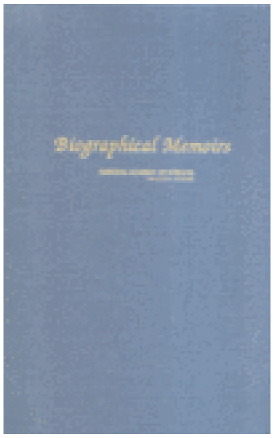

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Biographical Memoirs

VOLUME 85

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PREFACE

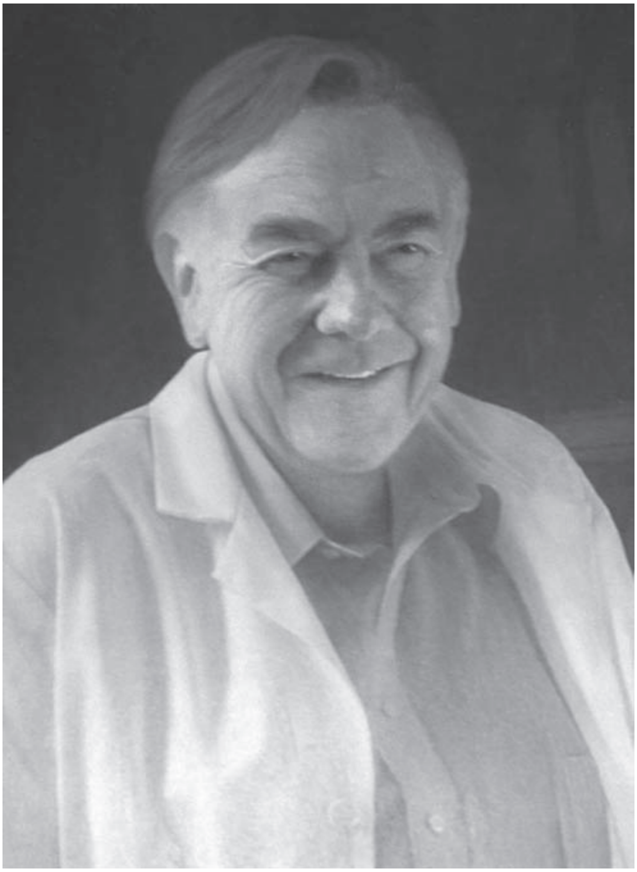
On March 3, 1863, Abraham Lincoln signed the Act of Incorporation that brought the National Academy of Sciences into being. In accordance with that original charter, the Academy is a private, honorary organization of scientists, elected for outstanding contributions to knowledge, who can be called upon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

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

R. STEPHEN BERRY
Home Secretary

Biographical Memoirs

VOLUME 85



D. BERNARD AMOS

April 16, 1923–May 15, 2003

BY EDMOND J. YUNIS

D. BERNARD AMOS WAS one of the most distinguished scientists of the genetics of individuality of the twentieth century. He shares with K. Lansteiner and Peter O. Gorer the discovery that serologically detectable differences of individuals and transplantation specificities are related in nature. Discoveries of Gorer and Amos of the serology of the H-2 genetic system of murine histocompatibility and the subsequent discovery in both mouse and human (HLA system) that the transplantation antigens are controlled by closely linked loci with polymorphic alleles were fundamental in the development of clinical transplantation and the understanding of the genetics of the immune response. Amos's research for more than 35 years was seminal to the knowledge of the T-cell repertoire and the role of major histocompatibility complex (MHC) gene products in the immunology of restriction—self versus nonself—that are the basis of modern immunology.

Bernard, as he preferred to be called, was born on April 16, 1923, in Bromley, Kent, England. He was the only child of Vera (née Oliver) and Benjamin Amos. His family background was relatively humble, to the extent that no one could have predicted his outstanding place among the great scientists of the world. His father was a car mechanic, his mother a teacher. As a child he was a good student but was mischievous. At the Bromley School he once produced a contact explosive that burst as the instructor walked to the board. He was known for experimenting with chemicals and enjoyed lively dancing. He also attended Sir John Cass Technical Institute and worked as a technician at Burroughs Wellcome.

From 1940 to 1945 he worked for Dr. Macfarlane at Ratcliff Infirmary, Oxford. During this time he was assistant scoutmaster to children evacuated from London. In 1946 he returned to London and worked as a technician in Harley Street for D. Scott Jones. He attended Chelsea Polytechnic and obtained an M.B. degree before entering Guy's Medical School in 1947. During his studies he was awarded the Golding Bird Prize in Bacteriology and the Leonard Lubbock Gold Medal. He graduated in 1951 with M.B. and B.S. degrees. A postgraduate M.D degree for research was awarded to him in 1963.

In 1949 he married his first wife, Solange Labesse (medical student at Royal Free), and their first two children, Sue and Martin, were born in 1951 and 1953, respectively. He was appointed a postdoctoral fellow and pathology trainee at Guy's hospital from 1952 to 1955. From 1955 to 1962 he was appointed a senior research scientist at Roswell Park Memorial Hospital in New York state. During that time three more children—Chris, Nigel, and Renee—were born. From 1962 to 1992 he was the James B. Duke Professor at Duke University and the Division of Immunology head. His wife, Solange, died in 1980. In 1984 he married Kay Veale, who had been widowed in 1980; he had met her while he was working for Dr. Macfarlane before Bernard became a medical student. Bernard Amos died on May 15, 2003, in Durham, North Carolina. At the time of his death he was the James B. Duke Emeritus Professor of Immunology and Experimental Surgery at Duke University Medical Center. He leaves his wife, Kay; daughters, Susan and Renee; sons, Martin, Christopher, and Nigel; and their families.

It has been 50 years since he was in the laboratory of Peter A. Isaac Gorer at Guy's Hospital in London and reported his method of agglutination of white blood cells against what is known today as MHC antigens (1953). His mentor had discovered the genetics of individuality in 1934, including the genetics of tissue antigens that are involved in the rejection of allografts. Subsequently they demonstrated that skin allografts and intradermal injection of lymphocytes elicit antibodies to murine MHC antigens (1954) and the use of immunization against tissues of different strains of mice and analysis of serologic activity against tissues by cross-absorption with tissues to define what is today called the H2-D, H2-K, and H2-B haplotypes, with the first definition of more than one locus in the MHC region (1955).

In 1955 Bernard joined Ted Hauschka's cancer research group at Roswell Park, where he continued his pioneering work in tumor immunity. While there he documented the earliest demonstration of tumor immunity using antibodies (1959,1). He described the first sex-linked histocompatibility antigen and developed expertise in skin graft models of tissue rejection in mice and humans (1959,2).

He was recruited to Duke University in 1962 as a professor of experimental surgery, and remained at Duke University for more than 40 years. One of his many contributions demonstrated the use of lymphocytes for typing the MHC antigens to match donors and recipients for organ transplantation. As a matter of fact, the first kidney transplant between a recipient and a living related donor who was selected on the basis of MHC matching was performed at Duke University Medical Center in 1965, resulting from his fundamental research (1965,1). The 1962-1965 period saw advances in his ability to define antibodies to be used for matching histocompatibility. At first they depended on small volumes of sera and serum procurement from multiparous women or from transfused individuals. Then his original efforts to study the genetics of antigens were combined with the use of functional assays. Before 1967 the most widely used methods were the normal lymphocyte transfer test (NLT) of Brent and Medawar (*Brit. Med. J.* 5352[1963]:269) as adapted to man by Gray and Russell (*Lancet* 2[1963]:863) and the third man-skin-graft procedure of Rapaport et al. (*Ann. N. Y. Acad. Sci.* 99[1962]:564). In the NLT, live lymphocytes were injected intradermally.

Positive reactions were characterized by erythema and induration appearing at 24 to 72 hours. When Bernard's group carried out the NLT tests, they found that the subjects tested could provide an excellent supply of novel antibodies. By serendipity they found that when the test was performed weeks later in the same individuals, the reactions could not be reproduced; their reactions diminished or did not occur. Serum samples were tested and cytotoxic antibodies were found; there was a good correlation between reduced reactivity and antibody production (1965,2).[1]

During the late 1960s skin grafts were used to match donor and recipients for kidney transplantation together with serological results; two HLA identical pairs rejecting each other's skins several days later than haploidentical (half identical) or HLA different siblings. Such contributions established the foundations for the importance of HLA typing in allotransplantation (1966,2; 1967,1; 1978,1).

He with other pioneers defined the role of public and private epitopes in the MHC antigens as well the concept of shared epitopes by HLA alleles to explain cross-reactivity (1955; 1969,2; 1972,2).[2] It is remarkable that he and his collaborators made the discovery of both the first H-2 recombinants and the first HLA recombinants (1966,1; 1969,1), which prepared him to understand before others in the field the concept of linkage (haplotypes) and, at the population level, nonrandom association of HLA alleles.

With Fritz Bach he discovered MHC-controlled reactivity, using the mixed leukocyte reaction, later used as a tool for matching in organ and bone marrow transplantation; HLA identity resulted in the lack of MLC reactivity. Since skin grafting and serotyping were well advanced at Duke and mixed lymphocyte culture reactions and access to large families were available at the University of Wisconsin, the two schools had a productive collaboration that resulted in the demonstration that HLA controlled MLC reactivity, serologically detectable leukocyte antigens, and skin graft reactivity (1967,2; 1967,4). The degree of stimulation was related to the difference in the number of haplotypes shared. Subsequently the map order of HLA-A, HLA-B, and HLA-DR was established in family studies with Frances Ward, Janet Plate, and Edmond Yunis (1971,2; 1971,3). These results were confirmed in investigations of large Amish families using serology and MLC testing (1974,1) and in a three-generation family study (1974,2).

Bernard was a pioneer in cellular immunology. The mixed lymphocyte test was used routinely, together with serology, for many years to match transplantation antigens (1967,3; 1970,2). Further, his laboratory demonstrated a major role for cellular immunology not only in transplantation but also in cancer, with the characterization of cytotoxic lymphocytes against tumors, in collaboration with Gideon Berke and Karen Sullivan (1971,4; 1972,1), and also against Class I gene products (1978,2). He was also involved in the early studies of immunological enhancement (1970,1).

He was one of the first to recognize the value of studying different ethnic populations in order to understand polymorphisms in the evolution of the MHC. Such interest gave him the opportunity to do research and teach in Asia and South America. He personally helped to establish clinical histocompatibility laboratories in the United States, Chile, Brazil, Argentina, Peru, Thailand, and India. He established the association of histocompatibility genes with various disease states, such as iron storage (1980), and the first documentation of MHC allelic loss during malignant transformation (1971,1).

His contributions were many and he took pride in teaching, which for him was more important than the many awards he received. He had the skill to bring together leaders and make things happen. He organized the First International Histocompatibility Workshop, in Durham, North Carolina, which in an unprecedented manner stimulated international collaboration and led to competitive studies that define the MHC (HLA) complex (Publ. no. 1229, National Academy of Sciences and National Research Council, Washington, D.C., 1965). There have been 13 of these international workshops. The genetic diversity detected in different ethnic populations has been the topic of these workshops, influenced by many of his pioneer efforts.

In 1969 he organized, with Dr. David Hume, the first regional organ-sharing program in the United States, later known as the South-Eastern Organ Procurement Foundation (SEOPF). This organization continues and was fundamental to the establishment of UNOS, which is the national center for organ allocation in the United States. He was the main force in organizing the Transplantation Society in 1967 and was its first past president.

Bernard also organized the first World Health Organization Nomenclature Committee, which has been responsible for the naming of HLA specificities and alleles since 1967. Through his contact with the National Institutes of Health, he facilitated support for several laboratories involved in transplantation research in the United States and abroad. He was the chairman of the task force on immunology and disease for the National Institute of Allergy and Infectious Diseases, the first chair of the National Institutes of Health committee on transplantation and immunity, and established a repository of reference reagents for histocompatibility testing laboratories worldwide.

He was the first chair of WHO HLA Nomenclature Committee and was a member of the Organizing Committee of the First International Congress of Immunology. In addition, he served as president of the American Association of Immunology from 1980 to 1981. Bernard's scientific contributions and his importance to the world were the reasons for his many awards, including his election to membership in the National Academy of Sciences and the Institute of Medicine, the Rose Payne Award presented by the American Society for Histocompatibility and Immunogenetics, and the 3M Life Sciences Award. He was pleased that SEOPF/Sandoz gives an award annually in his name to an outstanding young scientist for research in transplantation. He was proud to have been the first editor of *Human Immunology.* Upon his retirement as editor-in-chief of the journal *Human Immunology,* which he cofounded, he was honored in a special issue with tributes by his colleagues and students. The XIV International Congress of the Transplantation Society recognized him for his contributions (Transplantation Proc. Dedication of the XIV International Congress of the Transplantation Society—the Jean Hamburger Memorial Congress—and citations, pp. 4-6, 1993).

On February 21, 2000, members of Duke's original transplant team and current members gathered to celebrate the future of the program, and a graduate scholarship was endowed, the Bernard Amos Training Fellowship for Immunology to honor Duke's first immunologist.

Recently, and in connection with the last international histocompatibility workshop, many friends and colleagues contributed written statements of experiences concerning their friend and mentor ("A Tribute to Bernard Amos." *Clin. Transplant.* 16[2002]:75-91). Such a tribute is the best way to remember him and to honor him as a scientist who was loved by the many he trained or influenced.

Helping new laboratories develop research and clinical programs in histocompatibility testing and immunogenetics, whether in the United States or abroad, was always one of Bernard's major interests. He invited investigators to his laboratory; sent technologists and junior faculty to various places to help set up labs; brought them into national and international workshops; and sent supplies, reagents, books, and equipment to those in need.

My own experience with Bernard lasted 35 years and began by chance at the annual meeting of the Federation of American Societies for Experimental Biology in Atlantic City in 1967. Even though I had been trained in blood groups, I did not understand his lecture. Months later I was asked to establish a tissue-typing facility. Bernard Amos became my mentor. Space in his laboratory was limited, and it was generous of him to give me a corner of a bench and a desk. "BC" was the immunized donor in our studies of cross-reactivity of antigens (1969,2; 1972,2)

Family studies included a Minnesota family, which together with the studies of the HLA genetics and mixed lymphocyte reaction of Bernard's own family, demonstrated that there was a separate genetic locus from HLA-B. It was important because the dogma at the time was the belief that MLC reactivity was explained by the difference of HLA phenotypes in unrelated individuals (1971,2). His poem written in 1992 explained the discovery.

Soon came Janet, Fran and you
Observing how mixed cells react
In culture, Martin, Ni, and Sue
The Schlagel sibs, we made a pact.
To understand the many genes
Upon one haplotype arranged
In order, this became our theme
And with MLR-R and MLR-S we played.

The map order was confirmed in a large three-generation family, which included the genetic transmission of a recombinant haplotype (1974,2). In addition, Bernard suggested that MLR would have alleles in linkage disequilibria with HLA-B alleles and that their incompatibility would result in graft versus host disease and that genes in the HLA region (now Class I) were involved in the rejection of allotransplants. These conclusions were related to the lack of MLR reactivity in some HLA identical unrelated individuals that were unreactive, and the analyses of skin graft rejections. He once described the early period of transplantation genetics, times of unselfished cooperation to define the genetic basis and the definition of many loci and their alleles in HLA, when he and others adopted the concept of the haplotype, which appears to be greater than the sum of the parts. He said, "For me the two great unknowns were and are how to measure the immunogeneity of a haplotype and how to measure the host response to an incompatible haplotype."

He was proud to have been honored by many of his students who are presently in research and teaching, as was apparent by the participation of many of them from the United States and abroad in celebration of his eightieth birthday (including the unveiling of the portrait shown in this memoir) at Duke one month before his death. He was hospitalized at the time and was not able to attend. A memorial service was held at the Duke chapel on May 24, 2003.

We do not want to interrupt
the silence of our friendship.
Why destiny build such unions
to destroy them?
It is time without repair
as we remain with memories.
Let's for a sustained moment
continue without losing what we lost.

Let's remember his enjoyment of life
with family, students and friends
near the ocean, the sea that we loved
and the mountains so near.
And, above all the need to be with children
that will carry his torch,
his pride!

He cried, our eternal friend
with perseverance, without time,
with tears of stone
and thorns of crystals.
Yes, he cried and also we did
but most we laughed
and kept the smile
to see the birth and growth
of many that are present
and others in our thoughts!
—Edmond Yunis

His memory is imprinted not only in our minds but also in generations to come, because he was one of the outstanding scientists of the twentieth century. He was one of the selected pioneers of modern immunology, particularly cellular immunology, cancer immunology, and immunogenetics. Of course, he is one of the central figures in the success of allotransplantation, especially in the development of the methods to match MHC antigens. His studies were important in the discovery of H-2 haplotypes, HLA alleles and haplotypes, the genetic order of the HLA loci, the significance of histocompatibility antigens in skin graft rejection as a test for histocompatibility, and in the outcome of kidney allografts. He was the first to use histocompatibility data to select sibling donors for kidney transplantation. He was one of a small international group of scientists, which included Rose Payne, Jean Dausset, Ruggiero Ceppellini, Paul Terasaki, Roy Walford, and Jon Van Rood, to produce what became HLA serology. This international collaborative effort led to the identification and definition of alleles of the different loci used in research of immune responses, disease association, and transplantation.

In the interview given by Bernard to Floyd Rogers in 1990 (Floyd Rogers's article of April 15 in Tarheel Sketch. "Bernard Amos." *Winston-Salem Journal.*), Bernard said that he learned immunology from Gorer in the pub (Gorer would order a scotch—and he a beer—and discuss his results). He said that "Gorer's idea was that if you knew something about the body's normal tissue, you could build on that to learn something about cancer, to find something else that was special to the cancer. From the standpoint of immunology, cancer research and transplantation research are two sides of the same coin." He was asked about the Nobel Prize. "You can never evaluate yourself. I never saw that as a serious possibility. The Nobels are good for science, but can have negative effects when investigators don't fully disseminate their findings for fear of helping a competitor. I think you realize that we're none of us in isolation. We must talk to each other; must read. And so, consciously or unconsciously, we take somebody else's ideas, and a little bit of this idea and a little bit of what that one says, and they may simmer in your mind for six months or one year and you meld them like making cheese balls. A little of port wine goes there and a little cheese, and it gets mixed together and soon nobody can identify the cheese ball."

Perhaps his most endearing quality was his altruism, which combined with his humility, showed an aspect of his life that was unusual; for most of the time after 1990 he with his wife, Kay, volunteered weekly with the Meals on Wheels program in Durham. They took meals to many poor families and they continued even when he was in poor health during the last months of his life.

D. Bernard Amos made an impact on scientific knowledge in several areas: in immunogenetics, tumor immunity, and transplantation immunology. He was a pioneer in the studies of genetics of individuality and a leader in founding important national and international scientific organizations. He trained a number of prominent scientists and established pioneering clinical and basic research programs worldwide, including developing countries. He was altruistic and ready to help not only his students but also others who needed help.

SEVERAL INDIVIDUALS provided important help in preparing this manuscript. They include Fran Ward from Duke University, Chris Amos (Bernard's son) from the University of Texas, Kay Amos (Bernard's wife), Janet Plate from Rush Medical School, Jeffrey Dawson from Duke University, and Floyd Rogers in his article in the *Winston-Salem Journal* cited above.

NOTES

1. Only two months before his death I asked him about a research problem I had encountered testing individuals exposed to tuberculosis that have a negative skin test. He remembered his NLT work of 40 years earlier and suggested that I look for antibodies to Class II

and antibodies to tuberculin that could prevent delayed hypersensitivity reactions tested by intradermal inoculation of tuberculin.

2. In this regard it is important to mention that experiments performed during the last histocompatibility workshop using a panel of monoclonal antibodies and cells typed at the allele level formally established, without cross-absorptions, that there are epitopes shared by alleles as well as private epitopes and that such epitopes are amino acid sequences recognized by antibodies.

