between Gallup High and the University of Chicago was tremendous, and Vogt had to work day and night to learn all he was expected to. In his sophomore year he pledged Delta Upsilon (he was considered a "legacy," since it was his father's old fraternity), where he lived for the next three years.

In 1941 Vogt graduated with an A.B. in geography, despite not having found the field as exciting as expected. His background in geography was put to very good use later on, when he pioneered the use of aerial photography to understand Tzotzil Maya settlement patterns (see below).

After interviewing with Professor Fay-Cooper Cole, chair of the Department of Anthropology, Vogt changed fields and secured a Charles R. Walgreen Fellowship to study anthropology in graduate school. During the summer of 1941 he secured work as a ranger at Montezuma Castle National Monument in Arizona, building on previous experiences as a ranger at El Morro National Monument (just 10 miles east of his family's ranch) and Bandelier National Monument near Santa Fe.

On September 4, 1941, Vogt married fellow student Catherine Christine Hiller ("Nan" to her friends). They honeymooned in the Southwest just as Vogt's parents had in 1915. They camped at the Grand Canyon and Canyon de Chelly, and then returned to Chicago, where Vogt entered graduate school in anthropology.

VOGT BEGINS HIS CAREER IN ANTHROPOLOGY

The University of Chicago in the 1930s and 1940s was a very exciting place to study anthropology. The innovative Robert M. Hutchins was still chancellor, ethnographer Robert Redfield was the dean of Social Sciences, archaeologist Fay-Cooper Cole had started a new graduate program in anthropology, and in that department were such luminaries as Redfield, Edward Sapir, William Lloyd Warner, Fred Eggan, and Sol Tax. Vogt once said, "In my 60 years of association with universities, I have never again encountered the kind of exhilarating intellectual electricity in the air that I enjoyed as an undergraduate at Chicago." At the end of his first semester of graduate school, however, Vogt postponed his studies because of the Japanese attack on Pearl Harbor. He joined the Navy and spent nearly five years on missions to Brazil, the Pacific, and elsewhere.

Vogt was finally able to return to graduate school in January 1946. He undertook his first anthropological fieldwork in Illinois, resulting in a master's thesis on the Norwegian farmers of Grundy County. He was preparing to choose a Ph.D. dissertation topic when his first cousin once-removed, anthropologist Clyde Kluckhohn, came to lecture at Chicago; Vogt discussed possible topics with him. Kluckhohn was fluent in the Navaho language and had been studying the Ramah Navaho for years. He suggested that Vogt might focus on how Navaho veterans were adjusting to life back in Ramah after the war. During the war many of these Navaho had worked as "code talkers" and felt themselves to be an integral part of the United States. Once home in Ramah, they felt marginalized again.

THE HARVARD YEARS

While writing his dissertation on the Navaho, Vogt applied to the newly established Department of Social Relations at Harvard, where he was offered an instructorship. In December 1948 he finished his dissertation, *Navaho Veterans: A Study of Acculturation* (published in 1951 as *Navaho Veterans: A Study of Changing Values)*. He was then promoted to assistant professor at Harvard, the institution where he was to spend his entire teaching career. In the course of writing his dissertation Vogt had become increasingly interested in values and beliefs and in comparative studies, aware that groups (and even individuals within groups) perceive things differently because of the cultural values and beliefs they hold.

During his years as assistant professor, Vogt (along with Clyde and Florence Kluckhohn and John M. Roberts) codirected the Ramah Project, whose formal title was "The Comparative Study of Values in Five Cultures." Vogtie's interest in comparative studies was inspired by his professor, Fred Eggan, well known for his "method of controlled comparison." In 1955 Vogt published *Modern Homesteaders: Life in a Twentieth Century Frontier Community,* and in 1966 (with Ethel M. Albert) he brought out *The People of Rimrock: A Study of Values in Five Cultures*. All these early projects kept Vogt in familiar terrain—the Southwest United States—but that was soon to change.

VOGT'S CAREER AMONG THE TZOTZIL MAYA OF MEXICO

In the summer of 1954 Vogt traveled to Mexico; there he met Alfonso Caso, director of the Instituto Nacional Indigenista (INI). In 1955 Caso invited Vogt to tour INI study centers in Chiapas, Oaxaca, and Veracruz. Of all the places he saw, it was the Chiapas Highlands with its many small villages of monolingual Tzotzil and Tzeltal that fascinated him the most.

Now interested in doing fieldwork near San Cristóbal de las Casas, Vogt established a friendship with Alfonso Villa Rojas, INI director for the Tzotzil and Tzeltal area. Vogt and Villa Rojas had a lot in common, both having received degrees from Chicago, where they studied with Redfield. Villa Rojas had collaborated with Redfield in the study of Yucatecan communities such as Chan Kom, so he knew how to set up a project. He opened doors for Vogt, and their collaboration continued for years. In the 1960s when Vogt edited two volumes of the *Handbook of Middle American Indians*, he of course asked Villa Rojas to write the chapter on the Tzeltal.

Unlike their mentor Redfield, both Vogt and Villa Rojas were just as interested in the ancient Maya as in the contemporary Maya. Vogt pursued this interest further than most ethnologists; he envisioned tracing the contemporary Maya from their prehistoric protoculture and protolanguage by means of a framework he eventually called the "phylogenetic model" (see below).

Vogt decided to focus his study on the municipality of Zinacantan, an area of about 117 square kilometers just to the west of San Cristóbal de Las Casas. He concentrated on the ceremonial center of Zinacantan at 2,100 meters, later studying its surrounding hamlets at elevations of 1,500 to 2,400 meters. In 1957 he and Nan rented their project headquarters (later to be known as the Harvard Ranch) from Calixta Guiteras-Holmes, a Cuban anthropologist who had worked with the Tzotzil of Chenalhó and was to publish *Perils of the Soul: The World View of a Tzotzil Indian* in 1961.

The Harvard Ranch hosted (and sometimes housed) a wide range of foreign visitors, students, and professors. The birthday of every project member was celebrated with a cake, the peak coming when, as Vogt put it, "We once celebrated 10 students' birthdays with 10 birthday cakes!" I was fortunate to be a visitor in 1972 when "Vogtie" gave me an all-day tour of his informants' fields and houses. We ended the day appropriately enough at a birthday party for Gertrude Duby Blom, the widow of legendary ethnographer Frans Blom, who had worked among the Lacandon Maya of the Chiapas lowlands.