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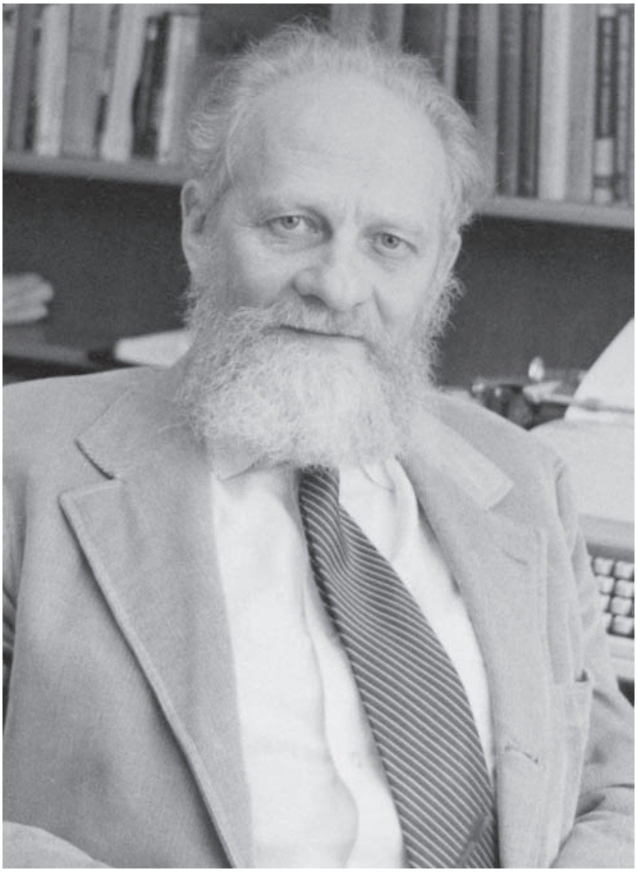

PETER MICHAEL BLAU

February 7, 1918–March 12, 2002

BY W. RICHARD SCOTT AND CRAIG CALHOUN

PETER BLAU WAS A LEADING figure in sociology throughout the second half of the twentieth century, and by its end among the most cited of all active sociologists. His major contributions were to the study of macrosocial structure—analyzing the large-scale systems of organizations, social classes, and the dimensions around which societies are structured. At the same time he was the author of an enduringly influential microsociological study of exchange relations. He was one of the founders of the field of organizational sociology and the coauthor of a highly influential study of the American occupational structure that transformed the study of social inequality and mobility. His contributions to conceptualizing and measuring the parameters of societal systems continue to inspire and guide current theory and research.

Peter was productive throughout his career, beginning with a pathbreaking and influential dissertation and first book examining the dynamics of bureaucracy. He continued to advance his macrostructural theory of society well beyond his formal retirement, submitting journal articles and working with graduate students into his eighties. He was a dynamic and inspiring teacher, and mentored a large and distinguished collection of graduate students and junior colleagues. He served as president of the American Sociological Association in 1972-1973 and was elected to the National Academy of Sciences in 1980. Colleagues will remember Peter as a man with an active interest in the world, an inquiring mind, a probing intellect, a gentle manner, a wry sense of humor, and a thick Austrian accent. He was a lover of theater and art, but most of all he reveled in the life of the mind.

A MEETING OF OPPOSITES

We think of Peter as embodying three outstanding qualities, each of which spanned or integrated two seeming opposites. (Peter was very fond of dilemmas and paradoxes.) First, his work and career connected theory and empirical research in an era when these were often disengaged activities. While he was originally attracted to "grand theory," he was converted by his graduate training at Columbia University to value theories of the "middle-range." Later, reading extensively in the philosophy of science, he developed a strong interest in formal, deductive theorizing. Throughout his entire career, however, Peter blended abstract ideas and empirical indicators and evidence. He drew from classical theory to select problems, formulate arguments, and improve interpretations. He tested propositions with survey data. But he also worked inductively: He thought with data; he learned from data. He gathered data not just to test theories but to revise and extend them.

A second type of connection was his linking of teaching and research. He was equally devoted to and strong in both. He was a dynamic, intense, even eloquent teacher. He was truly excited by ideas, and as his former students we recall many times when his mind raced ahead of his mouth so that he would become more and more excited and animated—and harder to understand—as the lecture progressed. One of his most popular courses at the University of Chicago was a required seminar on sociological theory. The "seminar" was often packed by more than 75 doctoral students, all struggling to understand and keep pace with this vigorous lecturer. Students were reminded in his class of the old adage that being a graduate student at Chicago was like trying to drink from a fire hose! Peter's influence as a teacher continued at Columbia, SUNY Albany, and the University of North Carolina. In each setting he taught not what he believed were the settled truths of science but the scientific method as a continual probing of received wisdom and pursuit of new knowledge. He was remarkably willing to see his own work subjected to revision based on new evidence or analysis and so taught his students some of their most powerful lessons by example.

The third integration embodied by Peter was his bridging of the Old World and the new. Born in Vienna, with much courage and good luck, he was able to make his way to America, where he completed his undergraduate and graduate education. Although Peter "escaped" from the Old World, he remained permanently imprinted by it. He embodied an Old World grace and charm. He was somewhat reserved and formal, even shy, in his interactions. His daughters tell of the terror he could inspire in prospective boyfriends while only intending to make small talk. Long after it was fashionable in academic circles, he wore a hat and a tie and often a heavy tweed suit no matter what the weather. And he retained his strong Viennese accent—which, indeed, seemed to get thicker over the years! In Vienna he had planned to study medicine. Yet, in the United States he enthusiastically embraced the new discipline of sociology. He was strongly committed to the conduct of positivist, empirical scientific research, rather than a looser, more humanistic "social thought." And he quickly mastered the skills of the entrepreneurial investigator, designing data-intensive studies and obtaining funding for large-scale projects. At the same time he believed that better scientific knowledge was of fundamental importance for democracy and for addressing social problems. He was as deeply committed to the ideals of a free and open society as to free and open scientific inquiry. And at the end of his life Peter worried deeply that the public conditions for both democracy and free scientific inquiry were being undermined by reactionary antimodernists.

A DRAMATIC BEGINNING

Peter Blau was born in Vienna, Austria, in 1918—the year that the Austro-Hungarian Empire fell. He was the son of secular Jews, and he watched with mounting concern the rise of fascism in postwar Austria. As a young student he wrote articles for the underground newspaper of the Socialist Worker's Party, speaking against his government's repressive regime. When he was only 17, he was convicted of high treason and given a 10-year sentence in federal prison. Ironically, he was released soon thereafter, when the National Socialists came to power and lifted the ban on political activity (Blau, 2002).

When Hitler marched into Vienna in 1938, Peter's family elected to stay, although his sister was sent to England on the *Kindertransport*. Peter, however, attempted to flee across the Czech border. He was captured, tortured, and eventually released, ending up in Prague. Returning to Austria to visit his family, which had been removed to the ghetto, Peter was able with the help of his high school teacher Fritz Redl to obtain an affidavit permitting his emigration to America. He managed to get to France by train, where because he was carrying a German passport, he gave himself in to the Allied forces. He spent time in a French labor camp, but through the intervention of an acquaintance was released when his visa number came up. He immediately left for Le Havre to seek passage on a boat to America (Blau, 2002).

Let us quote a paragraph from Peter's own matter-of-fact account of this terrible, wonderful, truth-is-stranger-than-fiction story.

> World War II broke out and the ship on which my passage was booked did not sail. Thousands of people—Americans returning home as well as refugees immigrating—waited in Le Havre for passage, and this turned out to be fortunate for me. While waiting in Le Havre for news about sailing, I met and passed the time with some Americans, one of whom was the graduate of a Midwestern protestant college. He told me that students at his college had collected a fund for a refugee scholarship, but for this scholarship they had no candidate. He asked me whether I would be interested. I could not believe my ears, and I did not believe him, honest as he seemed (and was), but I told him I would be very interested. (Blau, 1995, p. 2)

The unlikely connection was successfully made, and Peter sailed to the United States and with his refugee scholarship attended Elmhurst College in Illinois, majoring in sociology. In his undergraduate years he was attracted to the work of the grand theorists, ranging from Marx and Durkheim to Freud and Fromm, and his interests were initially more social-psychological than structural. The balance would shift progressively through his career. Following graduation from college, Peter spent three years in the U.S. Army, returning to the combat zone in Europe. Because of his German language skills, he served as an interrogation officer. He later learned that his family had been killed in Auschwitz in 1942.

A REMARKABLE COHORT

Following the conclusion of World War II, thanks to the GI Bill, Peter was able to continue his education, entering the sociology department at Columbia University in February of 1946—encouraged by his lifelong friend Lewis Coser, who entered a few months before. Attracted to Columbia by the work of Robert S. Lynd (1970), a scholar noted for his work on class and for translating sociological ideas into social reform efforts, Peter quickly fell under the influence of Robert K. Merton, the leading advocate of "middle-range" theory—theory closely related to and guided by empirical research (Merton, 1949). Merton, together with Paul Lazarsfeld, provided an alternative model to the continental tradition of "grand" theory—a tradition still entrenched at Harvard, under the sway of Pitirim Sorokin (1937-1941) and Talcott Parsons (1951). Gathered around Merton and Lazarsfeld were an extraordinary collection of graduate students, many returning veterans who were to be in the forefront of reinventing sociology for the postwar age. In addition to Blau they included Rose and Lewis Coser, James S. Coleman, Alvin W. Gouldner, Elihu Katz, Seymour Martin Lipset, Alice K. and Peter S. Rossi, Philip Selznick, Martin A. Trow, and Dennis Wrong.

A FOUNDER OF ORGANIZATIONAL SOCIOLOGY

Several of these scholars—including Coleman, Gouldner, Lipset, and Selznick—joined with Peter to launch the modern field of organizational sociology. All of them conducted insightful theory-driven, empirical studies of either public or private organizations and thus created a solid research foundation for this field of study. One of these studies was Peter's dissertation, carried out under the supervision of Merton. Drawing upon the human relations tradition in industrial sociology, Peter elected to study the behavior of work groups but with several amendments and refinements. He focused on behavior within white-collar, administrative systems rather than blue-collar settings. Following the lead of the Columbia anthropologist Conrad Arensberg (1951), he elected to systematically record interaction patterns among workers rather than basing his work exclusively on informal observations and interviews. And he addressed general theoretical questions regarding the bases of status and power, the unanticipated consequences of purposive action, and endogenous sources of bureaucratic change. The resulting study, *The Dynamics of Bureaucracy* (Blau, 1955), is rightly regarded as a sociological classic (Merton, 1990).

After his graduation from Columbia in 1952, Peter served briefly on the faculties of Wayne State and Cornell universities before moving to the University of Chicago, where he remained until 1970. While there he coauthored a treatise that became one of the foundational texts of the emerging field of organizational sociology (Blau and Scott, 1962). He also launched an ambitious research program—the Comparative Organization Research Project (CORP), which he continued after moving to Columbia in 1970. This involved a series of large-scale studies in which organizations rather than individuals were the units of analysis. He examined large samples of distinct types of organizations, including public bureaucracies, universities, and manufacturing organizations. Data were variously drawn from informant reports, official records, organization charts, personnel manuals, job descriptions, and performance ratings. The major findings were reported in *The Structure of Organizations* (Blau and Schoenherr, 1971) and *The Organization of Academic Work* (Blau, 1973).

One golden nugget resulting from this research program was, we believe, Blau's theory of structural differentiation, in which Blau devised a remarkable series of propositions to account for the complex relation between organizational size and bureaucracy (measured as a proportion of administrative staff to production workers). Based on the empirical studies he had conducted, Blau (1970) proposed that (1) size increases structural complexity (differentiation), which in turn increases pressures for coordination—the addition of administrators. But at the same time (2) size increases scale—the average size of organizational subunits—a development likely to be associated with administrative economies. Hence, size has two analytically distinct effects, which account for the indeterminant and conflicting association observed between size and bureaucratization.

A THEORY OF SOCIAL EXCHANGE

At the same time that Peter was pioneering organizational sociology, he undertook to develop a theory that would provide a more general explanation of the sorts of interactions and relationships he had observed in his field research. This led to the writing of one of his most famous books, *Exchange and Power in Social Life* (1964). Inspired by Max Weber's treatment of sociology as first and foremost about relationships, by Merton's concept of middle-range theory, and by microeconomic analysis and utility theory, Blau offered a microsociology of strategic interaction that anticipated and influenced the later rise of rational choice theory (Coleman, 1990; Cook, 1990, Homans, 1990).

Peter started from the premise that social interaction has value to people, and he explored the forms and sources of this value in order to understand collective outcomes, such as the distribution of power in a society. People enter all social interactions, Peter suggested, for the same reasons they engage in economic transactions: They need something from other people. By contrast with directly economic exchanges, other social exchanges tend to be long-term and to lack metrics by which parties can be clear as to whether their contributions are equal. Among other things, this leads to an escalation of social exchange as people strive to stay out of "debt" not only because of the norm of reciprocity but also because this gives them advantages of autonomy and potentially power. As Peter put it, "An apparent 'altruism' pervades social life; people are anxious to benefit one another and to reciprocate for the benefits they receive. But beneath this seeming selflessness an underlying 'egoism' can be discovered; the tendency to help others is frequently motivated by the expectation that doing so will bring social rewards" (Blau, 1964, p. 17).

Beginning with his early work on organizations (Blau and Scott, 1962), Peter's work consistently asked the question of who benefits, *cui bono?* Yet if he focused much attention on his own rewards it wasn't apparent to those who worked with him (perhaps such manifest altruism is only the best strategy). Certainly he received many awards and enormous recognition. He especially delighted in a year at Cambridge as Pitt Professor and a senior fellow of King's College (and brought back to the United States the rather formal custom of announcing a monthly date when he and his wife, Judith, would be "at home" for drinks). But it is crucial to remember that his formative experience was one of escape from the *Anschluss*, an almost miraculous chance to go to college, and mobility from a humble start in the United States to considerable eminence. Peter's story, in other words, was a very American story of immigration, opportunity, and social mobility. He never forgot this, and remained both humbled and grateful. He knew that both chance and social structure were crucial to his success alongside his own brilliance and enormous capacity for hard work. And he could delight in each honor and achievement as, at least in part, a gift.

A LANDMARK STUDY IN STRATIFICATION

It is appropriate too that Peter's most famous and influential book should address questions of stratification and mobility—and the distinctiveness of the American pattern in each. *The American Occupational Structure*, which Peter coauthored with Otis Dudley Duncan, was the most influential quantitative study in the history of American sociology (Blau and Duncan, 1967). It offered powerful and novel findings—such as the widespread distribution of mobility in the United States, as great numbers advanced in small steps rather than the few in giant steps, a pattern inconsistent with the country's Horatio Alger myth. The work embodied and popularized major new research methods—notably path analysis—and launched a novel approach to mobility processes that would guide a generation of work, giving rise to an entire school of "status attainment" research. The core question was to what extent factors other than parents' status explained children's status—operationalized mainly as education, occupation, and income. The more parental status explained, the more social inequalities were reproduced across generations.

Over time the enormous status attainment literature often focused on technical questions—to its critics, on explaining ever-smaller amounts of additional variance. This makes it easy to forget what a dramatic shift in the conceptualization and theorization of social inequality and mobility its origins involved. One central theme was thinking in terms of an overall occupational structure, integrating Weberian themes of status and Marxian concerns for economic inequality. There were, of course, critiques—not least of the fact that the initial work dealt with men and not women. More ironically the status attainment literature was criticized as insufficiently attentive to structural factors, too heavily focused on characteristics of individuals that aided in their attainment of higher statuses. Among the critics of this tendency was Peter Blau himself, though characteristically he didn't pause to redo the older study but rather chose to embark on a major new project.

A MACROSOCIOLOGICAL THEORY OF SOCIAL STRUCTURE

In the early 1970s Peter began an ambitious effort to develop a new macrosociological theory of social structure. This was informed by the Comparative Organization Research Project, but it was also a significant departure. In many ways it was at odds with his earlier work. Though he had moved away from the mainly social psychological interests of his student days, in his work on exchange theory and social mobility he had sought what came to be called "microfoundations" for macrostructure. He treated social relations as emergent phenomena, not mere collective aggregates of individual phenomena—and his approach to exchange theory differed from that of George Homans (1961) on just this point. But he focused mostly on the directly interpersonal patterns that might explain those found at larger scales. Now, however, he asked the reverse question. How might the macrostructure shape the patterns of more micro relations (Schwartz, 1990)?

Innovative and curious as always, Peter started a dramatically new line of theory building at a stage when many scholars attempt only syntheses of their earlier work (or simply rest on their laurels). Peter's 1974 presidential address to the American Sociological Association was the first major statement of his new theory, later developed in several books (Blau, 1977, 1994; Blau and Schwartz, 1984). New though the theory was, a key question harkened back to his earlier organizational research: How does size matter? Stimulated by Michels's theory of oligarchy and Simmel's advocacy of a formal sociology attentive to number and scale, Peter began to reason deductively about the implications of group size and rates of in-group and out-group interaction. A simple example: Assuming random interaction, any minority will have more out-group relations than a majority. Every marriage between a Christian and a Jew in the United States, thus, has a bigger impact on the Jewish population. If overwhelmingly white colleges assigned roommates randomly, nearly all black students would have white roommates while most whites would also have white roommates. From such basic effects of relative group size, Peter began to build a complex and systematic account of the social structure of populations.

Though Peter's theoretical strategy changed markedly from his work of the 1950s and 1960s, his new theory was capacious enough to allow for a reconciliation. As he showed in his 1994 book, opportunities were the products of structural contexts—whether they were opportunities for marriage, ethnic group relations, or social mobility. And social relationships were still matters of exchange and power, though they were always situated in and both made possible and constrained by larger structures (Calhoun et al., 1990).

Among other things Peter's theory gave a more structural account of "homophily," the concept coined by his mentor Robert Merton to describe the common observation that people are drawn to others like themselves. This attraction is a product of structure and not only taste, and indeed what seem individual tastes may be partly structurally produced. Sociologists, for example, are apt to spend a lot of time with other sociologists—and Peter married two fellow sociologists. With his first wife, Zena Smith Blau, he had a daughter, Pamela—herself now married to a sociologist. His second wife, Judith Blau, is a distinguished sociologist of culture, and their daughter Reva has followed that lead into art and literature. Even those with much in common are not just alike, which is after all the basis for an exchange relationship. Judith and Reva pushed Peter to see how cultural meaning matters alongside strategy and structure. And if there is one thing Peter Blau liked to exchange, it was ideas. He will be remembered for lively intellectual arguments with a twinkle in his eye and sheer pleasure in thinking clearly and well.

A MAKER OF MODERN SOCIOLOGY

In an extraordinary career of more than 50 years, Peter Blau played a central role not merely in advancing but also in *making* modern scientific sociology. Together with his teachers, Merton and Lazarsfeld, themselves only slightly older, he and others of approximately the same generation developed lines of inquiry that became the main branches of sociology, and they developed analytic approaches and a characteristic way of relating theory to research that shaped the "mainstream" of the field as it matured into a stable and cumulative science. Peter pioneered the sociology of organizations, turning the insights of Weber, Merton, and others into a highly productive research program using methods ranging from ethnographic observation to comparative statistical analysis. He was among the founders of exchange theory and shaped the emergence of rational choice theory. He recast the study of social inequality as an increasingly precise and mainly quantitative inquiry into processes of social differentiation, mobility, and reproduction. He pushed sociologists to make more use of formal, deductive theorizing. He played a leading role in putting the analysis of social structure at the forefront of the sociological agenda and developed one of the most powerful of structural theories. Remarkably, his work from each stage of his career remains not only historically influential but in active, continuous use.

For each of us, as for an enormous range of others, Peter is an inspiration. Not only did he do great work, he also did it with a true love of science, a generous spirit, a mischievous sense of humor, and a deep appreciation for the opportunities chance and social structure gave him.

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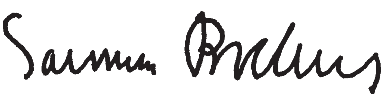

SALOMON BOCHNER

August 20, 1899–May 2, 1982

BY ANTHONY W. KNAPP

SALOMON BOCHNER WAS A mathematician whose research profoundly influenced the development of a wide area of analysis in the last three-quarters of the twentieth century. He contributed to the fields of almost periodic functions, classical Fourier analysis, complex analysis in one and several variables, differential geometry, Lie groups, probability, and history of science, among others.

He did not often write long papers. Instead he would typically distill the essence of one or more topics he was studying, begin a paper with a treatment not far removed from axiomatics, show in a few strokes how some new theorem followed by making additional assumptions, and conclude with how that theorem simultaneously unified and elucidated old results while producing new ones of considerable interest. Part of the power of his method was that he would weave together his different fields of interest, using each field to reinforce the others. The effect on the body of known mathematics was often to introduce a completely new point of view and inspire other mathematicians to follow new lines of investigation at which his work hinted.

His early work on almost periodic functions on the line illustrates this approach. Harald Bohr of Copenhagen, younger brother of Niels, had established himself as a notable mathematician by writing two papers[1] in the *Acta Mathematica* in 1924 and 1925, each about 100 pages long, introducing these functions and establishing basic theorems about them. The *Acta* at that time was the premier international journal in mathematics, and Bohr's work was considered to be of top quality. One way of viewing Bohr's theory was that the definition was arranged to give an abstract characterization of the functions on the line that are uniform limits of finite linear combinations of exponentials $e^{i\lambda x}$, the exponents λ not necessarily all being integer multiples of a single exponent. The actual definition is not particularly memorable, and there is no need to reproduce it here. The almost periodic functions are closed under addition, multiplication, and uniform limits; periodic functions provide examples. Bohr showed that any such function has what is now called a Bohr mean (in other words, that $B(f) = \lim_{T\to\infty}(2T)^{-1}\int_{-T}^{T} f(x)dx$ exists). Armed with this mean, Bohr defined a kind of Fourier expansion for these functions, writing $f(x) \sim \sum_{\lambda} a_{\lambda} e^{i\lambda x}$, where $a_{\lambda} = B(f(x)e^{-i\lambda x})$. Only countably many of the coefficients a_{λ} can be different from 0. The main theorem of Bohr's first *Acta* paper is that f is determined by its coefficients a_{λ}. In the second *Acta* paper the main theorem is the desired result that any almost periodic function can be approximated uniformly by finite linear combinations of functions $e^{i\lambda x}$.

Bohr's results had been announced in 1923, and Bochner went to work on almost periodic functions while the second of these 100-page papers of Bohr's was still in press. In the three-page announcement (1925) Bochner observed first that the function f is almost periodic if and only if every sequence of translates has a subsequence that converges uniformly; in modern terminology, *f* is almost periodic if and only if its set of translates has compact closure in the metric of uniform convergence. This definition was much easier to work with than Bohr's definition. Bochner's next observation was that the approximation theorem in Bohr's second paper could readily be deduced from the main theorem of the first paper by constructing what is now called an approximate identity, a step that Bochner carried out in short order. In effect, Bochner reduced Bohr's second 100-page paper to an argument that is so short that it can be described in a conversation. The notion of an approximate identity, not just with almost periodic functions but also throughout real analysis, has become a standard tool for reducing problems about arbitrary functions to problems about nicer functions. The Bochner definition made sense on any group, not just the additive group of the line, and Bochner had opened an avenue of investigation for someone. Indeed, John von Neumann[2] in 1934 published a generalization to all groups that used Bochner's definition and combined it with techniques from the work of Hermann Weyl. Bochner and von Neumann combined forces to write a sequel (1935) that extended the theory to vector-valued functions, no doubt motivated by the theory of vector-valued integration for the Lebesgue integral—what is now called the Bochner integral—that Bochner had introduced in 1933.

The announcement of 1925 was only Bochner's third paper. The first two, which appeared in 1921 and 1922, dealt with the subject of his thesis, a combination of Fourier analysis and complex-variable theory. In this thesis Bochner constructed, before Stefan Bergman, what is now called the Bergman kernel.[3] Bochner did not pursue the subject, while Bergman did, and thus the kernel came to be named for Bergman.

Pursuing his interest in complex analysis, Bochner wrote several further papers in the theory of functions of one complex variable. The paper (1928) in this direction, dealing with maximal extensions of noncompact Riemann surfaces, is of unusual interest not so much because of its topic but rather because it contains a comprehensive version of what has come to be known as Zorn's lemma, which Zorn apparently discovered[4] as late as 1933 and published[5] in 1935.

As a classical Fourier analyst, Bochner soon took an interest in the Fourier transform on the line and studied the multidimensional extension of it. He paid particular attention to convergence questions and to the Poisson summation formula, which relates the Fourier transform to Fourier series and is used in proving the "modular relation" that connects the values of a theta function at z and $-1/z$ in the upper half plane. His book *Vorlesungen über Fouriersche Integrale* (1932) is a classic in the subject and established his stature as an analyst once and for all. This book contains what is now often known simply as Bochner's Theorem,[6] characterizing continuous positive definite functions on Euclidean space. A continuous complex-valued function f is defined to be positive definite if $\iint \phi(x) f(x-y)\overline{\phi(y)}\,dx\,dy$ is ≥ 0 for every continuous function ϕ supported inside a finite cube. According to the theorem, such functions are characterized as the Fourier transforms of nonnegative finite measures.

In its Euclidean setting Bochner's Theorem has rather few applications outside of probability. The power of the theorem comes through its generalizations to other settings in harmonic analysis. One such setting is the theory of locally compact groups. The definition of positive definite function makes sense for such a group G if $f(x-y)$ is replaced by $f(xy^{-1})$. I. Gelfand and D. Raikov[7] observed that if U is a unitary representation of G, then $x \to (U(x)v,v)$ is positive definite for any *v* in the underlying Hilbert space, even if *U* is infinite-dimensional. Combining this observation and the Krein-Milman Theorem, they proved that *G* has enough irreducible unitary representations to separate points. Their result was part of the impetus for including infinite-dimensional representations in representation theory, a subject that has continued to grow in importance to the present day.

Another such setting is the special case in which *G* is abelian. For this special case the foundational duality theory of L. Pontrjagin went through at least three incarnations, carried out successively by Pontrjagin, by A. Weil, and by H. Cartan and R. Godement. The Cartan-Godement approach[8] starts from the results of Gelfand and Raikov. A version of Bochner's Theorem is established simultaneously with the proofs of Pontrjagin duality, the Fourier inversion formula, and the Plancherel formula for the group's Fourier transform. All four of these results have to be established together; none can be omitted in the Cartan-Godement approach. Thus Bochner's Theorem becomes part of the foundation of the theory of locally compact abelian groups. The adele and idele groups of a number field furnish important examples of locally compact abelian groups, and these theorems for such groups are essential underpinnings in the modern understanding of class field theory.

Bochner's initial multidimensional investigations of convergence questions in Fourier analysis mostly concerned rectangular partial sums. Then, beginning with the classic paper (1936), he addressed in earnest the natural question of summing Fourier series and Fourier integrals in spherical fashion. The question had been considered earlier by other authors, but Bochner brought to the question a new summability method that has come to be called Bochner-Riesz summability. This results in helpful simplifications that do not occur with related summability methods. In dimension $k > 1$, let x and y denote real k-tuples and let n denote an integer k-tuple. The Fourier series of f is $f(x) \sim \sum_n c_n e^{in\cdot x}$, where $c_n = (2\pi)^{-k} \int_{[-\pi,\pi]^k} f(y) e^{-in\cdot y} dy$ and the dot in the exponents indicates the dot product. The Bochner-Riesz sums are $S_{R,\delta}(x) = \sum_{|n|\le R} \left(1-(|n|/R)^2\right)^{\delta} c_n e^{in\cdot x}$ with $\delta > 0$. We are to think of letting R tend to infinity. Ordinary spherical convergence is the case of $\delta = 0$, and the cases $\delta > 0$ are to be viewed as easier to handle. Bochner examined the validity of the localization property (i.e., the extent to which the existence of $\lim_{R\to\infty} S_{R,\delta}(x)$ depends only on the values of f near x). He showed that localization holds for $\delta > \frac{1}{2}(k-1)$ and fails for $\delta < \frac{1}{2}(k-1)$. A similar conclusion holds for Fourier transforms.

Bochner returned to these matters in the early 1950s. Spherical summation inevitably leads one to Bessel functions, and Bochner was led to combine his knowledge of Bessel functions with that of the "modular relation" in (1951) to give a complete analysis of the effect of rotations on the Fourier transform in k-dimensional space R^k. Any function on R^k is a suitable kind of limit of linear combinations of functions $g(|x|)H(x)$, where $H(x)$ is a harmonic polynomial. Bochner showed that the Fourier transform of such a product is of the form $(Tg)(y)H(y)$, where Tg is given in terms of g by an explicit one-dimensional integral involving a Bessel function, called a "Hankel transform." In a paper the next year he extended his work on Bessel functions by obtaining transformation formulas for what have come to be called Bessel functions of a matrix argument.

These topics were taken up by Bochner's student Carl Herz. For the case of a radial function f with Fourier transform $\hat{f}$, Herz examined the sense in which f could be recovered as the limit on R of the inverse Fourier transform of the product of $\hat{f}$ by the characteristic function of the ball of radius R centered at the origin. He showed[9] that if f is in L^p and $2k/(k+1) < p \leq 2$, then the approximations converge to f in L^p. In the direction of positive generalizations, E. M. Stein later obtained analogous results for convergence when f is not necessarily radial but the truncated Fourier transforms are replaced by Bochner-Riesz approximations to the truncations; Stein obtained norm convergence for an interval of p's that depends on the Bochner-Riesz index δ. For $\delta = 0$, Stein obtained nothing new—only the convergence in L^2. Stein made critical use of an observation that although the restriction of the Fourier transform to a hyperplane does not make sense for an L^p function when $p > 1$, there is a nontrivial interval $1 \leq p < p_0$ such that restriction to a sphere makes sense for the Fourier transform of an L^p function. The interplay between curvature of a set and the meaningfulness of the restriction of a Fourier transform to the set was studied extensively by later authors and continues to be a subject of investigation. In the direction of negative generalizations of the work on spherical summability, C. Fefferman ultimately proved that the Herz approximations for a nonradial function f need not converge in L^p except for $p = 2$. Thus the use of Bessel functions is an essential aspect of the theory. Herz[10] took up another topic of Bochner's and developed a substantial theory of Bessel functions of a matrix argument. Later K. Gross and R. Kunze generalized aspects of the Herz theory and related these matters to the subject of analysis on semisimple Lie groups.

In the subject of differential geometry Bochner is best known for his stunning quantification of the century-old idea that the curvature of a compact Riemannian manifold can force global topological conclusions about the manifold. This curvature-topology work was initially encapsulated in a single formula (1946) and its variations and applications. Of Bochner's formula M. Berger writes[11]:

The Bochner article [(1946)] will remain an unavoidable cornerstone of transcendental methods linking the local geometry to global properties of the underlying space. Bochner calculated the Laplacian of the norm squared of a differential 1-form ω on a Riemannian manifold [in terms of the covariant derivative of ω, the Hodge Laplacian $d\delta + \delta d$ of ω, and the Ricci curvature tensor applied to ω].

In reviewing this paper S. Myers lists some consequences of the formula and its variations for compact manifolds[12]:

For example, (1) a compact *M* with positive mean [=Ricci] curvature has no vector field whose divergence and curl both vanish, (2) a compact *M* with negative mean curvature has no continuous group of isometries, (3) a compact *H* with negative mean curvature has no continuous group of analytic homeomorphisms, (4) a compact *H* with negative (positive) mean curvature has no analytic contravariant (covariant) tensor field, (5) if a compact *H* with positive mean curvature is covered by a finite number of neighborhoods, if a meromorphic functional element is defined in each neighborhood and if the difference of meromorphic elements is holomorphic whenever the elements overlap, then there exists one meromorphic function on *H* which differs by a holomorphic function from each meromorphic element given.

Bochner pursued this topic for five or six years, writing several papers, one of them joint with K. Yano, and ultimately publishing the book *Curvature and Betti Numbers* (1953) jointly with Yano.

Other mathematicians developed this topic in two quite distinct directions. K. Kodaira worked with complex Kähler manifolds, which include all nonsingular projective algebraic varieties, and arrived at the celebrated Kodaira Vanishing Theorem. In the paper[13] in which this theorem is proved Kodaira writes, "In the present note we shall prove by a differential-geometric method due to Bochner some sufficient conditions for the vanishing of [the sheaf cohomology spaces] $H^q(V;\Omega^p(F))$ in terms of the characteristic class of the bundle *F*." This theorem is fundamental in modern algebraic geometry. Sixteen years later P. Griffiths and W. Schmid[14] adapted to infinite-dimensional representation theory the idea that curvature conditions can imply vanishing of sheaf cohomology, and sheaf cohomology became a tool for realizing interesting infinite-dimensional representations of noncompact semisimple Lie groups.

A. Lichnerowicz took up aspects[15] of the theory for noncomplex manifolds. He obtained different applications of Bochner's original formula and also obtained additional formulas of his own. One of the latter applied the Bochner technique to the spinor fields on a spin manifold, yielding a formula[16] relating the square of the Dirac operator, the covariant derivative, and the scalar curvature. M. Gromov and H. B. Lawson[17] combined this formula with work of A. Borel and F. Hirzebruch and with the Atiyah-Singer Index Theorem and were able to classify all simply-connected compact manifolds admitting a Riemannian metric with positive scalar curvature.

In the late 1930s Bochner began a systematic investigation of functions of several complex variables. Robert Gunning, the editor of Bochner's collected papers and a student of Bochner's from the 1950s, summarizes this work as follows[18]:

Bochner's interest in functions of several complex variables began with their Fourier analysis, leading to his characterization of the envelopes of holomorphy of tube domains [(1938)]. He later wrote on generalizations of Cauchy's integral formula for functions of several variables (including what is known as the Bochner-Martinelli integral formula [(1943)]) and applications of these formulas to analytic continuation on singularities of analytic spaces [(1953)] and on conditions for the analytic and linear dependence of complex analytic functions in various cases. The book *Several Complex Variables* . . . (1948), written jointly with W. T. Martin, summarized much of his earlier work and his own outlook on the subject.[19]

About the book (1948) S. Krantz says,[20] in reviewing the volumes of collected papers, "The book by Bochner and Martin . . . was among the first on the subject of several complex variables; although there are now many books on the subject, that volume is frequently cited in the modern literature." About Bochner's work as a whole, Krantz continues, "Not only did Bochner touch many areas of mathematics, but his ideas are so profound that they are still of great interest today."

Salomon Bochner, son of Joseph and Rude Bochner, was born on August 20, 1899, into a Jewish family of modest means in the Polish city of Krakow, which was then part of the Austro-Hungarian Empire. His brilliance was already evident to the teachers in his Jewish elementary school, and when Bochner was nine years old, one of them predicted that he would make his living as a mathematician. In 1915 shortly after the outbreak of World War I, the threat of a Russian invasion of Austria-Hungary led the Bochner family to flee to Germany, which at that time was seen as more hospitable to Jews than was Russia, or even Austria-Hungary. One example of this greater openness was the fact that, unlike in Krakow, the state schools, including the prestigious gymnasia, made accommodations for orthodox Jewish children whose religious practices did not allow them to write on Saturdays, which was a school day. When his family arrived in Berlin, Bochner immediately took the entrance examination for a gymnasium, without having studied much

German, and he received the highest score in the city, which garnered him financial support from a wealthy Berlin Jew. At the gymnasium he developed a great love for classics and history, which he maintained throughout his life, but he chose to pursue mathematics professionally, because he felt that it was a surer career path.

He received his doctor of philosophy degree from the University of Berlin in 1921. The elder Constantin Carathéodory and he became good friends during this time. According to an online mathematics genealogy project,[21] Bochner's thesis adviser was Erhard Schmidt. In later years Bochner would not say much about Schmidt. Instead he would occasionally say, with a little smile, that in his observation, a mathematician often took after his mathematical grandfather. In Bochner's case this was David Hilbert.

The time when Bochner got his degree was a time of hyperinflation in Germany, and his family was in desperate straits financially. As a consequence Bochner did not immediately take an academic job but instead went into the family import-export business, doing mathematics only recreationally. Over a period of four years he did extremely well at the business. Despite this success his family could see that his real interest was in mathematics, and they encouraged him to return to mathematics full time. He did so, and on the basis particularly of his paper (1925) he became an International Education Board fellow at Oxford University, Cambridge University, and the University of Copenhagen for 1925-1927. In England he became good friends with G. H. Hardy, and they wrote one paper together. In 1927 at the end of the fellowship he became a lecturer at the University of Munich.

Like many untenured academic Jews in Germany, Bochner was dismissed from his position during the 1932-1933 year. For the second time he became a refugee; he went to England, a country he had come to love during his stay there in the 1920s, and asked Hardy for help in getting a position. Meanwhile, an offer arrived from Solomon Lefschetz, who was the first Jewish professor to have been hired by Princeton, and Hardy encouraged Bochner to accept the offer rather than to try to stay in England, which was rapidly becoming overcrowded with German academic refugees. Bochner accepted the offer and left for America alone, becoming an "associate" at Princeton for the 1933-1934 year and an assistant professor starting in 1934.

During the 1930s he would travel every summer to Germany to visit his family, and in 1938 he helped his family immigrate to England and get properly settled. It was on one of these voyages that he met Naomi, his wife-to-be, an American traveling to Europe on a vacation. They were married on Thanksgiving Day in 1938, with John von Neumann as best man. After their marriage the Bochners developed lifelong friendships with Marston Morse and his wife, Louise, as well as with Eugene Wigner and his wife, Mary.

Bochner was promoted to associate professor in 1939 and to professor in 1946. During this period in his life, Bochner was a part-time member of the Institute for Advanced Study for 1945-1948, a lecturer at Harvard for the spring semester of 1947, a consultant to the Los Alamos Project in Princeton in 1951, and for 1952-1953 a visiting professor in the Department of Statistics at the University of California, Berkeley.

In 1959 Bochner was appointed Henry Burchard Fine Professor of Mathematics, and he held that position until his mandatory retirement from Princeton in 1968. He was then immediately appointed E. O. Lovett Professor of Mathematics at Rice University, a position he held until his death in 1982. For the interval 1969-1976 he was chairman of the department. The atmosphere at the two institutions was quite different. At Princeton younger people in the department who knew him would refer to him in the third person as "the Master" or sometimes "Himself." The staff called him "Professor Bochner" in recognition of his endowed chair; ordinary professors were simply "Mr." At Rice, however, the environment was more relaxed, and a number of people called him "Sal." While still at Princeton, Bochner himself commented, "Princeton has more prima donnas per square foot than any other place in the world."

Bochner was elected to the National Academy of Sciences in 1950. He was an invited speaker at the International Congress of Mathematicians in 1950, gave the Colloquium Lectures of the American Mathematical Society in 1956, and was keynote speaker at the AAAS Symposium in 1971 on the "Role of Mathematics in the Development of Science." In January 1979 the American Mathematical Society awarded him the first Leroy P. Steele Prize for Lifetime Achievement, citing him for "his cumulative influence on the fields of probability theory, Fourier analysis, several complex variables, and differential geometry."

As Bochner grew older he partly turned from mathematics to classics, philosophy, and the history of science and of mathematics. He regarded this move not as a forced retreat from his chosen field but rather as an opportunity to return to the humanistic interests that had engaged him in his youth. He was most proud of his book *The Role of Mathematics in the Rise of Science* (1966), which went into paperback. During his Rice years he became close colleagues of the historians of science and received much acclaim for a public lecture on Einstein, delivered in honor of the centenary of Einstein's birth.

The Bochners had one child, Deborah, who became Deborah Bochner Kennel. Trained as a Renaissance historian, she is at this writing in 2003 working as a writer and editor for the Center for Medieval and Renaissance Studies at the University of California, Los Angeles. She has two children, who both chose Princeton for their undergraduate educations. Matthew Bochner Kennel is an assistant research physicist at the University of California, San Diego, and Sarah Alexandra Kennel is an assistant curator at the National Gallery of Art in Washington, D.C. Deborah described Salomon Bochner as a very attentive father, who gave lifelong unconditional love and, as she matured, intellectual stimulation and companionship in a wide variety of humanistic subjects. She commented also that he was witty, with an intellectual formation typical of the prewar continental academic mode, and was also a strong Anglophile. She said he enjoyed describing himself as having been "born under Victoria." Deborah added that he definitely had his idiosyncrasies: he disliked both picnics and barbecues, always repeating that "it took man millions of years to learn to cook and eat inside and I don't see why I should reverse the process." This attitude was consistent with various comments he made to his colleagues, such as, "Scenery is for adolescents—of all ages."

At the time of the move to Rice the Bochners rented an apartment in Houston but continued to keep their house in Princeton. They would travel from one place to the other seasonally, and on occasion would visit their grandchildren in Los Angeles, where Deborah had settled with her family. While they were on a trip to Los Angeles in 1971, Naomi died unexpectedly, and her husband soldiered on alone at Rice. He developed eye trouble and a heart condition. In 1981 he had successful cataract surgery on one eye, but in 1982 he had a heart attack during surgery on the other eye, and died a few days later on May 2, 1982.

In his time at Princeton, Bochner took a few young faculty members under his wing as postdocs, officially or unofficially.

One of these was K. Chandrasekharan, with whom Bochner jointly authored a book (1949). Another was a young functional analyst from Yale, Robert Langlands. Bochner pushed Langlands in the direction of algebraic number theory, arranging for him to teach a course in class field theory. One of Bochner's thesis students, William Veech, remembers passing Langlands in the hall one day in the 1960s and asking Langlands what he would do next. His response was "noncommutative class field theory." Indeed he did; aspects of the work by Langlands played a crucial role 30 years later in the proof of Fermat's Last Theorem. According to Veech, Atle Selberg thanked Bochner publicly at a banquet in 1969 in honor of Bochner's seventieth birthday for having sent him at an early stage some papers by Langlands, who Selberg said, "is now one of the best mathematicians in the world."

Veech went on, saying that Selberg, in that same brief talk, mentioned that once in conversation with Hermann Weyl, Weyl remarked something close to, "Now Bochner, he is really somebody." Veech recorded in his May 1982 eulogy of Bochner a further memory of that banquet: After all the banquet talks had been completed, Bochner himself "was invited to make some remarks, of which he had but one: In the 1930s there was a trolley car that ran from Princeton to Trenton and back. Bochner's one regret in life, he confided to the hushed assembly, was that he had never ridden that trolley."