Decades of fieldwork in the Chiapas Highlands by Vogt and his legions of students have given us a wonderfully comprehensive view of Zinacantan and its hamlets. Vogt's

Chiapas Project ended up training an amazing 120 undergraduates and 40 graduate students, all of whom did original research with him in the field. The undergraduates produced senior honors theses, and the graduate students wrote doctoral dissertations, hundreds of articles, and scores of books. It is unlikely that there has ever been an ethnographic project that attracted so many students who actually went on to publish their results. (The titles of their theses, articles, and books can be found in *Bibliography of the Harvard Chiapas Project: The First Twenty Years 1957-1977* and in *Fieldwork Among the Maya: Reflections on the Harvard Chiapas Project.*)

FIELDWORK STRATEGY, RESEARCH DESIGN, AND RESULTS

From the beginning of his Chiapas project in 1957, Vogt stressed four things to his students: (1) utilizing the native language; (2) actually living with Zinacanteco families; (3) working alongside the Tzotzil in their households; and (4) attending all their rites and ceremonies. To prepare the students for this fieldwork, Vogt saw to it that Tzotzil was taught at Harvard for more than 20 years by someone who had done fieldwork with him. He even brought Zinacanteco informants up to spend six weeks or more in Cambridge, Massachusetts, so that students could learn from a native speaker.

Vogt's research design, as revealed in his 1957 proposals submitted to the National Institute of Mental Health and the National Science Foundation, was "to describe the changes occurring in the cultures of the Tzotzil and Tzeltal" and "to utilize these data for an analysis of the determinants and processes of cultural change." He went on,

> The processes in cultural change are of two major types: the *microscopic* comprising specific additions, subtractions, or replacements in cultural content—

the replacement of stone by steel axes being a classic example; and the *macroscopic* comprising the more pervasive patterns of change which persist over long time spans and involve basic changes in social structure—the shift from bilateral to unilineal [social] organization being a good example. . . . One of the most advantageous features of this [proposed project] is that we shall be able to study cultural changes "on the hoof," and to control (by comparative analysis) the critical variables in these changes.

This was ambitious, but that was typical of Vogt: to plan ahead, tackle big issues, and spend years in the field collecting empirical data to evaluate hypotheses.

After his first decade of fieldwork Vogt published the landmark ethnography *Zinacantan: A Maya Community in the Highlands of Chiapas*. That book earned him the 1969 Harvard Faculty Prize for the "best work of scholarship by a faculty member" and the 1969 Fray Bernardino de Sahagún Prize from Mexico for "the best work by a foreign investigator." As Gary Gossen and Victoria Bricker (1989, p. 3) have stated: "Zinacantan has become a standard world benchmark in cross-cultural studies, and Evon Z. Vogt is indelibly attached to the place, as surely as Boas is attached to the Kwakiutl, Evans-Pritchard to the Nuer, or Malinowski to the Trobriand Islands."

Vogt also used his empirical research in Zinacantan to evaluate many theoretical issues. For example, in a powerful paper published in 1960 ("On the Concepts of Structure and Process in Cultural Anthropology"), Vogt attempted to reconcile the British focus on structural-functionalism, which tended to create synchronic reconstruction and the American tradition of culture process and history. He proposed (1960, p. 26) that

the less economic security a society has, the less decisive will be social structural and value-system variables in shaping the course of events. The more economic security a society enjoys, the more there emerges the possibility for the exercise of "human choice" based upon value-systems to become crucial in the directions of further change. If this proposition holds, it of course follows that the importance of human values in the course of world history is currently increasing at an accelerated rate.

To analyze process or long-term change Vogt suggested that anthropologists conduct field projects of at least 20 or more years of *continuous* observation (emphasis in the original), instead of one- or two-year projects that were too short-term to yield data on ongoing process. Vogt followed his own advice by working for 35 years among the Tzotzil.

A second example of Vogt's commitment to use empirical data to deal with major issues can be found in his 1965 article "Structural and Conceptual Replication in Zinacantan Culture." Vogt argued that the Zinacantecos "have constructed a model for ritual behavior and for conceptualization of the natural and cultural world which functions like a kind of computer that prints out rules for appropriate behavior at each organizational level of the society." His familiarity with these general models allowed Vogt to recover a set of specific rules and principles that allows one to understand and interpret a wide range of rituals and ceremonies. He showed how certain ritual behaviors were replicated at various structural levels in the society (from the house, to the patrilineage, to the multilineage waterhole group, to the hamlet) and that certain concepts were replicated in various domains of the culture. For example, the Tzotzil concept of *bankilal* (older brother) and *'its'inal* (younger brother) was extended to include older and younger brother mountains, ritual specialists, drums, waterholes, officeholders in the religious hierarchy, and so on. He also showed that aspects of social ranking and hierarchy, likely a legacy from the prehistoric Maya, were such that all 150 Zinacanteco ritual specialists were ranked from 1 to 150, depending upon the number of years that had elapsed since each practitioner made his debut as a seer.

Vogt brought these themes and others together in another tour de force effort, his 1976 book *Tortillas for the Gods: A Symbolic Analysis of Zinacanteco Rituals*. This book was the culmination of Vogt's lifelong interest in ritual, ideology, and religion. His fascination with these topics is also evident in his other writings, from his 1958 book (with William A. Lessa) *Reader in Comparative Religion: An Anthropological Approach* to his 1998 book chapter "Zinacanteco Dedication and Termination Rituals." Vogt's interest in ritual undoubtedly began when his father took him to see performances of ceremonies, dances, and masked figures among the Zuni, Hopi, and Navaho.

Vogt's interest in ritual prepared him perfectly for studying the Maya.

> The Zinacanteco way of life emphasizes ceremony. Hardly a day passes in Zinacantan Center without some ritual being performed as the annual ceremonial calendar unfolds; hardly a week passes, even in the smaller hamlets, without at least one ceremony being performed by a shaman to cure illness, dedicate a new house, or offer candles in a maize field (Vogt, 1990, p. 101).

In 1970 Vogt and his wife, Nan, coauthored another important article on ritual, "Lévi-Strauss among the Maya." Here they drew on their own eyewitness field data to explore ritual features of one curing ceremony, utilizing (1) a traditional functional approach; (2) an alternate approach stressing cultural replication (that is, emphasizing themes repeated elsewhere in Tzotzil culture); and (3) an approach inspired by Lévi-Straussian structuralism in which they investigated the opposition of nature and culture. In the last approach Vogtie and Nan emphasized the fact that the Zinacantecos make a distinction between *naetik* (houses and the human-created space filled by the Zinacantecos and their structures) and *te'tik* (trees or forests, referring to the space that is left unused). They go on to show that human encroachments into the domain of nature can be made so long as they are *accompanied by ritual*. Each building of a new house entails taking a piece of land from nature, the taking of mud for the wattle and daub walls, and the taking of grass for the thatched roof. This acquisition of things from nature requires ritual acts of compensation. The Earth Lord must be compensated by a house dedication ceremony, including prayers that ask his pardon, and material offerings of liquor and incense. The carving of a new maize field out of the domain of nature also involves land belonging to the Earth Lord, and again he must be compensated with offerings and prayers.

In addition to his functionalist and structuralist efforts, Vogt made contributions to cultural evolutionary theory. His interests lay not so much in general evolution, or increasing complexity, as in divergent evolution—ethnogenesis, the evolution of daughter cultures from a common ancestor. These interests grew out of the work of historical linguists, especially the work of professor Edward Sapir, who was interested in the relationships among languages and in determining the way languages had diverged from a common ancestral protolanguage. Vogt first referred to this in 1964 as the genetic model, but revised that term in 1994 to the phylogenetic model to emphasize the fact that it was about *cultural* change and divergence, not biological change.

Vogt argued that the Maya were an ideal test case for the phylogenetic model, because they had a nearly contiguous distribution in Mesoamerica; all 35 or so Mayan languages were traceable to a single proto-Mayan language spoken before 2000 BC; and he suspected that careful comparative and historical analyses should help to account for the variation among the 35 present-day groups of Maya.

Vogt's phylogenetic work had implications for Maya archaeology. Consider, for example, his hypothesis that each significant unit in a Maya social system, such as "the extended family living in a patio group, the patrilineage, or the patriclan" had "deified ancestral beings that were given offerings at some kind of ceremonial focus whether it be a small household shrine or a seventy-meter pyramid. If that is the case, then the multiple pyramid-temples in the ceremonial centers probably represented the ancestors of the various important lineages." He linked this hypothesis to a second—that the mountains of the modern Tzotzil and the pyramids of the ancient Maya "function as conceptual and structural equivalents," both serving as dwelling places for the ancestors. It is indeed the case that many ancient Maya pyramids were considered to be mountains where ancestors dwelled—some pyramids, in fact, even housed the tomb of a ruler. It is even possible that these artificial mountains were the conceptual and structural equivalents of the natural peaks where today's Zinacantecos believe their ancestors (or fathers-mothers) dwell. Judging from the frequency with which people think about these ancestors and perform rituals for them, they are the most important Zinacanteco beings. They are remote ancestors of the living Zinacantecos, and according to them "they were ordered to take up residence inside the mountains by the Four-Corner Gods in the mythological past" (1990, p. 19).

For the most part, Vogt's Chiapas Project was focused on what his professor, Robert Redfield, called "the little community." Vogt, however, saw the need for getting a broader view of the region, and as a result he became a pioneer in the use of aerial photography to interpret settlement patterns, the spacing between houses and hamlets, the region's sacred geography, the principles of Tzotzil cosmology, and Tzotzil worldview.

Vogt's 1974 edited volume, *Aerial Photography in Anthropological Field Research*, is perhaps the most comprehensive treatment of this topic ever assembled by an ethnologist. His interest in geography was a legacy from his undergraduate days at Chicago. When he first turned to aerial photographs, Vogt wondered whether cultural factors or environmental factors would be dominant in settlement choice and spacing. In the end, he discovered "that the determinants of settlement patterns in any given municipio [district] were an intricately interwoven set of ecological and cultural factors."

Vogt's aerial photos revealed that the crucial ecological constraint was the availability of water in the karstic highlands during the dry season; the more water available in the communal waterhole, the more compact the settlement. The critical cultural factors, on the other hand, revolved around Tzotzil kin groups (for example, a preference for living in patrilocal extended families, the building blocks for patrilineages). The aerial photos also provided a wealth of data on land plots and ownership. They further facilitated Vogt's study of sacred geography, pinpointing the location of shrines on mountaintops and documenting their relationship to hamlets, caves, and cardinal and intercardinal directions. One could also use them to study ceremonial circuits, or pilgrimages, performed by lineages and waterhole groups.

While he was clearly a towering individual in American anthropology, Vogt never thought of himself as more than one-half of a team. The other team member was his wife of 62 and one-half years, Catherine ("Nan") Vogt. They published together, did fieldwork together, attended all meetings of the National Academy of Sciences together, and produced four children: Shirley Naneen, b. March 6, 1945, now Countess Skee Teleki of Toronto; Evon Zartman ("Terry") Vogt III, b. August 29, 1946, of San Francisco; Eric Edwards Vogt, b. October 22, 1948, of Belmont, Massachusetts; and Charles Anthony, b. July 27, 1953, of Quito, Ecuador. The Vogts had six grandchildren and four great-grandsons, and their names will forever be linked to Zinacantan.

Among Vogt's many honors were his election to the American Academy of Arts and Sciences in 1960, the National Academy of Sciences in 1979, and the American Philosophical Society in 1999. In the National Academy of Sciences he served as the chair of Section 51 (Anthropology) from 1982 to 1984, then as chair of Class V (Behavioral and Social Sciences) from 1987 to 1989.

Vogt was a fellow at the Center for Advanced Study in the Behavioral Sciences at Palo Alto (1956-1957); a visiting scholar in the Soviet Union, as guest of the Institute of Ethnography of the Soviet Academy of Sciences, lecturing in Moscow, Leningrad, and Tashkent in 1968 and in Moscow, Leningrad, and the Republic of Georgia in 1989; and a councilor for the American Academy of Arts and Sciences from 1974 to 1978. He organized two Burg Wartenstein Conferences in Austria: one in 1962 on "The Cultural Development of the Maya" and the other in 1980 on "Prehistoric Settlement Patterns," a symposium to honor his long-time friend and Harvard colleague Gordon R. Willey. In 1978 Vogt was decorated as knight commander, Order of the Aztec Eagle, by the Mexican government for his outstanding study of the Tzotzil Maya. In 1985 he was a visiting scholar in Bulgaria at the Institutes of Ethnography and Folklore of the Bulgarian Academy of Sciences in Sofia, and a visiting scholar in Yugoslavia at the Serbian Academy of Sciences in Belgrade.

PROFESSIONAL QUALITIES OF THE MAN

What were the special qualities that enabled this man to leave Vogt Ranch in Ramah, New Mexico, study at Chicago, teach at Harvard, and have the enormous impact he has had on Middle American ethnology? Vogt possessed many of the necessary attributes, including tenacity; dedication to goals; the ability to complete arduous tasks; tremendous loyalty to family, informants, students, and colleagues; an exceptional ability to collaborate with large groups; and the wisdom to see merit in diverse and divergent views and frameworks. I always valued Vogtie's exceptional warmth, sense of humor, enthusiasm, unwavering support, and heartfelt advice. He was a marvelous listener, who always offered crucial insights to me and to countless former students throughout our careers. He stayed in touch with all of us by e-mail, discussing our work as well as his, literally until just a few days before his death. He was truly a special person and a dedicated social scientist who will be missed by all of us.