The online mathematics genealogy project[22] lists Bochner as having 38 doctoral students. I was one of the last, finishing in 1965. Bochner was not someone to whom students flocked, and he actually had no current students in the semester before my qualifying examination. Bochner's student Veech, who had recently graduated and had stayed on as an instructor, pointed out to me the advantages of seeking

Bochner as adviser. I found that Bochner was awe inspiring, yet approachable and not particularly intimidating in person. This man had had, after all, 40 more years of experience at mathematics than I had had, but he still made me feel that I could produce something new that would interest him.

After I had passed my qualifying examination, Bochner gave me a warm-up problem, which took two weeks to solve, and then I was on my own to produce a thesis. The advice he offered was more philosophical, or sometimes sociological, than mathematical. Mathematical advice was left to another, earlier Bochner student, Harry Furstenberg, who was visiting Princeton for a year.

The piece of philosophical advice that I remember most vividly, and would always pass along to my own students, was, "Theorems come from theories, and not the other way around." On one occasion he said, "Young mathematicians work on theorems, mature mathematicians work on theories, and elderly mathematicians work on theories about theories."

At some point Bochner told me that part of his job was to keep me on an even keel emotionally, picking me up when I was down and knocking me down a bit when I was too confident. After I had produced a first theorem and cheerfully proposed to show it to him, he peered at me while walking with me toward his office and asked, "Is it earth shaking, earth shattering, or earth annihilating?" Later on, when I had assembled a body of my own mathematics and we were discussing it, I said dejectedly that it all seemed so trivial now. He responded, "Yours is experience number 13765972 of this kind [or perhaps it was some other large integer]. Everyone has this kind of experience. It means that you finally have understood what you have done."

At another time he said that he did not want to be a father figure to me. This was a comment whose complexity

I still have not fully understood. Perhaps this was just a pithy comment of the kind that he would often make on the spur of the moment. Or perhaps he knew that my father had died unexpectedly a year before I arrived in Princeton.

At some point when I was well along toward a thesis, he and I had a conversation about his experience with different branches of mathematics. He said that he deliberately chose to avoid competitive areas. Only later would I understand that he had, in fact, created a number of areas and then left them when other people took them up.

I am indebted to Deborah Bochner Kennel for extensive help in preparing this article and to Robert Gunning and William Veech for offering useful information and comments.

NOTES

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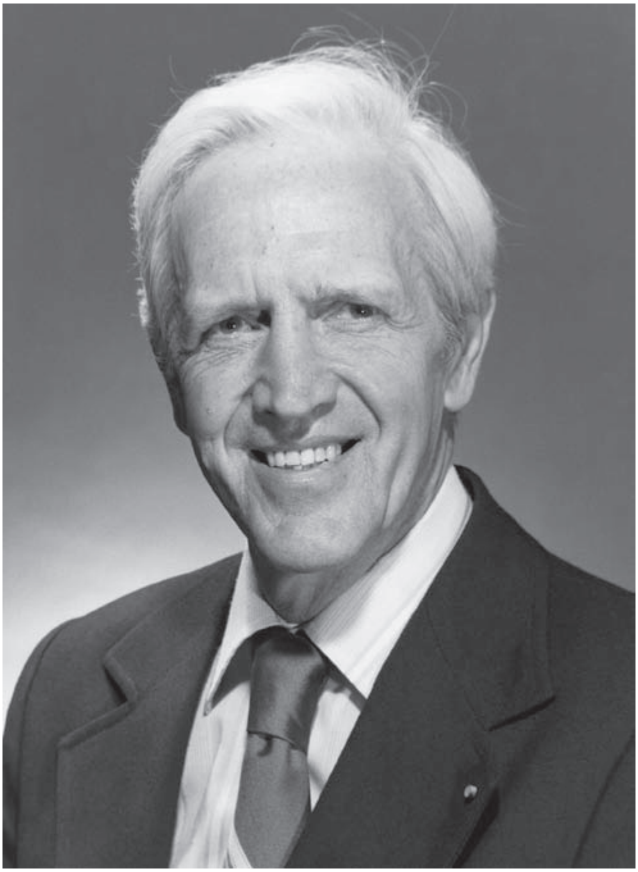

C C Cutler

C. CHAPIN CUTLER

December 16, 1914–December 1, 2002

BY PING KING TIEN

THE ECONOMY WAS IN a depression between 1929 and 1934, and the country went to World War II between 1941 and 1945. C. Chapin Cutler built his character and his strength in those chaotic years. He led a successful career of research in communication science for more than four decades. His inventions in radio, radar, signal coding, imaging, and satellite communications earned him more than 80 patents, numerous awards, and a worldwide reputation. Shortly after he received the Alexander Graham Bell Medal from the Institute of Electrical and Electronics Engineers (IEEE), Cutler said in the spring 1992 issue of the *WPI Journal,* "I don't think I'm really that smart. I just think my imagination got turned on at an early age and that gave me tremendous motivation."

Cassius Chapin Cutler was born on December 16, 1914, in Springfield, Massachusetts. He was the son of the late Paul A. and Myra (Chapin) Cutler. He was raised in a small town environment and was educated in the public school systems of western Massachusetts. His resourcefulness and ingenuity had their roots in his youth and later in his education at Worcester Polytechnic Institute.

In 1929, radio was very popular with young people. At age 14 Cutler played with elementary crystal receivers and his grandfather's three-tube radio, mostly listening to the air broadcasts. Radio technology was still a mystery to him. When school started that fall, he went to the library to look for books on radio. Later he met his friend Larry Reilly. Reilly gave him a copy of *Radio Craft* magazine containing an article, "The Junk Box Radio." Cutler later wrote in his journal, "This, I believe to be the most crucial event of my life."

"That's how I started," he said, "I built the junk box radio receiver, using parts from a defunct broadcast set. I screwed the parts onto a pine board and used a single old vacuum tube. I salvaged even the wire and the solder from the old radio and used my dad's soldering iron heated on the kitchen stove." After the radio was built he heard "dit, dit, dit, dah, dit, dit, dah" from a station in Mexico City, and he was forever hooked on radio.

Shortly after, his father took him to a talk given by a visiting scientist from the newly established Bell Telephone Laboratories. The talk was "The Wonders of Radio and Communication." It was a popular talk with demonstrations. The speaker modulated a neon bulb, talked over a light beam, and demonstrated inverted speech. That was when Cutler learned of Bell Laboratories and decided what he wanted to do with his career. Experimentation with electronics soon became his avocation, and he supported his hobby by baking beans and selling them to neighbors.

In the summer of 1933 Cutler graduated from the Springfield Technical High School. His father and mother were determined that he should go to college, but no one in the family had college experience and none of them had any idea of how to enroll. One day in August he was stopped by his neighbor Eddy Milde, then a graduate student and a track star at Worcester Polytechnic Institute (WPI). Eddy encouraged Cutler to apply to WPI and gave him an application form. A week later Cutler visited the campus and was interviewed by Professor Zelotes W. Coombs. Later that day Professor Coombs told him he was accepted provided he did well in the first semester. When Cutler returned to Springfield, the whole family was elated. His Christian Science practitioner, Evangeline P. Walbridge, came for the occasion and said, "The ONE MIND, Infinite Intelligence is yours. Whatever it is your duty to do, you can do. . . . God gives the increase. . . ." Cutler did not appreciate what she said until much later.

These were the Great Depression years, and there was a problem with finances. The money he saved from selling beans was less than $100. His mother gave him $300 from Grandfather Chapin's estate, and his dad obtained a loan from an insurance policy. Cutler had enough to start for the first year: $135 for room, $270 for board, $350 for tuition, and more for supplies. Cutler was excited. Mrs. Walbridge built his confidence by saying, "God supplies all our needs, only requiring that we be worthy."

Cutler soon learned of the "Fuller Scholarship for Yankee Ingenuity." He worked day and night cleaning up his radio station, photographing and writing up the project. He got advice from many. Dorothy Noble helped put the story together and typed the report. He did not win, but he was cited as the runner-up; and, with that in his record, he later received special attention at WPI from professors and employers.

Worcester Polytechnic Institute, established in 1865, was the third engineering school in the United States, after Rensselaer in 1824 and the Massachusetts Institute of Technology in 1861. It is situated inside the city of Worcester, which in the late 1800s became a major manufacturing center, particularly in the machine tool industry.

The founders of the school believed that students should work with various metal and woodworking tools so that they would be familiar with the capabilities and limitations of the manufacturing process. They selected the motto, "Lehr and Kunst," which in German means "learning and skilled art." To prepare future generations of engineers, students were encouraged to discover, to create, to innovate, and to lead. In addition to academic courses, students were encouraged to work with the community and learn to solve practical problems. The institute produced great leaders in government and industry, and Cutler was among them.

The first day in Worcester was memorable. It was a gregarious class of 126 boys. Cutler and his roommate, Earl Curtis, were assigned a room in the new Sanford Riley Hall in the corner of the first floor overlooking the city. Most of the classmates were from the working class or laboring class and not much better off financially than Cutler. To support themselves the students took such odd jobs as cleaning windows, washing cars, shoveling snow, and tending coal-fired boilers. They were typically paid 40 cents an hour. The instructor in English, Paul Swan, ran the College Student Christian Association (YMCA), which was the contact point for the jobs. In addition, the school provided a few scholarships. Cutler earned more than $700 each year, which with the scholarships, was more than enough to cover his expenses.

Students at WPI pioneered in amateur radio as early as 1911. They obtained call sign 1YK, which was later W1YK. The Worcester Tech Wireless Association, later named the WPI Radio Club, is reputed to be the oldest college radio station in the United States, but it was not successful when it first went on the air. Cutler joined the club soon after he became a student at WPI. Partly because he was the only ham with a two-letter call, he was elected as the chairman of the Transmitter Committee; later he was elected vice-president and then president of the club.

One day Cutler went to the electrical engineering department looking for books on radio. He met Professor Hobart H. Newell, a radio pioneer, and Victor Siegfried, a beginning instructor with an advanced degree from Stanford University. Siegfried introduced to him the new *Radio Engineering* textbook by Frederick E. Terman. Much of the text was over his head, but in the library he found *Shortwave Wireless Communication* by Ladner and Stoner. He devoured it eagerly. In the first year at Worcester, whenever Cutler got ahead of his homework, he studied radio.

As the chairman of the Transmitter Committee in the radio club, Cutler was soon charged with the responsibility of getting the radio station on the air. He redesigned the station and the QSL card. In 1936 the club entered the ARRL sweepstakes and ran up a commendable score of 20,451 points. In the meantime the school provided a pair of high-power (100 W) transmitter tubes, which generated a great deal of enthusiasm in the electrical engineering department.

During one Saturday night Cutler operated in a radio contest, in the windowless third floor of the Atwater-Kent Building. When he emerged from seclusion the next morning, he found that it had snowed all night. He immediately found jobs shoveling snow and he worked until five in the afternoon. He was $6 richer but very hungry. That night he ate two dinners and still had almost $5 left.

During four years in college, Nate Korman was Cutler's close friend. They worked together, intellectually challenging and supporting each other. In the second year at WPI they shared a rented room at 47 Institute Road. Both of them intended to major in electrical engineering, but they wanted to learn more about advanced physics and mathematics. With the support of Professor Newell they switched to general science, a major that had almost entirely elective courses. Years later Nate had a successful career at RCA, while Cutler prospered at Bell Labs. Cutler did well in college, and in the summer of 1937 he graduated with distinction (seventh in his class) from Worcester Polytechnic Institute with a degree in general science.

In the summer of 1934 Cutler worked as a chauffeur for Mr. T. Hovey Gage, a lawyer in Worcester, for $105 a month. On Memorial Day weekend he drove Mr. Gage to his summer residence in Maine. It was a 180-mile, eight-hour trip. Top speed was 12 mph on backroads, sometimes dirt roads. On entering Waterford, Maine, on the dirt road from Bridgeton, Cutler observed two very attractive girls on the street in bathing suits, buying fish from the traveling fishmonger. Later in the evening Cutler was invited to a party of young people at the Wilkins Community House, where he met the girls he had encountered earlier. The most attractive girl was Virginia Tyler. At that time he knew he wanted to be close to Virginia Tyler.

During that summer Cutler drove again and again to Maine, where Mr. Gage provided a room for him at a local inn, the Lake House. He swam in the lake and pitched horseshoes with the local boys beside Round's store. Some of the young people had formed an orchestra, The Rhythm Ramblers, which practiced evenings in the community house. Virginia was the pianist and sat facing the side window. The sight of her concentrating on the music, framed by the window, Cutler described as a most attractive scene.

One evening Virginia, Christine McKean, and Cutler were in Waterford village looking for adventure. Virginia suggested that they go canoeing, using the bishop's canoe, the *Mary B*. It was a beautiful night and terribly romantic, flooded with the light of the moon. They played Truth or Consequences (without the consequences). Afterward Cutler walked Virginia home. They stood in front of Round's store, under her living-room window, for a long time talking about anything and everything.

After the graduation from WPI, Cutler's life was very busy. On September 27, 1941, Virginia Tyler and Chapin Cutler were married in Waterford, Maine. They had a beautiful church wedding with Mother, Brother Lee, Sister Natalie, Virginia's family, and most of the Waterford village present (about 100 people). Chapin's Cousin Wilbur was the best man.

After graduation from WPI, Cutler applied for employment at Bell Telephone Laboratories. Job opportunities were scarce in 1937; the economy had not quite recovered from the Depression and the laboratories, to reduce the costs, were open only four days a week. Cutler's interview was at 463 West Street in New York City on the Hudson River waterfront. During the interview he met an erudite fatherly figure, Ralph Bown. Bown had pioneered in military radio communication during World War I and had made his way through the U.S. Army Signal Corp, Western Electric, and into management at Bell Labs. Cutler described his antenna experiments and attempts at WPI to carrier-depressed modulation for communication. Bown was impressed, and they shared the vision that the science of the future was radio.

There were no openings in the research departments in New York City, but Cutler was offered a position at a branch laboratory in Deal, New Jersey. At Deal, research and development was centered on shortwave radio, high-power transmitter tubes, new antenna designs, and ionospheric radio propagation. They were all areas close to Cutler's interests.

The Deal laboratory consisted of six buildings, a few shacks, and outhouses scattered over a 360-acre field. The buildings were overshadowed by 150-foot steel towers, wooden antenna poles, and mysterious antenna arrays. The main building built before 1920 had recently been renovated for shortwave radio experiments in preparation for telephony across the seas. Other buildings included a machine shop, a garage, a classic “Harvey” farmhouse, and two temporary wooden frame structures that housed the shortwave and the longwave laboratories. There was a tennis court, a picnic area, and a softball field in a grove of giant maple trees. The site was on prime fallow, roughly mowed farmland, with Whale Pond brook running though the center.

Cutler’s subdepartment head and later department head was John C. Schelleng, a veteran of the Army Signal Corp. Schelleng was well known for his paper on ionospheric radio propagation. Cutler’s close associates were J. Peter Schafer, their supervisor; James Wilson McRae, a recent Ph.D. graduate from Caltech; and Thomas G. Morrisey, who was Cutler’s age and just as eager.

Cutler and McRae shared an office and half of the transmitter lab on the second floor of the main building. Initially they were assigned to design a high-power transmitter at 23 MHz using 25-kW experimental tubes supplied by Sid Ingram in New York. They used a feedback amplifier configured as the transmitter stage, which required a delicate balance between the stray capacitances.

Cutler called his first invention the “self-neutralized amplifier” because it balanced the internal tube capacitances, plate to cathode and grid to plate, against each other to prevent capacitive feedback. The grid and cathode were driven by the signals opposite each other in the optimum ratio. It proved to be stable, gave sufficient radio frequency feedback, improved linearity, and provided reasonable input impedance. One day Mervin Kelly, then director of research and later vice-president, came to Deal for a visit. Cutler showed him the transmitter, and Kelly said, “Oh, you are the one who invented the new amplifier.” Cutler was thrilled.

One day Schelleng told Cutler, “Do what you want. You don’t have to check with me. Tell me about it only when you want to.” He gave him complete freedom, with full confidence that important results would be forthcoming. It was the culture and the wisdom of the management that made Bell Laboratories renowned worldwide, and the best place for research.

After gaining experience on the 25-kW transmitter tubes, Cutler and McRae embarked on another adventurous project: the development of a 200-kW transmitter to operate at frequencies switchable from 4 MHz to 23 MHz with feedback over four stages of amplification. The objective was to provide 12-channel, single-sideband, multiplex telephony between the United States and England. They worked for two years until 1940, when Bell Labs was diverted into military work in preparation for war.

In 1940 Schelleng asked Cutler to work on the proximity fuse. The idea was to install a radio circuit in an explosive shell that would be shot from the ground toward an enemy airplane. The circuit would sense the proximity of the airplane and send a signal to the ground to detonate the shell. Cutler designed the circuitry and tested the fuse at Aberdeen Proving Ground in New Jersey and Indian Point in Maryland. The project was shortened by the success of a self-contained triggering circuit.

Late in 1941 McRae and Cutler were asked to design and build waveguide plumbing for an X-band aircraft antenna. They received lots of advice. The world’s waveguide inventors and experts, George Southworth, Arnold Brown, and Archie King, were still at Holmdel, and Sergi Schelkunoff was only a phone call away. Cutler successfully built waveguide elbows, rotating joints, and connectors.

At that time one had to build one’s own testing gear, including power supplies. McRae wanted to test the waveguide circuitry and the antenna as a full working assembly. The antenna and the waveguide feed were designed at the facility in Whippany, New Jersey. McRae built the assembly according to their design and mounted one antenna on the second floor of the main building and another in a remote location.

They were able to measure the directivity pattern and field intensity versus elevation angle and azimuth. They obtained good pattern in the E plane or in the H plane but had to adjust the structure between measurements. The beam width was about three degrees as required, but side lobes were one-tenth as strong as the main beam in one plane or the other, not close to the one-half percent power level required. Cutler hastily constructed more apparatus for measurements of amplitude, phase, and polarization of the radiation from the antenna feed. He tried various configurations of the assembly. Nothing seemed to work.

In the midst of this work McRae was called to Washington to guide the Army Signal Corps into the new age of radar. (Years later he returned to the labs as the department head, director, and vice-president.) Cutler was left alone with the antenna project.

"Late in the night, abed," Cutler wrote in his notes, "it all came together in my mind. In the morning, I slapped my vision together with copper foil, solder and sealing wax, and I had quite a different horn structure and a good radiation pattern. I slimmed down the waveguide and channeled the energy into two relatively narrow slots on each side of the guide. I called it the Waveguide Splitting Head. By varying its shape and size, I found a simple way to match the impedances."

It was indeed a novel, ingenious design of the antenna feed. The two slots were located exactly half a wavelength apart. The radiations from the two slots reduced the energy in the side lobes and reinforced the energy in the main beam. He used a screw in the splitting head to adjust field distributions in the two slots. It was simple, and it was reliable.

The waveguide antenna system, dubbed the "Cutler feed," was produced by the thousands and was aboard every Allied bomber in the latter part of World War II. Overnight Cutler became known as a radar expert and was consulted on various antenna designs. In the meantime he invented a variety of antenna feeds, including the corrugated waveguide, which years later was used in microwave devices. When radar was unveiled to the public in 1945, an artist's rendition of the Cutler feed appeared in the August 20 issue of *Time* magazine. By then Cutler had the fame, a beautiful family, and a job he loved.

In March 1944 with the war winding down, they began to work off the huge backlog that had accumulated in the telephone plant. AT&T announced a crash program to build an intercity microwave relay system from New York to Boston for both television and telephone signals. The system involved the construction of a series of radio relay stations about 30 miles apart with 3 MHz of bandwidth, in 4 GHz channels. They selected the close-spaced triode for the repeater amplifier.

Returning to Deal from the wartime assignment, Schelleng invited Cutler to listen to a presentation by John R. Pierce, who was going to describe the traveling wave tube (TWT) that he had learned about in his recent trip to England. The TWT was invented by Rudolph (Rudi) Kompfner in England. It was a major breakthrough for microwave circuits. The bandwidth of a triode was limited by electron transit time, and interelectrode capacitances. In the TWT, amplification was obtained by the extended interaction between propagating waves and a beam of electrons. It eliminated virtually all the bandwidth limitations. At that time Schelleng and Pierce thought the TWT would be the next generation of repeater amplifiers.

Cutler was asked to study the circuit problems of the TWT and to move his laboratory to the newly constructed research center in the suburb of Murray Hill, New Jersey. Most of the activities in New York City had already moved to Murray Hill as the operations of Bell Labs rapidly expanded during the postwar period. The TWT faced several difficult technical problems, and there was a flurry of activities designed to overcome them. Pierce started the analysis and Cutler started the measurements. For years TWTs were made in a specialized shop where it took weeks to construct a single tube. Cutler longed to be able to make his own tubes. He studied vacuum systems and built his own pumping station. It was not easy. "I never did get the station clean enough for the oxide cathode," he wrote in his notes, "but I do not need to." He used a thoriated tungsten cathode button heated white-hot by electron bombardment from another electrode in the tube. The wonderful thing was that in a matter of hours he could open the vacuum chamber and change the parts.