HIS LEGACY

Among Vogt's most enduring accomplishments were (1) his controlled comparisons of Navaho, Zuni, and Hispanic groups in the Southwest United States (1947-1953); (2) his development of a theoretical framework to explain ritual behavior in terms of structure, process, and replication; (3) his application of the phylogenetic model to the cultural evolution of the Maya; (4) his long-term study of a Tzotzil Maya community (1957-1992); and (5) his commitment to documenting cultural origins and the persistence of cultural patterns, in spite of ongoing change and innovation. His longitudinal studies among the Tzotzil have provided a wealth of data and ideas that scholars will continue to mine for years.

SOME OF THE MATERIAL presented in this memoir was drawn from lengthy conversations I had over the years with Nan and Vogtie, and I thank them for telling me so much about their lives together.

CHRONOLOGY

1918	Born on August 20 in Gallup, New Mexico
1937-1941	Attended University of Chicago
1941	A.B. in geography, University of Chicago
1941-1942	Attended graduate school, University of Chicago, on a Charles R. Walgreen Fellowship to study anthropology
1941	September 4, married Catherine Christine Hiller (known as Nan)
1942	Fieldwork in Zuni farming village (Pescado) to study relationships between Zuni and Navaho
1942-1945	Served in United States Navy (rank of lieutenant, senior grade). Duty as air combat intelligence officer aboard aircraft carrier in the Pacific
1946-1947	Attended graduate school at University of Chicago
	Research assistant for Committee on Human Development, University of Chicago
1947-1948	Fellow of the Social Science Research Council, Demobilization Award, fieldwork with the Navaho
	Fieldwork among Ramah Navaho to study acculturation of Navaho veterans
1949-1950	Fieldwork at Fence Lake, New Mexico, to study intercultural relationships among Navaho, Zuni, Spanish Americans, Mormons, and Texans
1953-1955	Coordinator of the Comparative Study of Values Project
1954	Fieldwork in Nayarit, Mexico, to study acculturation among the Cora and Huichol
1957-1992	Director of the Harvard Chiapas Project, fieldwork among the Tzotzil Maya
1958-1960	Member, Executive Board of the American Anthropological Association
1974-1982	Master of Kirkland House, Harvard University
1985-1988	Chairman, Committee on Latin American and Iberian Studies, Harvard University
1989	Retirement dinner, Cambridge, Massachusetts
2004	Died on May 13 in Cambridge, Massachusetts

AWARDS AND HONORS

1960	Elected to the American Academy of Arts and Sciences
1969	Awarded the Harvard Faculty Prize for his book *Zinacantan,* "the best work of scholarship by a Harvard faculty member"
	Awarded the Fray Bernardino de Sahagún Prize by Mexico for his book *Zinacantan,* judged to be "the best work by a foreign investigator"
1978	Decorated knight commander, Order of the Aztec Eagle, Mexico
1979	Elected to the National Academy of Sciences
1982-1984	Chair of Section 51 (Anthropology), National Academy of Sciences
1987-1989	Chair of Class V (Behavioral and Social Sciences), National Academy of Sciences
1999	Elected to the American Philosophical Society

PROFESSIONAL RECORD

1941	A.B., University of Chicago
1946	M.A., University of Chicago
1948	Ph.D., University of Chicago
	Named instructor in the Department of Social Relations at Harvard University
1950	Promoted to assistant professor, Department of Anthropology, Harvard University
1955	Promoted to associate professor, Department of Anthropology, Harvard University
1959	Promoted to full professor, Department of Anthropology, Harvard University
1959-1990	Curator, Middle American Ethnology, Peabody Museum
1969-1973	Chair, Department of Anthropology, Harvard University
1990-2004	Professor emeritus, Harvard University

MEMBERSHIPS

American Anthropological Association (fellow)
Society for American Archaeology
Sociedad Mexicana de Antropología
Sigma Xi
Royal Anthropological Society of Great Britain and Ireland

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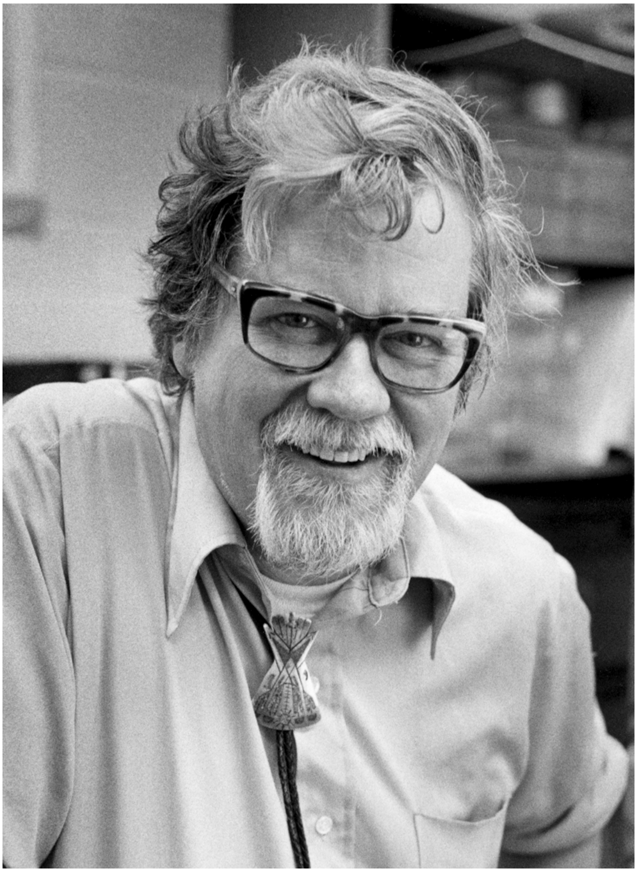

Courtesy of Washington University Photographic Service, St. Louis, Missouri

Robert M. Walker

ROBERT M. WALKER

February 6, 1929–February 12, 2004

BY P. BUFORD PRICE AND ERNST ZINNER

ROBERT WALKER DIED ON February 12, 2004, in Brussels, Belgium, after an extended battle with stomach cancer. He was a visionary with two great dreams, both of which paid off handsomely. He conjectured that meteorites and lunar rocks contain a record of the ancient radiation history of the solar system in the form of fossil tracks of radiation damage. With his colleagues at General Electric Research Laboratory, he made that dream come true. He conjectured that grains that originated in stars could be found in meteorites and analyzed individually to provide new insights into basic astrophysical processes. With his colleagues at Washington University in St. Louis, he made that dream come true, too. As he liked to put it, "In high school, I promised my sweetheart the sun, the moon, and the stars. We now have samples of the moon, we have samples of stars, and we have samples of the sun."

Bob was born on February 6, 1929, in Philadelphia. His father left him and his mother when Bob was only four. While working in New York City, his mother met and married Roger Potter, a construction worker, whom Bob regarded for the rest of his life as his real father. During the Great Depression, when there were few jobs, the three of them moved to a farm near Cobleskill, New York, where his mother's parents lived. The winters were long and cold, and surviving on the farm during the Depression was challenging. He had to walk a mile, sometimes in blizzard conditions, to catch the bus to elementary school. When Bob was 12 they moved to the Bronx, where he went to Thomas Knowlton Junior High School, which he described as "a perfectly awful ghetto school" and a dangerous place where many students carried weapons. He worked throughout this time as a paperboy, then as a delivery boy and helper in a tuxedo and wedding dress store. Bob supported himself through college. Inspired by a couple of good teachers, and despite the fact that no one from Knowlton had ever been admitted to the Bronx High School of Science, he took the entrance test and was admitted, largely on the strength of his essay on the wonders of astronomy. In those days it was all male and mostly Jewish. Now it is mostly Asian and 50 percent girls. Bob learned that he could excel at science in a dog-eat-dog environment. From the age of three, initially influenced by his maternal grandfather, who owned a butterfly collection, he had wanted to become a scientist. On Saturdays he haunted the Museum of Natural History and the Hayden Planetarium, where he marveled at their meteorite collection.

As a result of his family's move back to upstate New York, he transferred to Cobleskill High School for his junior and senior years. His science teacher there allowed him unlimited use of the lab. As senior class president he gave a speech on "living with the atom." It was 1946, just after the war, and a recruiter for Union College in Schenectady persuaded him to enroll in their physics program. During the summers of his college years, in a cement factory where his stepfather was then working, Bob carried heavy loads and helped to repair kilns in almost unbearable heat. He graduated from Union College in 1950, fourth in a class of 400.

As a graduate student at Yale under the nominal direction of Earl Fowler, Bob was the first student to do a thesis using his own experimental apparatus at the Brookhaven Cosmotron, the first accelerator capable of producing strange particles. Fermi was setting up a nuclear emulsion experiment across from where Bob was working, and Bob crossed paths with a number of other future Nobel Prize winners who were doing experiments there. In his thesis he was among the first to show that strange particles had to be produced in pairs. During his time at Yale, other graduate students told him about their research on cosmic rays, which were then poorly understood. This early contact may have influenced his thinking about cosmic ray tracks in meteorites a few years later.

After finishing his Ph.D. in 1954, he met with his draft board in Schenectady, New York. They told him that if he were to take a university position in high-energy physics, he would be drafted, whereas if he were to take a position at General Electric (located in Schenectady), he would be deferred because of the essential defense work being done at the main GE plant. Traditionally, industrial research laboratories send recruiters to cultivate promising graduate students, and so it was quite a surprise when Bob showed up at the employment office of the GE plant and said he wanted a job. He joined the GE Research Laboratory and started research in solid-state physics, an area about which he then knew nothing. He thrived in the free atmosphere that characterized some sections of the lab in those days. He first collaborated with George Watkins, with whom he set up an electron paramagnetic resonance (EPR) spectrometer to study defects in solids. Together with Jim Corbett, he used electron irradiation to produce intrinsic defects in silicon for EPR studies. Their experiment with electron-beam-induced defects was the first to demonstrate the unexpectedly high mobility of lattice vacancies in silicon, and it provided the key approach that eventually led Watkins to a detailed understanding of defects in silicon.

Bob then collaborated with Corbett in studies of intrinsic defects in metals by irradiating them in an electron beam at cryogenic temperatures and then monitoring the change in resistivity during annealing. Their technique became a classic: using an electron beam instead of a proton beam they were able to produce vacancies and interstitials by displacing single atoms. Among their findings was that interstitial atoms in copper move easily at ultralow temperatures. During their experiments, both Bob and Jim received severe radiation burns to their arms due to failure of an interlock to shut off the beam. Bob was given skin grafts a number of times by a specialist at Washington University, where he ended up spending his career from 1966 onward.

It is interesting to try to trace the origin of great ideas in science. Bob was a constant source of scientific ideas, two of which memorialized him in the history of science: fossil tracks of nuclear particles in natural solids and stardust in meteorites. One of us (P.B.P.) was intimately involved in the first great idea, and the other (E.Z.) was involved in the second one. At a meeting in France in September 1960, R. S. Barnes showed Bob electron micrographs of tracks left by fission fragments in mica. E. C. H. Silk and Barnes had mounted sheets of mica against a thin layer of uranium, exposed the sandwich to thermal neutrons in a reactor, stripped thin layers from the mica, and examined them in an electron microscope. To Barnes the discovery was not worth pursuing, because the tracks faded in a few seconds in the electron exposure, because only tiny amounts of mica could be observed, and because the method seemed applicable only to mica exposed to fission fragments. But Bob had long been thinking about the possibility that cosmic radiation might produce permanent records of the irradiation history of the early solar system in meteorites or lunar rocks. The tracks in mica, together with his childhood interest in meteorites, his thesis work on tracks in a cloud chamber, his recollection of the work of students at Yale on cosmic rays, and his research at GE on radiation effects in metals stimulated him to do calculations of ways that cosmic rays might produce permanent damage in the form of tracks in crystals in meteorites. In July 1961 he persuaded one of us (P.B.P.), who was an expert in the study of defects with transmission electron microscopy, to join him. Thus began a long and fruitful collaboration. He showed Price that cosmic rays passing through mica in meteorites might lead to tracks of spallation recoil nuclei that could be seen in the electron microscope, provided the threshold for track production was somewhat lower than for fission fragment tracks.

Within a few months they solved the problem of track fading with the discovery that immersing the mica in hydrofluoric acid "fixed" the tracks by removing the cylindrical regions of radiation damage, leaving fine holes a few nanometers in diameter. They took passing note of the possibility of creating molecular sieves made of irradiated and etched mica, but concentrated on the question of whether mica was sensitive enough to record etchable tracks of spallation recoils. Using the 3-GeV proton beam at the Cosmotron, they found spallation recoil tracks, which showed that cosmic rays should produce detectable tracks in at least one mineral (mica).

In early 1962, looking in the vicinity of a pleochroic halo in thin etched mica, they, in collaboration with W. G. Johnston, discovered the first example of "fossil" tracks radiating from the region around the halo. The etched tracks originated in spontaneous fission of ^{238}U, and the dark-colored spherical region was the result of radiation-induced color centers produced by alpha particles from uranium alpha decay. The uranium was concentrated in a micron-size zircon grain at the center of the pleochroism. This discovery established that tracks in mica survive more than 100 million years and led them to the invention of the now-famous fission track dating method of geochronology. To Bob the most important consequence of the fission track dating method was the proof that cosmic ray tracks would be stable over geologic time periods.

Bob had learned that, of the naturally occurring radionuclides, only ^{238}U spontaneously fissions at a rate high enough to be of interest: ~10^{-16} per year, more than a million times lower than the total decay rate, which is mainly *via* alpha decay. He proposed a clever way of measuring the uranium concentration precisely in the region of sample where one is observing fission tracks. The idea was first to etch the mica and count spontaneous fission tracks and then expose the mica to a known dose of thermal neutrons in a reactor and count the new tracks due to neutron-induced fission of the rarer ^{235}U isotope. Knowing the ratio of ^{238}U to ^{235}U in nature, one could solve two equations for the two unknowns—track retention age and ^{238}U concentration—as a function of counts of spontaneous and induced fission tracks.