Calvin F. Quate joined Bell Labs in 1950 and Rudi Kompfner in 1951. By then Cutler was promoted to department head reporting to Pierce, who had been newly promoted to director. With his analysis Pierce deduced that noise on the electron beam due to thermal emission of electrons should appear as waves on the beam. It was not obvious at the time that anything as random as noise could propagate in the form of waves. Cutler and Quate set up an experiment to verify Pierce's theory. They projected an electron beam through the center of a toroidal resonant cavity in the newly designed pumping station. The cavity could move along the beam. They measured noise level excited in the cavity and found the waves predicted by Pierce. That was the famous Cutler-Quate experiment.

Herwig Kogelnik wrote in his notes, "When I first learned of Chap Cutler, I was still in graduate school in Vienna. It was in the late 1950's and my PhD thesis blended in the Vienna group's effort to reduce the noise in traveling wave tubes. This was the widest band amplifier for microwaves known at the time and many groups all over the world were trying to improve the performance. Our bible for this work was the famous and fundamental Cutler/Quate experiment described in a 1950 *Physical Review* paper. Little did I know at the time that Chap would be my first Director at Bell Labs."

The Cutler-Quate experiment was well received in the science community because of its fundamental nature. After that experiment, the names Cutler and Quate were linked and frequently interchanged. In conference gatherings Cutler was often mistaken as Mr. Quate.

In Murray Hill Cutler lunched frequently with William M. Goodall, who at the time was digitizing prefiltered TV signals using pulse coding modulation up to seven or eight bits per sample. His paper in the *Proceeding of the Institute of Radio Engineers* described the first successful experiment for digitizing TV signals. Because each picture amplitude sample was very much like the preceding one, Cutler thought that if only the difference in signal amplitudes were coded, it would require only a fraction of eight bits per sample; thus, the saving would be substantial. Cutler concluded further that if one quantized the difference between quantized signals, some of the quantizing error would be compensated and one would get a more accurate representation. Based on those ideas Cutler invented differential pulse code modulation (DPCM). Through the years many coding schemes were derived from his pioneering work on DPCM. Today predictive coding is used in digital TV transmission, fax machine, and medical imaging systems. With this background in signal coding and imaging, he extended his work to pulse heterodyne radar, stereoscopic radar, and stereothermography. In 1957 he was invited by Professor John R. Whinnery to spend a semester as the Visiting Mckay Professor at the University of California, Berkeley.

Cutler was promoted to assistant director of electronics research in 1959 and director in 1963. While Cutler was assistant director, Kompfner was the director and Pierce was the executive director.

The advent of the Russian satellite, *Sputnik,* in 1957 generated in Bell Labs a great deal of activity and enthusiasm for rocketry and spacecraft guidance and control. Pierce wrote a classic paper on the potential of Earth satellites for communications. Cutler wrote a technical memorandum, "A Space Vehicle Communication System." With Pierce's approval Cutler organized an ad hoc committee to study the components that would be necessary for a long-life radio repeater in an orbiting satellite. They had frequent meetings that paved the way for the Telstar experiment, which was soon followed by Project Echo.

Early in 1958 NASA was planning to orbit a 100-foot-diameter aluminized Mylar balloon, proposed by William J. O'Sullivan, as a method to measure the density of the atmosphere in near space. NASA was receptive to the idea of performing a passive communication experiment in space using O'Sullivan's balloon for the reflector. Suddenly Project Echo was underway. The experiment required that they set up a transmitter-receiver station at Bell Labs in Crawford Hill, New Jersey, and an identical station at the Jet Propulsion Laboratory Earth Station in Goldstone, California. The balloon would orbit in low altitude with regular passes over North America. Radio signals would be sent from one station into space, reflected by the balloon, and received by the other station. Dozens of people were involved at Bell Labs, at JPL, NASA, and NRL. William C. ("Bill") Jakes was appointed project manager with full authority, and Cutler was an active participant in all of their operations.

By mid-1960 they had a commercial 60-foot-diameter paraboloidal transmitting antenna, a novel 20-foot horn-reflector receiving antenna, and a 10-kW Varian Klystron tube for the transmitter for each ground station. The newly invented maser was used for the first time as the low-noise amplifier.

On August 12, 1960, *Echo 1* was launched into space. They planned to transmit and receive a recording of President Eisenhower's voice during the first pass of the balloon. It was a day filled with excitement. Jakes wrote in his notes for the occasion:

> We were all up in the middle of the night, well before launch time to get everything running and checked out. Of particular concern was the 2390 MHz receiver on the horn antenna. To provide the most sensitivity possible it was equipped with a maser preamplifier which had to be cooled by liquid helium to a few degrees above absolute zero. Liquid helium is tricky stuff, so we were on edge when the helium was transferred to the maser, but all went well. To provide the tracking of the antennas we arranged to get several possible drive tapes keyed to different starting times to allow for any variations from the planned launch. When the word came of the actual launch, we scampered around to get the tape close to that time, and hoped that it would be good. Goldstone acquired the balloon at the right time after launch and we were really excited that the balloon was apparently in the right orbit. Shortly after that, our man in the tracking telescope van beside the Control room excitedly yelped that he saw the balloon not far off track and began to apply directional offsets to bring us in. I checked with Goldstone to see if they were ready to receive from us at 960 MHz. They said yes so I told Cutler to start that tape of Eisenhower's recording.

"I remember starting the tape with my own fingers," Cutler said later in the WPI Journal, "It was probably the most exciting period in my life, because everything had to be done on the second. We had to have that antenna pointed exactly right, because this thing is whizzing from horizon to horizon in just 20 minutes." Goldstone reported back, "It was coming in loud and clear." There were excited cheers from those in the control room. They had succeeded with the first experiment in space communication! After Project Echo and the Telstar experiment, the world was suddenly ready for commercial satellite communications. The federal government created a semipublic corporation, the Communications Satellite Corporation, as the sole owner of this business.

Cutler gloried in physical activity, was a Boy Scout leader, and loved taking his children on adventures, teaching them survival skills and the virtues of the compass. Cutler hiked and skied with many family members and friends. He hiked much of the Appalachian Trail with his childhood friend Gus Blow. He also climbed Mt. Rainer with hiker Milt Boone. His most strenuous and remarkable hike was to the top of the Matterhorn with a Swiss team. Most winters he spent skiing on the slopes of New England, often near the vacation property he acquired in Waterford. In addition, he and Quate took many adventurous trips together.

Quate first met Cutler at a conference in Ithaca, New York, while he was a graduate student. "It was the start of an association that lasted throughout our career," Quate said. "Our friendship was formed with hiking in the summer and skiing in the winter. The ski trips were mostly in New England, the Catskills, Bromley, and the trails of Stowe. The hiking trips were scattered across the country." Quate recalled:

> In Montreal, we walked on Mt. Tremblant. In Maine, we walked about 3 miles to reach Chimney Lake and then climbed to the top of Mt. Katahdin.

To reach the peak at 5200 feet, we had to traverse the Knife Edge, which is a narrow rock ridge, steep and unfriendly on either side. On another trip in California, we planned to hike to the top of Mt. Banner. We started in Agnew Meadows, walked to Lake Ediza and on to a campsite near Shadow Creek. The next day we traversed a snow packed chute leading to the saddle between Ritter and Banner. From there we climbed to the peak of Mt. Banner. The return was far more difficult and took much longer than we had planned. With lightning striking the peaks, the storm was imminent and we realized that we had to stop for the night regardless of terrain. We slept that night on a slab of solid rock in the open fully exposed. That was a night to remember!

Equally memorable were their later trips in the Grand Canyon and in Paria Canyon.

In 1959, while Quate was still at the Labs with Cutler, Quate was tapped as a vice-president of the Sandia Corporation in Albuquerque, New Mexico. Sandia was then run by Western Electric under contract to the U.S. government. In 1961 Quate left Sandia to join the faculty at Stanford University as a professor of applied physics and electrical engineering. In 1975, while Quate and his students were working on the acoustic microscope, he invited Cutler to spend time at Stanford.

The acoustic microscope was a novel device. It operated on the same principles as the optical microscope except that acoustic waves at microwave frequencies were used instead of visible light. The image from the microscope was taken with a single on-axis spherical lens with limited numerical aperture. Cutler wanted to circumvent the limit imposed by the small numerical aperture of the single lens; he suggested a multibeam arrangement with several off-axis lenses distributed over a wide angle. The wavelets emerging from the lenses acted constructively to form a coherent beam with a large numerical aperture according to the Huygens principle. The difference in the images with and without Cutler's arrangement was striking.

Later, at a three-level meeting at Bell Labs that included the vice-president, directors, and department heads, Sol Buchsbaun, then executive director, made a ceremonial speech. He presented Cutler with a framed certificate showing acoustic microscope pictures of onionskin before and after Cutler's innovation. The certificate had been framed and sent by Quate.

Cutler was the director of electronics systems research from 1963 to 1971, and the director of electronics and computer systems research from 1971 to 1978. Over the years hundreds of scientists reported to him. He hung the organization chart upside down in his office to remind himself that those at the bottom of the chart were the important ones. After a successful career lasting more than 40 years, Cutler retired from Bell Labs in 1979 to become a professor of applied physics at Stanford University, where he continued to work on acoustic imaging.

Cutler was a member of Sigma Xi, a fellow of America Association for the Advancement of Science and of the IEEE. He served as the chairman of the IEEE Awards Board (1975-1976) and as the editor of *IEEE Spectrum* (1966-1967). He was awarded an honorary doctor of engineering degree by the Worcester Polytechnic Institute in 1975; he received the Robert H. Goddard Distinguished Alumni Award in 1982. Cutler was elected to the National Academy of Engineering in 1970 and the National Academy of Sciences in 1976. He received the Edison Medal of the IEEE in 1981, the IEEE Centennial Medal in 1984, and the Alexander Graham Bell Medal in 1991.

C. Chapin Cutler passed away on December 1, 2002, in North Reading, Massachusetts, at an age approaching 88. He is survived by his wife, Virginia; son, C. Chapin Cutler, Jr.; daughter, Virginia Raymond; and grandchildren, U. Tyler

Raymond, William C. Raymond, Virginia L. Raymond, and C. Chapin Cutler III.

Those of us who knew Cutler well can still think of the warm moments we shared with him and his family. Kogelnik recalls that soon after he joined Bell Labs, there was a budget crisis and no purchasing orders could be signed. He desperately needed a current-controlled power supply to drive a laser. Cutler, at the time a director with about 100 Ph.D scientists in his group, approached Kogelnik before he could complain, with a large wooden board on which he personally had assembled a current-controlled power supply using old vacuum tubes. The power supply worked well.

Quate remembers one of their hiking trips with Cutler, when they traveled to the Colorado Rockies to climb Mt. Alice. It proved to be a trip 18 miles in length with a gain in elevation of 4,810 feet. Their evening meal of chicken soup was prepared by a wonderful lady in the next campsite. After the meal Quate tried to stand and discovered that his legs and thighs were gripped with severe cramps. He could not move. Cutler stood behind him, lifted him up on his feet, and said, “Now walk, walking will cure you.” Cutler was right.

My wife, Nancy, and I were graduate students at Stanford University. We were married in 1952. Shortly after, we moved to New Jersey and I joined Bell Labs. It was a difficult transition for us, from the carefree university environment to the reality of a corporate life in a country that was still foreign to us. Cutler and Virginia were mentors to our family, providing warm, parenthood care. Knowing me well, Cutler said, “When PK is frustrated, he speaks Chinese.” One year we had to go back to Hong Kong for a visit. Virginia took care of our two daughters, Emily and Julia, when Julia was only a few months old. Virginia pulled out a large drawer from the chest and emptied it to make a bed for Julia.

As I finish this memoir I recall and admire his passion to discover and his unbounded energy for work. He will always be a role model cherished by all of us who work in science and engineering.

I WAS ASKED by Professor Andreas Acrivos to write this memoir for the National Academy of Sciences and was overwhelmed by the help I received. Most of the materials were collected from Cutler's personal notes supplied to me by the family. Several pieces were written by his close associates, Quate, Jakes, and Kogelnik. Searching through his old records, Professor John R. Whinnery found a nine-page text written by Cutler on his early experiences at the labs. Kogelnik retrieved a collection of e-mail messages between Cutler and Nick Sauer discussing the lab at Deal. Roger N. Perry, Jr., provided the information about Worcester Polytechnic Institute back in the 1930s and 1940s. Professor Bruce Wooley made Cutler's files at Stanford University available. Gary Boyd and Susan Feyerabend helped to locate Cutler's old papers left in the lab. I have also obtained files from the Lucent Archives and the IEEE History Center. My daughter edited the English of the first draft. Quate edited the final version of the text. I want to thank in particular Patricia A Tier, who assisted me in every phase of this project.

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John R. Johnson

JOHN RAVEN JOHNSON

August 9, 1900–May 25, 1983

BY CHARLES F. WILCOX, JERROLD MEINWALD, AND KEITH R. JOHNSON

JOHN RAVEN JOHNSON WAS born in Chicago, Illinois, on August 9, 1900. He developed into a precocious science student and received his B.S. degree at the University of Illinois in 1919. He stayed on at Illinois to work with Roger Adams, under whose direction he earned his M.S. in 1920 and his Ph.D. in organic chemistry in 1922. His Ph.D. thesis work was concerned with the synthesis of pharmacologically active arsonic acid derivatives for treatment of trypanosomiasis, which resulted in two publications and a U.S. patent (1921, 1923, 1925). He spent two years abroad doing postdoctoral research in the laboratory of Charles Moreau at the Collège de France in Paris under a prestigious American Field Service Fellowship. There he developed a lifelong love of France and French wines. His research with C. Moreau and C. Dufraisse on the bromination of furylacrylic acid and its conversion to furyl acetylene was published (1923, 1927) along with other work on the reactions of furan derivatives (1928, 1929).

He returned to the University of Illinois, spent three further years there as an instructor, and made several contributions (1925, 1926, 1927) to the *Organic Syntheses* series introduced by R. Adams. At the time, chemical supply houses did not exist as they do now and the development of practical syntheses of organic starting materials on a large scale was extremely valuable to the organic chemistry community. Johnson had a passion for organic laboratory work, and he coauthored with his teacher and friend, Roger Adams, a widely used laboratory textbook on organic chemistry (1928). That book in a metamorphosed form continues to be used today. He also used his time as an instructor to continue his work, started in Paris, on furan chemistry.

In 1926 Johnson was invited to give a lecture in Cornell's Department of Chemistry. It was so well received by both the students and the faculty that he was offered a job as an assistant professor, which he accepted. Although only 27 years old when he started teaching at Cornell, he had already developed a reputation as one of the nation's brilliant young chemists. He brought to Cornell the new organic chemistry for which Illinois had become famous. He quickly put into place a lively program of research and attracted large numbers of graduate students. He also restructured Cornell's courses in organic chemistry and developed a reputation as a superb teacher. At Cornell he continued his studies of furan reactions (1930). He also collaborated with Professor A. W. Browne on the photochemistry of alkyl and acyl derivatives of azido-dithiocarbonic acid (1930).

Jack (as he was known to his friends) Johnson was a meticulous lecturer. His lectures were prepared on 5 × 8 note cards that he called "board cards." Not only did he know what he was going to say but he knew exactly where he was going to write each structure and equation on the board. This was difficult for his later junior colleagues whom he would, on rare occasions, ask to lecture for him. He would insist that they show him their own board cards before he would sign off on their lecturing.

In 1930 Johnson, even though he was only 30 years old, was promoted to full professor. His publications from this period show continued activity in furan chemistry, medicinal chemistry, and what was somewhat unusual among organic chemists of the time, a budding concern about electronic properties and mechanisms of reaction. In 1931 he initiated research programs in boron chemistry and organometallic chemistry, which continued for many years.

The 1930s were not kind to Cornell or Johnson. A department brochure for 1933 shows that Johnson was the only faculty member of professorial rank teaching the organic lecture courses. His predecessor, Professor W. R. Orndorff, who had been at Cornell since 1887, had died. He had two instructors to run the laboratories and to teach one of the graduate courses, but he had to lecture in three courses for each semester.

Johnson had a long, professional connection as chemical consultant with the Du Pont Company, where his old Illinois friend W. H. Carothers was the research director of the newly established Central Research Experimental Station. Carothers, who was credited with the development of Nylon for Du Pont, had a nervous breakdown in 1937 and fled to somewhere in France to escape the pressure. Based on his familiarity with both France and Carothers, Johnson was engaged to go to France, find Carothers, and bring him home. This he did. Unfortunately, Carothers, despite treatment that appeared to be working, a little while later committed suicide.

In 1937 there was a chance encounter that had lasting consequences. At that time the 20-year-old R. B. Woodward had obtained his Ph.D. from the Massachusetts Institute of Technology and was teaching a summer course at the University of Illinois. Johnson, then 37 years of age, apparently met and befriended Woodward there; at least in later years he spoke of counseling the young instructor Woodward. However it happened, Woodward and Johnson developed a mutual respect that resulted in important later collaborations.

In 1938 Johnson wrote a 116-page chapter in Gilman's influential *Organic Chemistry, An Advanced Treatise* entitled "Modern Electronic Concepts of Valence." This major review apparently was inspired by Sidgwick's Baker Lecture series at Cornell in 1933 on "The Electronic Theory of Valency," but it went well beyond Sidgwick by including Robinson's and Ingold's discussions of reaction mechanisms, as well as Pauling's treatment of resonance. For many organic chemists of the time this chapter served as a roadmap to the newly emerging field of physical organic chemistry.

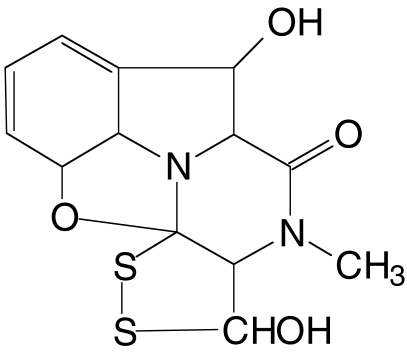
In 1938 Johnson and Van Campen discovered the nearly quantitative oxidation of organoboranes to alcohols by alkaline hydrogen peroxide. Although this new reaction was only an incidental part of Johnson's borane program, 20 years later H. C. Brown and S. Rao capitalized on it and employed it as an essential facet in their powerful development of hydroboration chemistry.

During the period 1941-1945 Johnson became deeply involved in the scientific aspects of several U.S. wartime research efforts. He was an early participant in studies on the synthesis of new chemical explosives and contributed to the vigorous U.S. search for new antimalarial agents. He also took part in the Alsos mission, a section of the Manhattan Project charged with trying to discover what progress, if any, Germany was making toward an atomic bomb.

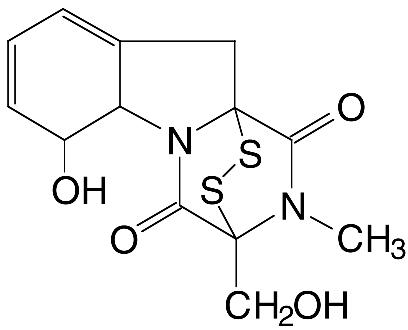
In 1942 and 1943 he served in London, England, as the scientific officer for chemistry in the U.S. Office of Scientific Research and Development. One of the issues he dealt with concerned the new high explosive RDX (cyclonite) that the United States had developed. RDX has a nasty habit of crystallizing in different polymorphic forms, at least one of which is extremely shock sensitive, making the explosive unusable. Johnson, in collaboration with his Cornell colleagues A. T. Blomquist and W. C. McCrone, Jr., developed new crystallization solvents and procedures that eliminated the undesired polymorphs and made RDX an explosive of choice.

He participated in the Alsos mission in Italy in 1943 and 1944, following the Allied advance closely in order to interview Italian scientists who might know something of German nuclear research. Fortunately, the results were negative. He enjoyed telling of how he was accompanied near the battlefront by Lt. Col. Boris Pash, the chief of Alsos, who packed a .45 pistol. When Johnson observed that a .45 would be little use against an enemy attack, Pash said that wasn't the point: He had orders to shoot Johnson if necessary to make sure he did not fall into enemy hands. Johnson received the U.S. Medal of Merit for his wartime service, and in 1945 he was elected to the National Academy of Sciences.

Jack Johnson returned to Cornell after the war and resumed his career of supervising the research of graduate students, teaching large organic chemistry classes, and consulting with the Du Pont Company. In 1943 Johnson had published his first paper on the structure of the powerful antibiotic gliotoxin. In spite of the wartime hardships this work continued through a dozen publications until at last with the assistance of R. B. Woodward a final correct structure was published in 1958. Johnson was a classically trained organic chemist. The only instrument available to him for the bulk of the gliotoxin studies was an ultraviolet spectrometer that required separate point measurements at each desired wavelength. In spite of not having nuclear-magnetic-resonance spectroscopic evidence, he and his students managed to correctly identify all the structural pieces and most of the connectivities. It fell to his friend Woodward to put them together in the correct fashion. Woodward in later years credited Johnson as one of the pioneers in using physical methods for structure determination.