At about this time Bob had a devastating conversation with Harold Urey, who told him that there was no hope of finding mica in the moon or meteorites! Countering this setback was their discovery that prolonged etching of mica enlarged the track diameters enough that they could easily be seen in an optical microscope, with the implication that minerals that could not be cleaved into thin flakes might serve to record etchable tracks as well as mica. It is interesting in hindsight to recall that Walker and one of us (P.B.P.) had convinced themselves that tracks could not be etched up to optical dimensions; it was not until an engineer asked whether they could produce a single micron-size hole in mica as a device for fabricating microcircuits that they were led to discover how to make tracks visible by optical microscopy. With the ability to control track diameter, they abandoned electron microscopy in favor of the much simpler optical microscope, which permitted one to study minerals present in meteorites and to scan very large areas for rare events.

Beginning in late 1962, in collaboration with Robert Fleischer, another expert on defects in solids, they carried out experiments on a rich variety of topics, both fundamental and applied. In order to encourage free investigation of speculative ideas, the three of them agreed on an effective rule, that each of the three would be a coauthor of every paper and that they would always list their names alphabetically. This paid off handsomely. For example, Fleischer decided not to be inhibited by his colleagues' view of a track as a disordered, chemically reactive region. He discovered that two kinds of noncrystalline solids—glasses and polymers—could record etchable tracks, which forced a revision of their model of track structure and ultimately led him to invent the "ion explosion spike" model of track formation. Walker was on a sabbatical in Paris at the time and would not believe that Fleischer's claim about tracks in noncrystalline solids was correct until he irradiated and etched glass himself and confirmed the discovery.

Throughout the collaboration the wisdom of their rule was demonstrated. Walker returned from a trip, excited about the possibility of searching for magnetic monopoles in minerals, and he devoted time to calculations of the radiation damage rate of a monopole while one of us (P.B.P.) and Fleischer were busy doing experiments. Fleischer became interested in using the new fission track dating method in some tedious experiments that ultimately paid off: confirmation of the concept of spreading of the sea floor on either side of the mid-Atlantic ridge; confirmation of the then-disputed approximately 2-million-year-age of hominids from the Olduvai Gorge; and studies of fission track ages and upper limits on cosmic ray exposure ages of tektites. A year or so later one of us (P.B.P.) measured fission tracks in several different minerals in an iron meteorite, which established the presence of the now extinct nuclide ^{244}Pu in the early solar system and allowed the cooling rate of the parent body to be estimated. Sometimes the three of them pursued both productive and unproductive ideas at the same time.

During the heady period after the discovery of ancient tracks, the number of ideas for applications of the nuclear track technique mushroomed. Among the early scientific applications were the discovery of ternary fission of heavy elements induced by heavy ions and the realization that one might be able to find rare cosmic ray events in meteoritic crystals. Among the early technological applications were production of molecular sieves; deposition of quasi-one-dimensional solids inside etched tracks; neutron dosimetry; radon dosimetry; neutron radiography; mapping of spatial distributions of uranium, thorium, boron, and lithium; alpha autoradiography; and uranium exploration via radon fluxes.

During Bob's 1962-1963 sabbatical, he was joined by Michel Maurette, then a graduate student at the University of Paris (Orsay), and by Paul Pellas, a mineralogist at the Paris Museum. Under Bob's supervision Maurette was the first to fulfill Bob's dream of seeing cosmic ray tracks in a meteoritic mineral (olivine). After Walker's return to GE, he, Fleischer, one of us (P.B.P.), and Maurette reported their observations of cosmic ray tracks in 23 meteorites, showed that most were due to slowing nuclei of iron and neighboring elements, and announced the discovery of elements much heavier than iron in the cosmic radiation. The determination of atomic numbers of ultraheavy cosmic ray tracks in meteorites was crude but sound; it depended on calibrations with heavy ion beams at the Berkeley Heavy Ion Accelerator, from which the atomic number of the slowing ion could be related to the length of the etchable track. Characteristic of Bob's computational ability was his work reported in their 1967 theoretical paper that discussed six potential kinds of fossil particle tracks in meteorites. For decades after their discovery of trans-iron cosmic rays, Fleischer, Price, Walker, and Russian researchers at Dubna searched in vain for superheavy elements ($Z \sim 110$) and magnetic monopoles in meteorite crystals, using heavy ion calibrations together with improved etching techniques and various procedures such as partial annealing to get rid of the background of abundant tracks of iron cosmic rays. The attractive feature of meteorites as track recorders—a collecting time measured in tens of millions of years—was thwarted by the complexity of the partial relaxation of the radiation damage during such a long time and by the dependence of track-recording sensitivity on chemical composition of the mineral under study.

While the early research on etched nuclear tracks was proceeding from one triumph to another, Walker was also building an organization called Volunteers for International Technical Assistance. Known as VITA, the organization was born in 1959, when he and a small group of colleagues in Schenectady decided to contribute their know-how to solve technical problems in developing nations. One of its earliest successes was an inexpensive, effective solar cooker consisting of an umbrella coated with reflective foil. Bob was its first president and served on its board of directors for several decades. One of the secrets of VITA's success is the policy of putting the client in direct contact with the VITA member who handles his problem. It now boasts more than 7,000 engineers and scientists who volunteer their time to projects ranging from a *Village Technology Handbook* to a digital communications satellite system that relays information from VITA headquarters to ground stations in such places as Somalia and Indonesia.

Two technological spin-offs of the nuclear track technique led to the creation in the 1960s of industries owned in part by General Electric. The first and still the best-known is the Nuclepore filter, available in hole size from 0.015 to 10 μm, made by irradiating a long sheet of thin polycarbonate film with collimated fission fragments in a nuclear reactor and then etching it in hot sodium hydroxide to the desired hole size on an assembly line. This is one of the most valuable items in the stockrooms of microbiology laboratories, and yet biologists don't know who invented it or why it is called "Nuclepore." As with Kleenex, it is now a generic word rarely shown with its trademark symbol. The second is the radon dosimeter originally sold by Terradex Corporation and now marketed as RadTrak, by Landauer Corporation. Its high signal to noise and compact size make it the detector of choice with which to record alpha-particle tracks from decay of radon gas seeping into homes—a serious radiation hazard in some parts of the United States and other countries. The device is simplicity itself; a small piece of an alpha-track-recording polymer suspended in a porous box that is collected and the tracks measured after a month or so of exposure in a home. No doubt the environment at the GE Research Lab stimulated the three collaborators to think of practical applications of their research.

By 1966 Bob realized that despite the wonderful years he had spent at GE, the atmosphere for basic research there was deteriorating and life for him would be better at a university. He moved to Washington University as the first

McDonnell Professor of Physics. Three years later, one of us (P.B.P.) moved to Berkeley as a professor of physics, and the long-standing collaboration ended not long afterward, except for a few interludes in which the three of them coauthored review papers and wrote their famous monograph on *Nuclear Tracks in Solids*.

At General Electric Bob's intellectual efforts had been focused on his own research in collaboration with a couple of other scientists. After the move to Washington University, Bob's mode of operation changed in a fundamental way. As a professor of physics he greatly expanded the scope of his activities. He created the Laboratory of Space Sciences on the fourth floor of Compton Laboratory, where he gathered a number of gifted graduate students, postdocs, and collaborators and also created a place with a unique atmosphere of camaraderie and open interaction. While he was not necessarily involved in all the scientific activities of the group, he was a constant source of new ideas, and with his boundless enthusiasm he was an inspirational leader. In the 1970s he led in the revitalization of the Geology Department, transforming it into the Department for Earth and Planetary Sciences. New faculty members appointed with Bob's leadership included Ray Arvidson, Ghislaine Crozaz, Larry Haskin, and Frank Podosek in Earth and Planetary Sciences, and Charles Hohenberg in Physics.

Bob's efforts to create a permanent structure that would ensure the pursuit of space science at Washington University culminated in the establishment of the McDonnell Center for the Space Sciences in 1974 through an initial large gift by the McDonnell Aerospace Foundation. Bob served as the director of the center from its inception until 1999. When he made his pitch to the members of the foundation (among them James S. McDonnell, the founder of the

McDonnell Company), he chose two graduate students to make the presentations.

The center provided funds for endowed faculty positions in the Physics and Earth and Planetary Science departments, graduate fellowships and support for visiting scientists, and seed money for innovative scientific projects by center members. It became an international scientific meeting place through which passed dozens of visitors during the years. One was Ramanath Cowsik, who spent three months each year at the center before becoming a professor in the Physics Department in 2002. Today the 80 members of the McDonnell Center include professors, research scientists, and students at Washington University doing research in meteoritics, lunar science, planetary imaging and geophysics, theoretical and observational astrophysics, high-energy astrophysics, general relativity, extraterrestrial materials, and cosmic ray studies.

Bob had a knack for inspiring and motivating others for science. An example is his recruitment of one of us (E.Z.). In 1971, when he was completing a Ph.D. in high-energy physics, Zinner had a chance encounter with Bob in the elevator. During the ensuing discussion, which lasted several hours, Bob charmed him to the extent that Zinner decided to give up high-energy physics and join Bob as a postdoc the next year. During more than three decades, he saw Bob convert the laboratory from a place that concentrated on nuclear track studies into a place with sophisticated microanalytical instruments, such as the ion microprobe, NanoSIMS, scanning electron microscope, transmission electron microscope, and Fourier transform infrared spectrometer. This equipment, and Bob's vision, led ultimately to the discovery of stardust in meteorites and interplanetary dust particles and to their detailed isotopic and mineralogical characterization.

The exciting research environment of Bob's fourth-floor laboratory was celebrated at a symposium in his honor at Washington University in March 2003. Many previous students, postdocs, and collaborators gathered to honor Bob's scientific achievements and leadership. While the symposium presented scientific talks on a wide range of disciplines, it became clear in one testimony after another how universally loved and respected Bob was. Another important role Bob played as a leader of the fourth-floor group was that he took upon himself the administrative burden to secure financial support for the whole group. This enabled the other researchers largely to concentrate on their respective research.

In parallel with readying his laboratory for the arrival of lunar samples in 1969, Bob, together with Jim Arnold, Paul Gast, and Jerry Wasserburg ("The Four Horsemen"), played a major role in the establishment of the Lunar Receiving Laboratory.

Fleischer, Price, and Walker found themselves in a healthy competition in the quantitative use of nuclear tracks to study the radiation history of lunar samples. At least four kinds of energetic particles strike the moon: solar wind particles, suprathermal solar particles, solar flare particles, and galactic cosmic rays. In addition to these external sources of tracks in lunar rocks, internal sources include the spontaneous fission of ^{238}U and (in the early history of the solar system) of ^{244}Pu. Among the topics they studied were dating of lunar rocks and glassy impact spherules, analysis of gardening and erosion rates of lunar soil using solar flare track densities as a function of depth in soil particles, comparison of the average present-day and ancient spectra of heavy ions emitted in solar flares, suprathermal ions from the sun, ^{244}Pu fission tracks in ancient lunar rocks, cosmic ray exposure history of lunar rocks, and tracks of solar wind ions heavier than iron. The *Apollo 12* mission returned a sample of glass from the *Surveyor III* spacecraft, which had landed on the moon two and a half years earlier. The glass recorded tracks produced by suprathermal solar particles and by heavy ions emitted in solar flares. Bob was the principal investigator of an experiment in which various track detectors (glasses, plastic, mica) as well as metal foils for the capture of implanted noble gas solar wind ions that were taken to the moon with the *Apollo 16* and *Apollo 17* flights were exposed to solar radiation. It turned out that mica records heavy ions (iron and heavier) in the solar wind (having an energy of only ~1 keV/nucleon) and the heavy solar wind flux could be studied in this way.

Bob served on the scientific team that advised NASA on the handling and distribution of moon rocks and soil samples from the first Apollo missions. At the end of the Apollo program, he and James Arnold were invited to dinner at the White House along with the Apollo astronauts, Werner von Braun, and others.

Bob's wide-ranging interests sometimes led to activities beyond the confines of his space-related scientific work. For example, the use of thermoluminescence by Bob and two of his students led to the authentification of an ancient Greek bronze horse, which in turn led to Bob's founding of the Center for Archaeometry, whose focus was the application of modern scientific methods to art conservation and to the dating of art and archaeological objects. This center brought together scholars from widely diverse disciplines—art historians, conservators, chemists, and physicists. For several years researchers on the fourth floor worked on the restoration of sculptures in the midst of research on meteorites and lunar samples.

Bob's acquisition of an ion microprobe was inspired by the group's comparative studies of both cosmic ray tracks and micrometeorites in lunar samples. He thought that the ion probe might make it possible to identify ions implanted in lunar samples. Unfortunately instruments then available did not have high enough mass resolution to eliminate molecular interferences. That did not diminish Bob's hope that with an improved instrument wonderful things could be found if one could measure the elemental and isotopic composition of extraterrestrial samples on a small spatial scale.