Johnson Structure



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During World War II the chemistry and production of penicillin was treated as a war secret. The work of more than 20 U.S. industrial and academic research groups was coordinated with the similarly large British effort through the U.S. Office of Scientific Research and Development. In 1946 the massive joint effort was declassified and in 1949 Johnson collaborated with H. T. Clarke and R. Robinson to write and edit the monograph *Chemistry of Penicillin*, which described the knowledge about penicillin developed during the war.

Perhaps all chemists at some point in their lives dream that their research might help save a human life. Few get to realize this dream, and fewer yet get to see it applied in such a personal way as did Jack Johnson. In the summer of 1945, just after the war in Europe had ended, Jack's 10-year-old son had to have an emergency appendectomy, which was successful but led to a serious case of peritonitis, a commonly fatal condition at the time. Johnson was able to arrange for penicillin treatment and after a month of regular penicillin injections, his son made a full recovery.

During the spring semester of 1951 Johnson served in West Germany as special consultant on scientific matters for the U.S. Department of State. In 1952 he became the Todd Professor of Chemistry at Cornell, occupying the only named chair then available to the Cornell Department of Chemistry. Johnson's research continued to bridge the old and the new in organic chemistry. He and his students gave much effort to devising syntheses and determining structures of important molecules in the best tradition of organic chemistry. In 1963 he was joined in the authorship of the venerable Adams and Johnson *Laboratory Experiments in Organic Chemistry* by his Cornell colleague C. F. Wilcox.

In 1965 Johnson retired from Cornell and he and Hope, his wife of 36 years, moved permanently to their beloved farm in Townshend, Vermont. For several years after leaving Ithaca he spent six months at a time in Wilmington as a consultant in residence at the Du Pont Experimental Station. Although he was cut off from formal research, he also kept busy with one of his original loves, organic laboratory instruction. Johnson drew on his encyclopedic knowledge of organic chemistry and his treasured personal copy of Beilstein to suggest interesting new experiments that were then perfected at a practical level back at Cornell. The sixth and seventh editions of the laboratory manual were developed in this long-distance fashion. The seventh edition appeared in 1979, 51 years after the first: an extraordinary run for a manual in any field of science.

Johnson had been a heavy smoker for most of his life, and he eventually developed emphysema. His life became increasingly restricted, but he remained engaging and intellectually lively up to his death in May 1983. He was survived by his wife; two sons, Keith and Leonard; and three grandchildren. Jack Johnson's Cornell colleagues and his many students and other friends remember him with admiration and affection as one of the important players in the development of Cornell into a great research university.

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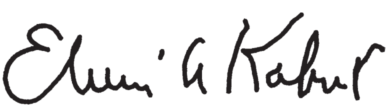

ELVIN A. KABAT

September 1, 1914–June 16, 2000

BY ROSE G. MAGE AND TEN FEIZI

ELVIN A. KABAT, WHO died on June 16, 2000, was a founding father of modern quantitative immunochemistry together with Michael Heidelberger, his doctoral mentor. During his long career the structural and genetic basis for specificity of antibodies was elucidated. It was he who first demonstrated that antibodies are gamma globulins. Although his name is most associated with characterizations of the size and heterogeneity of antibody-combining sites, his contributions to modern biomedicine go well beyond this subject. His work advanced our understanding of fundaments of developmental biology, inflammation, autoimmunity, and blood transfusion medicine. Elucidation of structures of the major blood group antigens, embryonic-stage-specific carbohydrate antigens, and functional carbohydrate markers of leukocyte subsets were either achieved by him and his associates, or made possible through meticulously characterized, invaluable compounds he generously made available to other investigators.

His more than 470 publications span a period of 65 years. Over several decades he was a leading figure in several parallel fields of investigation as is evidenced by the books he authored: *Blood Group Substances—Their Chem-*

istry and Immunochemistry (Kabat, 1956), *Kabat and Mayer's Experimental Immunochemistry* (Kabat, 1961), *Structural Concepts in Immunology and Immunochemistry* (Kabat, 1976), and the series of five editions of *Sequences of Proteins of Immunological Interest*. The most recent edition of *Sequences* appeared in 1991 (Kabat et al., 1991), after which a website was established. The printed and subsequent Web version was a pioneering effort that preceded the current GenBank database. Indeed, Kabat was also instrumental in urging the National Institutes of Health to support a national DNA sequence database and the development of sequence manipulation software (Lewin, 1982).

When one of us (R.G.M.) was in the Kabat laboratory as a graduate student (1957-1962), it was located in the Columbia Presbyterian Hospital's Neurological Institute. This came about because Kabat was hired by Columbia University to conduct immunochemical studies of neurological diseases, including the human autoimmune disease multiple sclerosis. He made seminal contributions to the development of an animal model of multiple sclerosis (Kabat et al., 1946, 1947, 1948) and of a diagnostic test based upon elevated levels of gamma globulin he found in cerebrospinal fluid (1948). The immunochemical measurements of patients' gamma globulin levels were done in the Kabat laboratory for 30 years. The experimental autoimmune (or allergic) encephalomyelitis (EAE) model is still widely used today.

Both of Kabat's parents arrived in the United States from Eastern Europe toward the end of the nineteenth century. Elvin was born on September 1, 1914. His father and two uncles had changed their last name from Kabatchnick to Kabat in 1908, possibly because they had a dress manufacturing business that they named Kabat Bros. The business prospered until 1927, when changing economic conditions led to bankruptcy and a period of difficult times for the family. Those of us who worked in Elvin's laboratory were made aware of his experience during the difficult times that extended through the Great Depression. Elvin was always very careful with laboratory expenditures. Rose Lieberman, a long-term member of the Laboratory of Immunology at the National Institute of Allergy and Infectious Diseases and fellow graduate student of Kabat at Columbia, used to tell one of us (R.G.M.) that she and Elvin shared one can of soup for lunch.

Elvin entered high school at the age of 12 and completed it in three years, thus he entered the City College of New York at 15, graduated with a major in chemistry in 1932 at the age of 18, and in January 1933 was already working in Michael Heidelberger's laboratory doing routine laboratory tasks at Columbia University's College of Physicians and Surgeons. The opportunity to work in the Heidelberger laboratory came through Heidelberger's wife, Nina Heidelberger, a customer of Kabat's mother who was selling dresses to help make ends meet. Once Heidelberger had agreed to give him a job, Kabat was able to start work on his Ph.D. in the Department of Biochemistry, taking most courses at night. Although originally hired to assist with routine laboratory maintenance, he was soon working on quantitative agglutination (1936) and precipitin (1937) reactions and completed his Ph.D. in only four years. A major conclusion emerging from this work was that the same antibody molecules could agglutinate particulate antigens, such as pneumococci bearing capsular polysaccharide and precipitate soluble pneumococcal polysaccharide (1936).

From his own statements in autobiographical memoirs (Kabat, 1983, 1988), it is quite clear that he wanted to go to medical school, but this was not possible without scholarship support. His career path brought him back to the medical schools he applied to; he was briefly on the faculty of Cornell (1938-1941) and then for the majority of his career at Columbia, where his impact on the progress of biomedical research was probably far greater than it would have been if he had practiced clinical medicine himself. He taught and strongly influenced generations of medical students, graduate students, and postdoctoral fellows. Thus, in addition to his own contributions to advances in basic immunology and clinical medicine, he helped to prepare many future leaders in clinical research as well as in basic fields as diverse as glycobiology, immunogenetics, and bioinformatics.

Michael Heidelberger suggested that Kabat do postdoctoral training with Arne Tiselius and Kai Pederson in the Svedberg laboratory in Uppsala, Sweden, where the new methods of ultracentrifugation and electrophoresis were being developed. Elvin received a postdoctoral fellowship sponsored by the Rockefeller Foundation. Antisera against pneumococci, purified pneumococcal carbohydrate antigens, and some purified antibody fractions (1938) from various species, including horses, pigs, rabbits, a cow, and a monkey, were prepared and shipped ahead to Uppsala. There, working with Pedersen, Kabat found by ultracentrifugation studies the 18 or 19S antibody (now known as IgM) in horses early after immunization, and the heterogeneity of molecular weights of antibodies from hyperimmunized horses and other species (Kabat and Pederson, 1938; Kabat, 1939). Working with Tiselius (1939), he conducted the groundbreaking electrophoresis experiments that first demonstrated that anti-ovalbumin antibodies in sera of hyperimmunized rabbits were gamma globulins (IgG). Before returning to New York to take up a position in the Pathology Department at Cornell University Medical College, Kabat started what was to be a pattern throughout his life: establishing friends and contacts in laboratories around the world. Not yet 24 and just completing a one-year postdoctoral fellowship, he managed to visit many leading lights in biochemistry at that time, including Linderstrom-Lang in Copenhagen, Hans Krebs in Sheffield, and numerous others in laboratories in London, Cambridge, Birmingham, Leyden, and Amsterdam (listed in his 1983 autobiography [Kabat, 1983, p. 8]).

In 1942 Elvin married Sally Lennick, a young and talented art student from Canada. Sally made those of us who worked with Elvin feel more like family, which helped to counterbalance the demanding standards and pace of life in the Kabat laboratory. She sketched beautiful portraits of their young sons and once Jonathan, Geoffrey, and David were older she traveled extensively with Elvin, sometimes sketching local scenes or portraits of cooperative participants at scientific meetings.

Kabat's work on the structures of the blood group antigens began in 1945. It is interesting to recall the background to his entry into this field, where his contributions have been spectacular. As Kenneth O. Lloyd and one of us (T.F.) wrote in Kabat's obituary published in the *Glyconjugate Journal* (2000) and *Glycobiology* (2001), he was working with Michael Heidelberger on quantitative immunochemistry of bacterial polysaccharides, when he read a paper published by Karl Landsteiner and Merrill Chase in 1936 in the *Journal of Experimental Medicine* describing the presence of blood group A substance in commercial pepsin. Kabat was stimulated by this report, knowing how little antigenic material can be obtained from red cells. He suggested to Heidelberger that they might "do some quantitative precipitin tests" using this soluble material and human anti-A sera. His mentor's response was that this was a good problem for him to pursue as an independent investigator. Thus, Kabat's interest in the blood group antigens remained latent, only to be rekindled by a proposal by Ernest Witebsky and colleagues during World War II that soluble A and B substances from hog and horse stomachs might be added to group O blood to neutralize the anti-A and anti-B to make it a better universal donor blood. Some of the preparations were not meeting specifications in that they induced anaphylactic shock in guinea pigs, and Witebsky suggested to Kabat that he look into the question. Kabat applied for a contract from the Office of Scientific Research and Development, which he received very late in the war, in 1945, shortly before V-E day.

By the time Elvin Kabat's studies on the blood group antigens were launched, there had been some important developments. Walter Morgan and colleagues in the United Kingdom had shown that large amounts of blood group substances occur in human ovarian cyst fluids, and they had developed methods for their isolation. Ernest Witebsky and colleagues had succeeded in producing high titer anti-A and anti-B sera by immunizing volunteers with hog A and horse B substances isolated from gastric epithelia. Thus, the scene was set for quantitative precipitin assays of the blood group antigens. These assays, coupled with hemagglutination assays, served initially to monitor the purification and characterization of the blood group substances, and their partially hydrolyzed forms. Later on they were important in identifying the oligosaccharides that contained the blood group determinants in the following two decades; the biochemical basis of the major blood group specificities was elucidated by Kabat's group in parallel with Walter Morgan and Winifred Watkins at the Lister Institute in London.

In the first phase of this pioneering immunochemical work with blood group-active polysaccharides (mucins) of human and animal origins, it emerged that there were some similarities in these highly complex substances, and also differences between those of differing A, B, and O (H) types. By mild acid hydrolysis, immunoreactivities were revealed with horse antibodies raised to pneumococcus type XIV polysaccharide. This indicated the presence of common sequences in the backbone regions of their oligosaccharide chains. The other major conclusion was that the "structural groupings" associated with each of the three blood group types and the type XIV cross reactivity were distinct. By 1966 the chemical structures of these major blood group antigens, as well as the blood group Lewis^a (Le^a) and Le^b, had been determined, and a composite structure for the carbohydrate chains on the epithelial mucins that bear the various blood group determinants had been proposed; the genes (coding for glycosyltransferases) involved in the biosynthesis of the blood group antigens were anticipated (Lloyd and Kabat, 1968).

The significance of this work extends beyond our current understanding of the biochemical basis of the major blood group antigens. This work opened the way to the elucidation of several blood-group-related carbohydrate antigens, later to be referred to as carbohydrate differentiation antigens, whose expressions change sequentially from the earliest stages of embryogenesis right through differentiation events in adulthood, and also in oncogenesis. Among these are the I and i antigens expressed on specific parts of the backbones of this family of oligosaccharides, and the Le^x and Le^y antigens expressed as capping structures (Feizi, 1985). The latter sequences were designated new gene products by Lloyd and Kabat (1968) when first discovered on the epithelial mucins. The foundations had been laid for understanding the roles of members of this family of carbohydrate antigens as ligands for carbohydrate-binding receptors. Notable examples are the roles of the Le^x-related and

Le^a-related oligosaccharides on glycoproteins and glycolipids as recognition elements for the leukocyte-endothelium adhesion molecules, selectins, which play a crucial role at the initial stage of leukocyte recruitment in inflammation (Feizi, 2000).

In a notarized document dated August 1, 1996, entitled "The Care and Maintenance of the Elvin A. Kabat Collections of Purified Carbohydrates and Other Materials at Columbia University" Kabat appointed his last graduate student, Denong Wang, to be the curator of research materials, including in addition to the carbohydrates, "antibodies, cell lines and other related materials." In the second paragraph of this document he states, "The collection of polysaccharides has been in the possession of Professor Elvin A. Kabat and used by his graduate students and collaborators since 1932 when Dr. Kabat was working and collaborating with Professor Michael Heidelberger. Some of the materials were prepared in Prof. Karl Landsteiner's laboratory while he was in Austria and later at the Rockefeller Institute for Medical Research." Those of us who worked in Kabat's laboratory remember well that some of the most valuable of these materials were kept locked in a safe. It is likely that the safe dates back to the days of World War II, when Kabat worked on several projects related to the war effort to improve methods of immunization and protection against potential biowarfare agents. The research was sponsored by the Office of Scientific Research and Development (OSRD) and the National Defense Research Committee (NDRC).

In addition to immunochemical studies of meningococcal meningitis that included immunization of medical student volunteers with meningococcal polysaccharide (Kabat et al., 1947), Kabat lists himself as "chemist" on an NDRC project. This supported the studies of ricin by Kabat and Heidelberger. A paper entitled "A Study of the Purification and Properties of Ricin" by Kabat, Heidelberger, and Bezer (1947) presents a summary of "a portion of a study carried out during 1943-45 in consultation and collaboration with other laboratories engaged in parallel investigations." The fears of its use for biowarfare (by the Nazis during World War II) are sadly still with us 60 years later in the form of the current anxiety about bioterrorism. Theodor Rosebury and Kabat also prepared a classified report on potential bacterial and viral biowarfare agents. This was declassified and published in the *Journal of Immunology* after the war (Rosebury and Kabat, 1947).

Kabat was involved in additional national and international endeavors after World War II. He served on several different advisory panels for the National Research Council, the Office of Naval Research, and the National Science Foundation, as well as for private foundations, including the National Multiple Sclerosis Society, American Foundation for Allergic Diseases, New York Blood Center, Roche Institute of Molecular Biology, Institute of Cancer Research, and Gorgas Memorial Laboratory, Panama. He was a member of the World Health Organization Advisory Panel on Immunology from 1965 to 1989. A succinct article by Kabat (1986) entitled "A Tradition of International Cooperation in Immunology" describes the World Health Organization's efforts starting in 1963 to establish research and training centers in immunology in developing countries. Kabat traveled several times to Africa, helped to select and establish the first such center in Ibadan, Nigeria, and helped to monitor its progress. His own longtime technician Ada Bezer later worked for the WHO and assisted in the running of the Ibadan laboratory and in the training of students and researchers who came to work there.

His interest in developing quantitative methods to study allergic reactions (Benacerraf and Kabat, 1949, 1950) led to an invitation to serve on the Subcommittee on Shock of the National Research Council. Dextran had been developed in Sweden for use as a plasma expander but administration had led to some severe allergic reactions. In a series of experiments Kabat proved that the allergic reactions were due to immune response to the dextran itself rather than to contaminants (1953). Most dramatically he immunized himself and then performed a skin test on himself (which proved positive) and on a control subject (negative) at a subcommittee meeting. This discovery of the antigenicity of dextran was the start of a long series of studies of the size and heterogeneity of the antibody-combining site using the very simple and well-defined dextrans and oligosaccharides of defined length as inhibitors of the precipitin reaction. Antisera were raised in medical student volunteers (and graduate students, including R.G.M.). Studies of the inhibition of the precipitin reaction between linear α-(1$\rightarrow$6) linked dextran and human antidextran antibodies by members of the isomaltose series of oligosaccharides between 2 and 7 monosaccharide units in length demonstrated that the upper limit of the combining site size was equivalent to a hexasaccharide (isomaltohexaose) (1957, 1960). This was later extended to rabbit antibodies raised against whole dextran-bearing *Leuconostoc mesenteroides* (Mage and Kabat, 1963a).

Before the advent of hybridoma-derived monoclonal antibodies, human antidextran antibodies were fractionated by differential elution with oligosaccharides of different length (Schlossman and Kabat, 1962) and later by differential precipitation of antibodies elicited to branched dextrans; first precipitating antibodies specific for α-(1$\rightarrow$6); and then for α-(1$\rightarrow$2) or (1$\rightarrow$3)-linked glucose residues (Dorner et al., 1967). A classic paper in the series on studies of human antibodies with Henry G. Kunkel's laboratory (1968) described heavy chain subgroups (isotypes), light chain types, and Gm allotypes found in fractionated human antibodies, including antibodies from subject 1 (Elvin Kabat) to dextran, levan, diphtheria toxoid, and tetanus. The studies suggested that some of the fractionated samples were of somewhat limited heterogeneity. The sharing of the Louisa Gross Horwitz prize for outstanding basic research in the fields of biology or biochemistry by Heidelberger, Kunkel, and Kabat in 1977 may in part have been because of this collaborative work.

In further studies of fractionated and purified antibodies from subject 1 (Kabat again) (1975), isoelectric focusing studies confirmed that fractionated antidextrans, as well as antilevan, and anti-blood-group A, could be quite restricted or possibly monoclonal. The most important finding in this paper, from quantitative inhibition studies of both the fractionated antidextrans and mouse myeloma proteins with specificity for dextran, was that antibodies could react at nonterminal locations along a linear α-(1$\rightarrow$6)-linked dextran chain. Kabat referred to these as "groove type" combining sites and in lectures would sometimes say that the polysaccharide chain was bound to the antibody "like a hot dog in a roll." Other binding sites that recognized only terminal nonreducing ends were referred to as "cavity-type sites." When hybridoma technology became available, Kabat's laboratory conducted further detailed analyses of monoclonal antibodies of defined specificity to both linear and branched dextrans (1981). These studies continued through the next decade (Wang et al., 1991).

The finding of groove-type antidextran combining sites also helped to explain earlier studies of precipitating antibodies to type III pneumococcal polysaccharide (SIII), a largely linear polymer of the disaccharide cellobiuronic acid (Mage and Kabat, 1963b). Before embarking on these studies, one of us (R.G.M.) had to generate precipitin curves using as antigen an acid hydrolyzed fraction of SIII that had been prepared by Michael Heidelberger and a horse anti-SIII, produced before I was born. After I generated a set of precipitin curves and showed them to Elvin, he produced the curve done by Heidelberger in 1935. The curves generated with the same materials more than 20 years later, superimposed. I learned the evening after I told this story at the memorial symposium held for Elvin at Columbia in November 2001 (<http://www.columbia.edu/~dw8/kabat>) that a student who worked with Elvin many years later was given antidextran antibodies that I had studied and was required to prove that he could reproduce the precipitin curves I had generated.

As a complement to his studies on anticarbohydrate antibodies, Kabat was interested in the specificities of plant and animal lectins, which also recognize sugar units. Other investigators were already engaged in studies on the specificities of lectins. Kabat was able to use his extensive collection of polysaccharides and oligosaccharides to analyze their ligands in greater detail. These studies often led to new insights into the specificities of the lectins and opened the way for their use as specific reagents for immunochemical and immunohistological studies. Among the lectins studied were those from *Helix pomatia, Dolichos biflorus, Griffonia simplicifolia*, several marine sponges, and the chicken hepatic lectin (the last with Gilbert Ashwell of the National Institutes of Health).