This hope was nurtured by two other developments. One was Robert Clayton's discovery in 1973 of isotopic anomalies of oxygen in refractory inclusions from primitive meteorites. The existence of these anomalies demonstrated that not all material from which the solar system was formed had been completely homogenized, so that some pre-solar grains might have survived. A second development was Don Brownlee's successful recovery of interplanetary dust particles (IDPs) in the stratosphere. Bob immediately realized the scientific potential of this type of extraterrestrial material, and he started a vigorous program of their study, using as many microanalytical techniques as were then available. These originally included analytical transmission electron microscopy and Fourier transform infrared spectroscopy but were later extended to the ion probe and to two-step laser-desorption laser-ionization mass spectrometry. His successes contributed to a large extent to the establishment of IDP studies as an important branch of meteoritics.

In the late 1970s the French company Cameca developed a new ion probe with sufficiently high mass resolution to separate molecular interferences from atomic ions for most elements of interest. In 1980 Bob raised the money to purchase a Cameca ion probe from the McDonnell Aerospace Foundation. During the academic year 1980-1981 he and his wife, Ghislaine Crozaz, spent a sabbatical, first in India and then in Paris, and in the spring of 1981 Bob and one of us (E.Z.), who was spending a year in Vienna, negotiated with Cameca the detailed specifications of the instrument. The fact that such negotiations usually followed a lunch with a fine French meal and ample wine posed a challenge, but Bob lived up to this challenge splendidly. The instrument arrived at Washington University in the spring of 1982. This was a hectic time for the fourth-floor researchers because, while some were breaking in the ion probe, others were constructing a capture cell experiment for NASA's Long Duration Experimental Facility (LDEF), a bus-size satellite that could carry large-area passive experiments and was supposed to spend one year in Earth orbit. The Washington University experiment was designed to capture material from IDP impacts. Unfortunately, as a consequence of the *Challenger* disaster, LDEF remained in orbit for five years, and that long exposure led to the destruction of most of the thin plastic covers over the capture cells. That and problems of contamination on the spacecraft impaired the quality of the results. On the other hand, the Cameca ion probe led to exciting developments. Its first application was the measurement of hydrogen isotopic ratios in IDPs, revealing large deuterium excesses. This led to a whole series of isotopic measurements on these particles throughout many years.

A few years later, in 1987, when Ed Anders and his colleagues at the University of Chicago had obtained a residue that was extremely enriched in neon-22 and a component of xenon synthesized by slow neutron capture (the s-process), the Washington University ion probe was able to identify the carriers of these noble gases as pre-solar silicon carbide, condensed in the expanding atmospheres of red giant stars. Thus, Bob's old dream of holding stardust in his hands was finally realized. This was just the beginning of the stardust story. The study of pre-solar grains expanded rapidly and today is an important new branch of astrophysics. The fourth-floor group leads in studies of pre-solar grains with discoveries of new types of stardust, detailed isotopic characterization of countless grains in the ion probe, and mineralogical studies in the transmission electron microscope. Bob was involved in many aspects of this work. Examples are the *in situ* detection of pre-solar silicon carbide grains, the discovery of pre-solar oxide grains, and the identification of complex aromatic molecules in pre-solar graphite grains.

However, this is not the end of the story. In his quest to extend instrumental capabilities to the analysis of ever-smaller amounts of material, Bob devoted the last years of his life to the implementation of a new type of ion microprobe, the NanoSIMS. This instrument has several important advantages over the previous generation of ion probes: the primary beam can be focused into a much smaller spot than was previously possible; the transmission at high mass resolution, necessary for most isotopic measurements, is higher by a factor of 20 to 30; and the instrument has several secondary ion detectors that make it possible to measure several isotopes or elements simultaneously. As a result of these features, isotopic compositions of smaller grains can be analyzed than was previously possible. In one of his last actions as director of the McDonnell Center, Bob provided some of the funds for the acquisition of a NanoSIMS and was instrumental in securing additional funds from NASA and the National Science Foundation. The instrument was delivered at the end of 2000. Bob was again correct in his hunch that wonderful things would happen if one could only analyze very small samples. The NanoSIMS has led to several important discoveries. One was the identification of pre-solar silicate grains in IDPs and primitive meteorites. Although it turned out that pre-solar silicates are more abundant than most other species of pre-solar grains, the reasons that previous searches had been unsuccessful were that these grains are smaller than 1 micron in size and that minerals in primitive meteorites are dominated by silicates originating in the solar system. The identification of pre-solar silicates requires the isotopic analysis of tens of thousands of submicron grains, and it is this capability of the NanoSIMS that led to their discovery.

In his last scientific effort Bob returned to the application of nuclear tracks. His idea was to measure both the ^{238}U and ^{235}U content of pre-solar silicon carbide grains—the ^{238}U by spontaneous fission tracks and the ^{235}U by irradiating the grains with both thermal and fast neutrons. Such a measurement might give information on the formation time of the parent stars of the grains and to their neutron exposure in the stars. Sadly, his disease and death prevented his fulfilling this dream that united nuclear tracks with stardust.

As part of his continuing interest in the study of meteorites, Bob participated in the 1984-1985 and 1990-1991 expeditions sponsored by the National Science Foundation to collect meteorites in Antarctica. For this work he received the Antarctic Service Medal. Despite the numerous recognitions that came his way, Bob was always concerned, first and foremost, with the pursuit of science for its own sake, rather than for his own glory. He was a tireless spokesperson and lobbyist for science.

Walker and his wife, Ghislaine Crozaz, a professor in the Department of Earth and Planetary Sciences at Washington University, maintained a residence in St. Louis, but they had been spending much of their time in Brussels since his retirement. Walker is survived by his wife; his two sons, Eric Walker (with wife, Terry, and children, Marie and Andrew, of Cottage Grove, Minnesota) and Mark Walker (with wife, Trisha, and son, Alden, of San Antonio, Texas); and his spiritual daughter, Meenakshi Wadhwa (of Chicago).

We are indebted to Ghislaine Crozaz for her help with this memoir and to Roland Schmitt for sharing with us his unpublished article "A Discovery and Its Uses: The Story of Particle Tracks in Solids."

HONORS

1964 Co-winner, American Nuclear Society Award for Distinguished Service
1966 Yale Engineering Association Annual Award for Contributions to Basic and Applied Science
1967 Doctor, honoris causa, Union College
1970 NASA Exceptional Scientific Achievement Award
1971 E. O. Lawrence Memorial Award of the U.S. Atomic Energy Commission
1973 Elected to the National Academy of Sciences
1975 Docteur, honoris causa, University of Clermont-Ferrand, France
1991 J. Lawrence Smith Medal, National Academy of Sciences
1992 Officier de l'Ordre des Palmes Academiques
1993 Leonard Medal of the Meteoritical Society
1997 Peter Raven Lifetime Achievement Award, St. Louis Academy of Science
1999 Asteroid 6372 named Walker by International Astronomical Union
2004 Doctor, honoris causa (posthumous), Washington University

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