In 1974 the Kabats arrived to spend a sabbatical year at the NIH. Elvin had received the prestigious appointment as an NIH Fogarty scholar. He quickly became an important part of the NIH immunology community and developed many friendships, including with David Davies and Eduardo Padlan. Eduardo built a model based on a protein sequence of purified monoclonal rabbit anti-SIII published by J.-C. Jaton and the solved crystal structure of mouse myeloma protein McPC603 from the Davies laboratory. In this model a hexasaccharide neatly fits across the combining surface of the antibody. This gave us all great pleasure for, as with dextran, we had concluded that the combining sites of antibodies to this more complex polymer had an upper size limit of a hexasaccharide (Mage and Kabat, 1963b). Although a picture of the model was never published in a formal paper, it appeared on the cover of *P&S, The Journal of the College of Physicians and Surgeons of Columbia University* (vol. 5, no. 3, 1986).

A most important result of the one-year sabbatical was that at its end Elvin remained a member of the NIH community for the rest of his career, until failing health interfered with his ability to travel. He became an expert consultant for the National Cancer Institute, later for NIAID and the Office of the Director, and on most weeks spent Saturday afternoons through Tuesdays at NIH and the remainder of the week at Columbia. This came about because Kabat had been compiling antibody variable region sequences with Tai Te Wu and seeking to understand which portions of the sequences contributed residues that made contact with antigens. William Raub of the NIH Division of Research Resources brought the PROPHET computer system to his attention and introduced Kabat to Howard Bilofsky of Bolt, Beranek, and Newman. This was the start of the database referred to earlier that was eventually published in five printed editions starting in 1976, when only amino acid sequences of variable regions were included, and extending through a three-volume fifth edition. It was now entitled "Sequences of Immunological Interest," because it included sequences of variable and constant regions of antibody heavy and light chains, including codons of those amino acid sequences that had been deduced mainly from cDNA clones. In addition, the database now included amino acid sequences and corresponding codons of a variety of other genes of the immune system, including T-cell receptors and transplantation antigens.

Although the Kabat database of proteins of immunological interest is no longer supported by government funds, it is currently available at <http://kabatdatabase.com> due to efforts of George Johnson. It is uncertain how long it will remain available; it is maintained with funds from subscribers, and current funds barely offset the maintenance costs. Andrew Martin has a valuable searchable "Simple Interface to the Kabat Sequence Database," called KabatMan at <http://www.bioinf.org.uk/abs/simkab.html>. This includes only immunoglobulin sequences that have at least 75 residues, so the database contains essentially only complete light or heavy chain sequences. The database version is that of July 12, 2000. It contains 6,014 light chain and 7,895 heavy chain sequences of which 2,140 form complete antibodies.

As early as 1967 Kabat was publishing analyses of variable regions of human and mouse Bence-Jones proteins (immunoglobulin light chains) (Kabat, 1967). After he enlisted the help of T. T. Wu, who had a background in mathematics, computers, and biophysics, they rapidly completed an extensive compilation of available light chain sequence information and published in the *Journal of Experimental Medicine* the first variability plot in a landmark 40-page paper (1970) that defined variability as the ratio of the number of different amino acids at a given position to the frequency of the most common amino acid at that position. The paper suggested that hypervariable regions within antibody variable region sequences would contribute to antibody complementarity. This has been amply confirmed, and now these regions are referred to as complementarity determining regions (CDR). In his 1988 autobiography (Kabat, 1988, pp. 13-16) he generously credits all the previous investigators whose studies contributed to the initial ideas and data that were crystallized in the 1970 paper and also describes the further refinements that came as further analyses and sequence data became available. Kabat had thought of submitting this paper to the *Journal of Theoretical Biology*. When he happened to mention this to Henry Kunkel, there was no doubt in the mind of the then editor in chief of the *Journal of Experimental Medicine* that the paper should be submitted to this journal. It was the privilege of one of us (T.F.), who was working as a guest investigator simultaneously in the Kabat and the Kunkel laboratories, to carry the submission from Columbia Medical Center to Rockefeller University. It is an interesting experience to revisit this paper more than 30 years after it was published and realize that it was written before immunoglobulin class switching and VDJ recombination had been discovered and before any maps or sequences of immunoglobulin germline genes were available. It already hypothesized that there might be some "episome"-like introduction of information into variable region gene sequences. Nine years later (1979) a formal paper supporting the idea of minigenes was published. Although the minigene hypothesis in its original form was not correct, the discovery of VDJ recombination revealed that the J genes accounted for the minigene-like behavior of the fourth framework region of variable regions. A 1980 paper (Kabat et al., 1980) provided evidence indicating independent assortment of framework and complementarity-determining segments of the variable regions of rabbit light chains. Now, from examination of rabbit germline Vκ sequences it appears that what they observed was due both to gene conversions that occurred during evolution of the multiple Vκ genes and to gene conversion-like changes that accompany somatic expansion and diversification of rearranged VκJκ sequences in rabbit splenic germinal centers (Sehgal et al., 2000).

Elvin Kabat was elected to the National Academy of Sciences in 1966, the same year that he served as president of the American Association of Immunologists. He received many prizes, honors, honorary degrees, and invitations to present named lectures. Perhaps most important to him was the award of the National Medal of Science in 1991. As William Paul and one of us (R.G.M.) wrote in his obituary published in *Nature* (2000),

> He valued this honor greatly, particularly because of the difficulties he had in the 1950s when the National Institutes of Health cravenly terminated his grants as fallout of the politics of the McCarthy era. Fortunately, the Office of Naval Research and National Science Foundation continued to support him. Kabat regarded the Medal of Science as recognition of a career-long record of accomplishment, and as a personal vindication.

Less well recognized in scientific circles were Kabat's strong beliefs in justice and the defense of what was right whether politically correct or not.

Throughout his career at Columbia, Kabat had three or four parallel lines of investigation ongoing at any given time. He was a chemist by training, a pioneer in the field of protein chemistry, and one of the founding fathers of immunochemistry. His contributions to understanding the nature of the antibody combining site and of antibodies that cross react with different carbohydrate antigens (1942), carbohydrates and DNA (1985, 1986), or DNA and peptide mimics are highly relevant today, for example, as we seek better understanding of the role of infectious organisms in initiating and exacerbating autoimmune diseases. It is remarkable that he also became a major figure in the fields of glycobiology and database development. The Wu-Kabat variability plot is still used to analyze sequence data not only of immunoglobulins but also of T-cell receptors and even of rapidly evolving viral sequences. As Donald Marcus and Stuart Schlossman wrote in the 2001 Kabat obituary in the *Journal of Immunology,* "His passion for science, integrity and high standards made him a demanding taskmaster, and his critiques of experimental data could be unsparing. His former trainees enjoyed getting together at international meetings to reminisce about their experiences in his laboratory and what it meant to be 'Kabatized'." According to Schlossman and Nobel Laureate Baruj Benacerraf, "To be 'Kabatized' and survive meant you could do well anywhere. . . . Kabat's wonderful sense of humor and his talent as a raconteur leavened the serious atmosphere of the laboratory. Scientists trained in his laboratory carried with them a model of how science should be performed, and his trainees maintained enduring personal and professional relationships with him."

IN PREPARING THIS memoir we were greatly assisted by Kabat's own detailed autobiographies (Kabat, 1983, 1988), as well as by memories of our close association with him during our training in his laboratory. In addition, one of us (R.G.M.) remained in close contact with him throughout his tenure as a consultant to NIH. We thank Denong Wang for providing a copy of the notarized document appointing him curator of Kabat's research materials. We also gratefully acknowledge critical suggestions and comments from family members and from colleagues, including Gilbert Ashwell, George Johnson, Ken Lloyd, Nancy McCartney-Francis, Mike Mage, David Margulies, Donald Marcus, Barbara Newman, William Paul, and Tai Te Wu, as well as permissions from the *Glycoconjugate Journal* to publish excerpts from the obituary of Elvin Kabat and from the *Annual Review of Immunology* to republish the photograph of Elvin Kabat that appeared in part I of his autobiography (1983). It is reprinted with permission from the *Annual Review of Immunology,* Volume 1 ©1983 by Annual Reviews. We thank Shirley Starnes for expert editorial assistance.

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Photo by Donald Gessling, Huntington

Berwind P. Kaufmann

BERWIND PETERSEN KAUFMANN

April 23, 1897–September 12, 1975

BY EDWARD B. LEWIS

BERWIND P. KAUFMANN began his career as a botanist but turned from studies of plant chromosomes to making pioneering contributions to three principal fields: the induction of chromosomal rearrangements by ionizing radiation, identification of nucleolar organizer and heterochromatic regions of the somatic chromosomes of Drosophila, and determination of the biochemical composition of plant and animal chromosomes using purified enzymes.

Berwind Petersen Kaufmann was born on April 23, 1897, in Philadelphia, Pennsylvania. He graduated from the University of Pennsylvania with the B.Sc. degree in 1918, the M.A. in 1920, and the Ph.D. in 1925. While attending the university, he was an assistant and lecturer in the Department of Botany and at one point taught biology and drafting at Northeast High School in Philadelphia. His Ph.D. thesis dealt with the structure of the chromosomes of Tradescantia and led to his first major publication (1926). In 1926 Kaufmann went to Southwestern College, Memphis, Tennessee, where he taught biology. He left in 1929 to become a professor and the chairman of the Department of Botany at the University of Alabama.

In 1924 he married Jessie Thomson McCulloch, of Philadelphia. They had three sons: Berwind Norman, deceased; and surviving, Carl B. and Anders J., and 10 grandchildren.

On a sabbatical in 1932-1933 Kaufmann was a National Research Council fellow at the California Institute of Technology. In 1936 he left the University of Alabama to become a guest investigator in the Department of Genetics of the Carnegie Institution of Washington at Cold Spring Harbor, Long Island. He became a member of the permanent staff in 1937 and remained there for the next 25 years.

Kaufmann was elected a member of the National Academy of Sciences in 1952. He had been nominated by both the genetics and botany sections of the Academy, an unusual honor. He chose to join the Genetics Section. He served on the Biology Council of the National Research Council and was on the Council's Executive Committee. As the result of his radiation studies, described below, he was appointed a member of the National Research Council's Committee on Genetic Effects of Atomic Radiation.

Kaufman was elected president of the Genetics Society of America in 1961, after serving as secretary and treasurer. He served on the editorial boards of the *Journal of Morphology*, the *International Journal of Radiation Biology*, and *The Nucleus.* He was a member of the Marine Biology Corporation, Woods Hole, Massachusetts, and served as secretary and director of the Long Island Biological Association.

Kaufmann became director of the Department of Genetics of the Carnegie Institution of Washington in 1960, succeeding Milislav Demerec, who had been forced to retire because of a strict age limit of 65 that was then in force. Upon retirement in 1962 at age 65 Kaufmann moved to the University of Michigan, where he held joint appointments as a professor of zoology and botany and as a senior research scientist at the Institute of Science and Technology. In 1967 he was named professor emeritus. In his final years Kaufmann suffered from Parkinson's disease and a gradual decline in his mental faculties. He died on September 12, 1975, in a retirement home in Myrtle Beach, South Carolina.

I am indebted to family members and archival materials for events that influenced his life and reveal aspects of his personality. His father was a painting contractor and his mother a housekeeper. Kaufmann's interest in science was stimulated by his paternal grandfather, who was an ardent naturalist and collector of plants and animals in the Philadelphia area. Kaufmann was the first member of his family to receive a university education. While at the University of Pennsylvania he was on the fencing team and played tennis. He was a member of the Varsity Club and of the Philomathean Dramatic Society. He worked summers while in high school and college, helping on his father's construction projects. Kaufmann's wife, Jessie, had a long career in social work, having graduated from the University of Pennsylvania with an M.A. degree in that field. As a young man Kaufmann played the mandolin and violin. Later he and family members formed a mini-orchestra, his wife on piano, he on violin, and sons Berwind on flute and Carl on clarinet.

At Southwestern College in western Tennessee, where he arrived in 1926, Kaufmann taught the theory of evolution, only one year after the Scopes trials in Dayton in eastern Tennessee. This surely took some courage on Kaufmann's part, even though Southwestern College was a moderately liberal college.

His life at the University of Alabama was made miserable at one point when he was unwilling to pass some members of the football team. He had them over to his house and not only patiently tutored them but even watered down the tests somewhat, but they still could not pass. He was told by the administration that the team was Rose Bowl material and that he had to pass them. When he refused, the administration put the low-scoring students in the hands of a more malleable faculty member, with the result that the team kept on winning. Kaufmann was so disheartened that, even though tenured, he left the University of Alabama in 1936 to take up a staff position at the Carnegie Institution of Washington at Cold Spring Harbor. It could not have been a better outcome for a sorry episode.

In going to Caltech for the academic year 1932-1933, the family drove to California in an old car. While crossing the desert on the way, the radiator boiled over and cracked. There were no gas stations in sight. To illustrate Kaufmann's practical approach to getting things done, the story is told that he poured oatmeal into the hot water, which promptly sealed the leak.

During the years at Cold Spring Harbor, Kaufmann's family saw him mainly at mealtimes, since he worked all day, including weekends and holidays, in his laboratory starting at about 9:00 a.m. After coming home for dinner he would return and work till midnight, "catching up on the literature," a way of life not uncommon among Drosophila workers then as now.

He is said to have hated administrative work that he was required to do during the few years he was the director of the Department of Genetics at Cold Spring Harbor. It kept him away from his research.

He was fond of classical music and was an avid reader of the *New York Times* and the *New Yorker* magazine.

A paper (1931) published while at the University of Alabama indicates he had already begun work on Drosophila. Remarkably, this paper shows drawings of short sections of salivary gland chromosomes from two Drosophila species, *melanogaster* and *virilis*. Evidently they were drawn from sectioned material, and the full significance of these chromosomes was not understood until later when E. Heitz and

H. Bauer (1933) introduced a method of fixing and squashing the salivary gland chromosomes of the midge Bibio that revealed their true nature. It seems likely that Kaufmann's work on Drosophila chromosomes led to his taking a sabbatical in 1932-1933 at the California Institute of Technology as a National Research Council fellow. Two papers resulted and appeared as publications of the Kerckhoff Biological Laboratories of Caltech. In the first (1933) he developed an important method, still in use today, of preparing slides of squashes of neuroblast chromosomes from the larval ganglia as a source of somatic mitoses. Previously, oogonia had been used, however their chromosomes are smaller than those of the neuroblast cells, as well as more difficult to prepare. In the second paper (1934) he used his new method to demonstrate that detachments of attached-X chromosomes are the result of recombination between the X and Y sex chromosomes in their heterochromatic regions.

His next major publication (1937) dealt with the morphology of the chromosomes of *Drosophila ananassae*. Similarities and differences between this species and *D. melanogaster* are discussed, particularly in regard to their heterochromatic regions and the location of their nucleolar organizing (NO) regions.

In a series of papers Kaufmann undertook an analysis of the types and frequencies of chromosomal rearrangements induced by ionizing radiations, using the salivary gland chromosomes of Drosophila. Although genetic methods for detecting such rearrangements had been developed, they were labor intensive and required several generations of matings before the various types of rearrangements could be identified. By contrast, such rearrangements could be scored in the first generation of matings between, for example, irradiated wild-type males with unirradiated females. It sufficed to examine the salivary gland chromosomes of mature third instar larvae of such matings without waiting for the adult stage. This was an advantage over genetic methods that had to rely not only on survival of the adult stage but also on being able to breed it. Because the larvae had to be sacrificed to prepare their salivary gland chromosome, rearrangements detected in the larva could not be perpetuated. The goal, however, was not to create new rearrangements but to quantify the relationship between their frequency and the dose of X radiation. Major findings are described below.

- In a collaborative study (1938) based on analyzing over 1,700 slides, the frequency of induction of chromosomal rearrangements over a dose range of 1,000 to 5,000 r (or in the newer unit, centiGray [cG]) departed significantly from linearity. This contradicted a hypothesis that the dose-response relationship should be linear on the basis that rearrangements result from a single X-ray hit at a point where two (or more) chromosomal regions are in contact. Instead, strong support was provided for their being a dose-response that approached a 2.0 power of the dose, supporting an alternative hypothesis in which separate, more or less independent breakages are induced by X rays that later, unless restituted, take part in producing a chromosomal rearrangement. For a history of the controversy over whether the total break production varies linearly with dose or approaches a 2.0 power, see (1941,2).
- X-ray-induced breakages in the heterochromatic regions of a chromosome occur in approximately the same proportion that such regions occupy in the mitotic chromosome (e.g., in the case of the mitotic X chromosome the heterochromatic region constitutes one-third of the length of the entire chromosome), whereas in the salivary gland chromosome that region is only a small fraction of the length of

the chromosome, owing to the highly condensed state of its heterochromatin. The best estimate of the frequency of breakages in the heterochromatic region of the salivary gland X chromosome was 28 percent (1941,1) or close to the value of one-third based on the relative physical length of that region in the mitotic chromosome.

- Interchanges occur more or less at random between the various chromosomal arms.
- The yield of rearrangements from irradiation of Drosophila sperm is essentially the same whether a given X-ray dose was given all at once or when the interval between successive fractions was 16 days (1941,2). This result strongly supported the view that breakages in Drosophila sperm remain open and do not participate in forming rearrangements until the sperm fertilizes the egg.
- A study (1946) of over 1,400 breakages in the X chromosome showed that some sections had a statistically significant excess of breakages in proportion to the number of bands they contained. Kaufmann concluded that such regions contained interstitial heterochromatin.

Kaufmann was quick to take advantage of the wealth of rearrangements generated in the X-ray studies to map the location of the NOs in salivary gland chromosomes (1938,2). Although it was inferred that the NOs were associated with the X and Y sex chromosomes, he provided a proof of this by selecting rearrangements in which the NOs had been translocated to euchromatic regions of the chromosomes.

In the early 1950s Kaufmann and collaborators (1951, 1953) made use of purified enzymes to determine the composition of chromosomes. These studies were begun before it was known that genes are made of DNA and not, as dogma had it, protein. As a result complexes of DNA and histone-like proteins in chromosomes were identified before these associations were to become well defined biochemically. The role of such complexes is now one of the most active fields of molecular biology.

With M. Demerec, Kaufmann wrote the student handbook *Drosophila Guide,* which went through several editions and was used extensively in laboratory courses in genetics in high schools and colleges in the United States and abroad. At Cold Spring Harbor, Kaufmann maintained a strong interest in science education, and his laboratory attracted several young biologists from the United States and abroad.

I was fortunate to have been able to spend the summer of 1941 under his tutelage. He provided bench space, a research microscope, and the collection of slides that were the source of the data on which the radiation studies already described were based. He was very kind and generous of his time. I spent the summer, when not taking time out for the beach and playing Ping-Pong, analyzing as "unknowns" the slides that contained the various types of rearrangements. This involved determining the breakage points to the nearest section on the standard maps of the chromosomes (Bridges, 1935). Especially challenging were rearrangements that involved more than two breakage points. These were frequent following exposures to doses above 3,000 r. Kaufmann was especially proud of a slide that he showed me in which a rearrangement involved at least 32 breakages, 30 of which he was able to decipher. Kaufmann published this example (1942) and concluded that it furnished further support for the hypothesis that breakages in Drosophila sperm chromosomes remain open for either reunion or rearrangement until the sperm enters the egg. Representative rearrangements were photographed and illustrated in a publication (1939). It also contains a remarkable photograph of a nucleus that has all five major chromosome arms well spread without any overlaps or contact loops.

At the University of Michigan, Kaufmann and Marcia Iddles (1963) made an extensive study of ectopic pairing involving intercalary heterochromatin-like associations and their relation to puffing as measured in salivary gland chromosomes. One of the final papers of that period was a study carried out with Helen Gay, a former staff member of the Carnegie Department of Genetics (1969) and a long-time collaborator with Kaufmann at Cold Spring Harbor. They were able to quantitate how ends of chromosomes in the salivary glands have affinities for one another that become especially clear in the case of translocations involving the fourth chromosome and tips of the second chromosomes. Such affinity studies had long engaged other workers as well and were inspired by H. J. Muller's invention of the concept of the telomere. These studies have taken on new significance as the molecular basis for the telomere has been determined at the level of its DNA sequences. The possible role of telomeres in carcinogenesis and aging of cells is currently an active field of research.

I WAS INVITED to prepare this memoir in 2001. I was pleased to do so because of my admiration for Kaufmann as a person and as a scientist and for his many contributions to the cytogenetics of Drosophila. I am indebted to Kaufmann's sons, Carl and Anders, and Bobbie Stephens, Kaufmann's widow, for help in preparing this memoir—as well as Mila Pollock, director, Library and Archives; Clare Bunce, archivist, of the Cold Spring Harbor Laboratory; and Ellen Carpenter, archivist of the Carnegie Institution of Washington. An important source was an interview with Carl Kaufmann that is part of an oral history collection in the Cold Spring Harbor archives.

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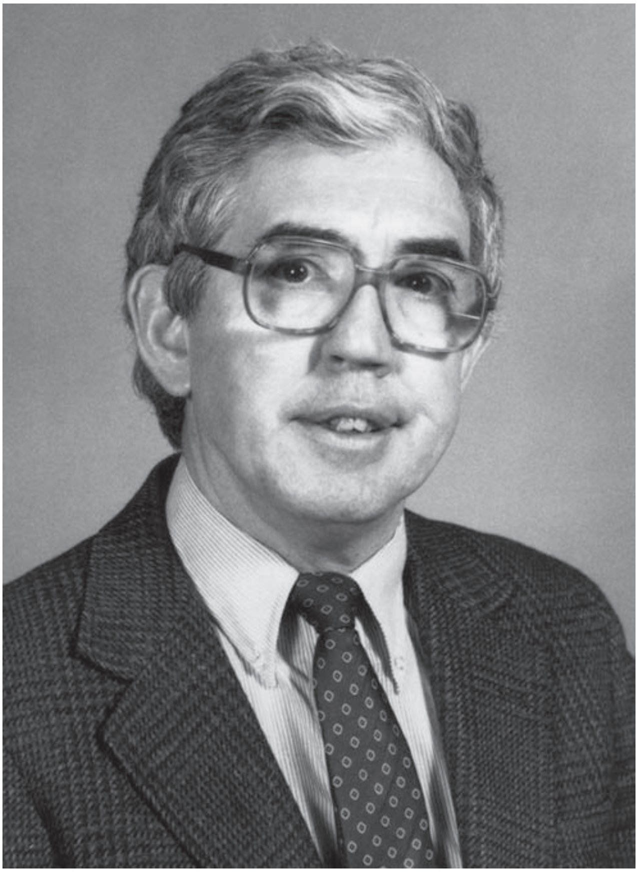

R. A. Laudise

ROBERT ALFRED LAUDISE

September 2, 1930–August 20, 1998

BY DONALD MURPHY

ROBERT A. LAUDISE GAINED his initial fame as a scientist and engineer by developing a manufacturable process for the growth of quartz single crystals and following the process through to industrial production. The passion he showed in that early work continued and broadened throughout his professional career and was evident in his relationship with his family, his colleagues, and the entire scientific community. His goal was to make the world better for posterity. Electronic and communications technology today is at a level unimaginable in Bob's youth, and much of that technology resulted from the science he did himself or championed as a Bell Labs manager and as a leader in the scientific community.

Bob was born and raised in the small town of Amsterdam, New York, the only child of parents who were both teachers in the public schools.[1] His parents encouraged his intellectual pursuits and as a child he was a voracious reader of all subjects. His broad knowledge and appreciation of a wide variety of topics continued throughout his life, but his future was cast when he received a chemistry set and his father built a "lab" in the basement for him at age 10. I did not know Bob as a child, but I am sure he was an egghead (the "nerd" of that era) and proud of it. Even as a youngster he knew how to gain the respect of his peers, by setting off stink bombs one April Fool's Day.

Bob graduated from Union College in Schenectady, New York, in 1952. He was an active alum, often hosting visits to Bell Labs by groups of students, with a stop at the Laudise's home for pizza. Bob earned his Ph.D. in chemistry in 1956 from the Massachusetts Institute of Technology, where he worked on the coordination chemistry of tungsten. Following graduation Bob took a job at Bell Laboratories, which was in expansion mode following the invention of the transistor.

Laudise spent his entire scientific career at Bell Labs. He was a member of the technical staff from 1956 to 1970, head of the Crystal Chemistry Research Department from 1970 to 1978, assistant director and director of the Materials Research Laboratory from 1972 to 1978, director of the Materials and Processing Research Laboratory from 1978 to 1992, and adjunct chemical director from 1992 to 1998. Beginning in the 1980s he also held adjunct professorships at MIT (materials science) and Rutgers (ceramics). He maintained his personal research throughout his career, even while managing as many as 150 scientists. During his last few years, he stepped down from management to concentrate fully on research and professional society activities. His last research effort was aimed at making electronic materials from organic molecules.

Bob was a devoted family man. He met his wife, Joyce, at a "mixer" while he was a graduate student at MIT and she was an undergraduate at Simmons College. Joyce told him she was having difficulty with chemistry, and Bob volunteered to meet her once a week at the Simmons Library to answer chemistry questions. The tutoring sessions ended with coffee dates and eventually Saturday night dates. Joyce says she learned a lot more about Bob than about chemistry.

They were married in 1957 shortly after Bob started working at Bell Labs. Bob was an only child and wanted a big family. Together they had five children.

Scientifically, Bob was renowned as a leading authority in the growth of single crystals and is often credited with transforming the field from an art to a science. He wrote the first comprehensive book on crystal growth, *The Growth of Single Crystals* (1970). His initial specialty was hydrothermal crystal growth. He began work on hydrothermal crystal growth to synthesize quartz (needed as an oscillator in electronics) with properties more reproducible than natural quartz at a reasonable cost. His research on synthetic quartz in the 1950s and perseverance through development and manufacture led to replacement of the highly variable, mined quartz with synthetic quartz that was cheaper and had better performance (a high quality factor). By the end of the twentieth century, quartz was second only to silicon in tonnage of single crystals used for electronics. Many of his colleague's desks were adorned with quartz crystals passed out by Bob.

Bob was widely recognized for his scientific accomplishments. He was elected to the National Academy of Engineering in 1980 and the National Academy of Sciences in 1991, as well as the American Philosophical Society in 1997 and the American Academy of Arts and Sciences in the following year. He received numerous prizes and awards for his work, including the Orton Lecture Prize of the American Ceramic Society, the Sawyer Prize for contributions to piezoelectricity, the Applications to Practice Award of the Materials, Metals and Minerals Society, and the Materials Chemistry Prize of the American Chemical Society. In 1984 he was awarded the first experimental award of the International Organization of Crystal Growth, which was renamed the "Laudise Prize" in 1989 in his honor. Also named for him is the "Robert Laudise Medal for Industrial Ecology" of the International Society of Industrial Ecology.

He belonged to at least 10 professional societies and was editor of the *Journal of Crystal Growth* for 15 years and the *Journal of Materials Research* until his death. He served in advisory roles to several national laboratories, universities, and National Research Council committees, including a term as chair of the National Materials Advisory Board. Bob worked aggressively to foster cooperation among the different professional societies to positively influence government action related to the research and development of materials. He was so identified with materials that following his death, the National Academy of Engineering dedicated a symposium ("Materials—The Opportunity") in his honor at the 1998 annual meeting, which was attended by his family.

Laudise first became a manager at Bell Laboratories at a time of enormous importance for crystal growth as a subject. Not only were silicon and quartz critical to electronics but large, high-quality crystals were needed for other emerging applications, such as lasers, nonlinear optics, and magnetic bubble memory. It was the heyday of crystal growth, and Bob was in his element. He was a visionary and knew the world was in a rapid state of change, and he always focused on opportunities while some saw only the obstacles. In a 1970 profile (Chemical Innovators, 1970) he talked about recruiting MIT students to work at Bell Labs in communications technology. Paraphrasing his retort to those who thought work with greater social consequences was a loftier aspiration, he said that communications was morally neutral and there might be great benefit if we had cheap, two-way picture phones and devices for ordering merchandise without struggling through traffic or crowded store aisles. He added that he did not see improved communications as a panacea, but he added, "Wouldn't it be nice to explore these technological implications in a sensible sort of way, to be in a position to do some of these things if they really seem useful?" Over the years Bob was a very effective recruiter of young talent to Bell Labs.

As a manager Bob championed research in a wide variety of areas that he imagined could help bring about the vision of better communications. These included materials such as optical fibers, thin films, superconductors, and nanoparticles. While he is best known as a champion of materials, he also stressed the idea that AT&T's (later Lucent's) factories were chemical plants and that processing and reliability were key to success. In the 1990s he was instrumental in advancing the concepts of industrial ecology as a critically important field. Just outside what was his office at Bell Labs is a garden marked by a plaque that reads,

> This garden is dedicated in the memory of Robert A. Laudise, Director, Lucent Bell Labs Physical Science Research by his friends and colleagues in recognition of his contribution to Electronic Material and Industrial Ecology September 1999.

I credit Joyce's love of nature and ecology with inspiring Bob to make the field of industrial ecology a part of his legacy. They had a vacation home in the Pocono Mountains on Twin Lakes. There they taught their children to swim, row, canoe, and sail. Bob and Joyce's mutual interest in the environment and lake ecology blossomed at Twin Lakes, as they became concerned that many Pocono lakes were being damaged by acid rain and that alga blooms appeared with greater frequency. Beginning in 1987 they were a team of two collecting lake samples, sending them off to a laboratory for analysis, and writing annual reports on the quality of the lake.

At Twin Lakes Bob loved to combine business with pleasure. I was there on the way to a Gordon conference with the imminent French solid state chemist Paul Hagenmuller. We had a swim, a barbecue, and taught Paul how to play monopoly. The Laudises were there several times with his colleague, Bob Barns, and his wife, both to have fun and examine the ice, with the intention of writing papers on ice crystals. They published an article showing that icicles had regions as long as 8 inches that were single crystal (Laudise and Barns, 1979) and another showing that single crystals of ice from Twin Lakes and a neighboring lake could be larger than a foot long (Barns and Laudise, 1985).

I learned many lessons of management and life in general from Bob. He may not have originated these thoughts, but he ingrained them in me and my contemporaries. A few that come to mind include

1. Remember, everything is made of something (and don't ever let those software guys forget it).
2. To ask permission is to seek denial.
3. Get people to do what you want by making them think they thought of it.
4. Research will not thrive if you throw it over the fence unless you follow it.
5. Do not have any meeting devoid of technical content.
6. The amount of angst each person feels is fixed, but you can control whether you worry about important things or unimportant things.
7. Ninety percent of the people go through life without really knowing what is going on 90 percent of the time.

He was anything but a "by the book" manager. He believed in shielding the troops as much as possible from constantly changing business pressures coming down from the top, but he had his nose to the internal political winds and would also pass along information he felt we needed to know even when instructed otherwise.

He demanded excellence from those who worked for him and rewarded those who truly did excel. Bob wrote more nominations for various forms of recognition than anyone else who comes to my mind. He truly reveled when his colleagues received accolades, often hosting impromptu wine-and-cheese parties to celebrate. He could also recognize an excuse a mile away and would give a little smile and say, “What’s the matter? Did the dog eat your homework?” Joyce said he did this at home with the kids too.

One of the most intense experiences for managers at Bell Labs was performance review, and Bob’s personality revealed its true nature during those reviews, which determined raises for the year and, more importantly, how your organization was viewed by those that controlled the resources. During much of the 1980s and early 1990s the group of department heads that reported to Bob (including myself) got pretty familiar with one another and Bob’s performance review style, which was more intense, franker, fairer, and more time consuming than I experienced with any of my other bosses. The process was basically that everyone wrote what Bob called an “I am great letter,” telling what their accomplishments for the year were and why they were world class. Then department heads would get together with directors and use these as the basis for evaluating people doing quite different work across department lines. This process was repeated at successively higher levels of management until, in principle, the entire company had been cross compared. Our first-level review generally took a week, often with pizza brought in on the weekend and even moved to Bob’s basement on occasion. During that time we all learned a lot about what other groups were doing and made lists of opportunities we wanted to emphasize in the coming year.

Bob had to feel he understood and believed everything in all the I-am-great letters before he declared the review finished. It was certainly the most intense week of the year for most of us. At some point during the review he would inevitably become irate that someone he judged as underperforming was making double the salary of his daughter, who was a nurse and had patients' lives depending on her performance. At the conclusion of one such review (celebrated at a local pub over beer) the department heads presented Bob with their own jocular review of his performance, which rendered him uncharacteristically speechless and embarrassed. At the conclusion of another equally intense review, Bob closed his notebook and said words to the effect of "I have prostate cancer and will be operated on next week, and I want each of you to go get a PSA test if you haven't had one recently." I sat stunned at how he showed no hint of what was facing him through that entire week. He did well through the surgery and had several more very productive years before the cancer metastasized, leading to a premature death.

Life with Bob as a boss could also be challenging at times. Over the years Bob owned two Volkswagen convertibles, which he loved to drive around with the top down. None of my peers really enjoyed riding around with the top down on a hot, humid New Jersey summer day, and I admit to conspiring to avoid riding with him. Occasionally I showed up in his office with a small problem that triggered a venting of some pent-up frustration. I soon learned that before long he would come to my office in a much better mood to apologize. I saved up my most important requests for those occasions, and they were almost always approved. These were small prices to pay for the best boss I ever had.

Bob had been a runner in high school, and he sired a second generation of high school track runners. The book, *Going the Distance* by George Sheehan, a medical doctor, runner, and prostate cancer victim, was especially meaningful and helped Bob's family though his last days. Bob's youngest son, Ed, operates a homeless shelter on Main Street in Immokalee, Florida, where Bob and Joyce often spent vacations helping out. There is now a bench in front of the shelter with a plaque that reads, "To my father. He ran the distance, making the most of every moment and bringing out the best in every person he met along the way."

I THANK BOB'S WIFE, Joyce, for sharing personal information for this memoir and for being a good friend for many years. I also benefited from helpful suggestions from Bob's friend and colleague, Paul Fleury.

NOTE

1. Personal information about Bob's youth and family used throughout this memoir were graciously provided by Joyce Laudise in several communications in early 2004.

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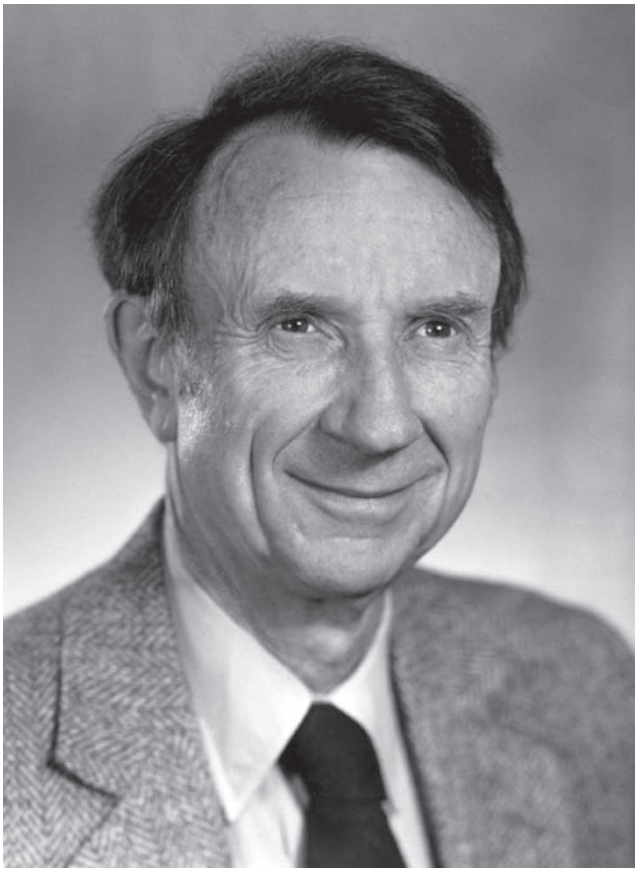

Boyce D McDaniel

BOYCE DAWKINS McDANIEL

June 11, 1917–May 8, 2002

BY ALBERT SILVERMAN AND PETER STEIN

BOYCE DAWKINS ("MAC") McDaniel died from cardiac arrest unexpectedly and quickly on May 8, 2002, at his home at Kendal of Ithaca in Ithaca, New York. For more than half a century Mac played a leading role in the birth, development, and mature phases of accelerators and experimental particle physics. Throughout his career his time, often on a daily basis, was seamlessly divided between administration, accelerator physics, instrumentation, and particle physics.

Mac was born on July 11, 1917, in Brevard, North Carolina, the youngest of the three children of Allen and Grace McDaniel. He completed high school in Chesterville, Ohio, in 1933 and graduated from Ohio Wesleyan University in 1938. In 1940 he received his M.A. degree under Eugene Crittenden at what is now Case Western Reserve University, and immediately entered a Ph.D. program at Cornell University. As a graduate student of Robert Bacher from 1940 to 1943, he built a multichannel high-resolution time-of-flight energy spectrometer and used it to carry out precision measurements of the energy levels of indium for his thesis (1946).

Following the completion of his Ph.D. he accepted a postdoctoral position at MIT to learn the rapidly evolving field of fast electronics to apply it to particle physics research.

After only a few months in Cambridge, Mac was recruited by phone to join a secret government project at an undisclosed location. Without any knowledge of its nature and location Mac abruptly pulled up stakes and joined the Manhattan Project in Los Alamos. A pressing need for accurate measurements of neutron cross-sections had arisen. Mac brought the neutron spectrometer he had used for his Ph.D. thesis at Cornell to Los Alamos, where he led a research team that discovered and made accurate measurements of fission induced by resonance absorption of epithermal neutrons in uranium and plutonium. This data made an important contribution to the design of the first nuclear bombs. He subsequently was transferred to a group set up to assemble the bomb, and played a key role in the test of the first plutonium bomb at Alamogordo. Mac had constructed a portable neutron counter to monitor the activity of the plutonium core of the bomb. Every four hours before the test he climbed an open steel ladder to make the measurement. Val Fitch, also at the test site, has written the following account of Mac's role.

Titterton and I were at the test site to make measurements on the simultaneity of detonation of the 32 lenses, and so I knew about climbing that ladder to the top of the tower. I can testify, personally, that climbing up an open ladder to the top of the tower was a highly intimidating experience, and Mac was doing it every four hours, day and night. He was not one to pass a job like that to someone else.

Mac's last measurement was made at 2:00 o'clock on the morning that the bomb was exploded at 5:30. It so happened that at that time a thunder and lightning storm was passing through the area. There was Mac alone, cozying up to that gadget at the top of the tower to measure its neutron activity, while lightning was playing around the area in the spectacular fashion common in the desert. I will let your own imagination play on what he may have been thinking.

After the war, Mac returned to Cornell and took charge of the 2 MeV proton cyclotron built by M. Stanley Livingston before the war. It was one of the earliest and lowest-energy cyclotrons ever built, but Mac and his students did a lot of good nuclear physics with it. One experiment in particular deserves mention. A good way to study nuclear structure is to measure the energy of gamma rays emitted by excited nuclei. To carry out these measurements it was necessary to measure gamma-ray energies more accurately than was possible with existing detectors. In characteristic style he, together with his student Robert Walker, invented the pair spectrometer, which for many years was the best available instrument for measuring gamma-ray energies (1948). Mac was much admired by his students. Bob Walker described him as the perfect thesis advisor, allowing the student great independence but always there when needed.

Under the leadership of Bob Bacher and Hans Bethe the Cornell Laboratory of Nuclear Studies was established in 1946, with Bob Bacher as the director. Bacher left in 1947 to become a member of the Atomic Energy Commission, and was succeeded by Robert Rathbun ("Bob") Wilson. Mac was one of the charter members of the laboratory and continued to work there for the rest of his life. He was the associate director under Wilson from 1960 to 1967, and was appointed director of the laboratory in 1967 when Wilson left Cornell to build Fermilab; he continued as director until his retirement in 1985. The authors of this memoir are members of the laboratory (A.S. since 1950 and P.S. since 1956).

Under Wilson's leadership, from 1947 to 1967 the laboratory built four electron synchrotrons, from 300 MeV to 10 GeV. The research at all these machines focused on two subjects: quantum electrodynamics (QED), which describes the electrical forces between elementary particles, and quantum chromodynamics (QCD), which describes the strong force between elementary particles. The drive for ever-higher energies was motivated by the desire to test QED at smaller distances and to extend the study of QCD to more massive hadrons.

To keep an active experimental program each accelerator was kept in service until its successor was built and ready for testing. It was a very ambitious program, with lots of technical problems, a perfect match for Mac's extraordinary abilities, and he played a leading role in its success.

The first accelerator, started in 1946, was a 300 MeV electron synchrotron, one of four such machines started at that time (1949). Dale Corson, also one of the laboratory charter members, tells of a bad moment in the construction of this machine. The magnet coil was wound incorrectly, a fatal flaw. To get it repaired by the manufacturer could take months. Mac made a toy model of the coil, studied it carefully for an evening, and discovered an ingenious but simple way to repair it, which he did in about a day, and defused the crisis.

The accelerator was completed in 1949. Among the early experimental results were precise measurements of the electromagnetic interaction of high-energy gamma rays with nuclei, confirming an important theoretical calculation of Bethe and Heitler, an elegant measurement by Dale Corson of the rate of synchrotron radiation, resolving a theoretical controversy on the subject, an influential measurement by Hartman and Tomboulian of the spectrum of synchrotron radiation, and, most importantly, many measurements of the properties of the pi meson, thought at the time to be responsible for the nuclear force. The discovery in the next few years of several other particles heavier than the pion, all of which appeared to play a role in the nuclear force, showed that the story was more complicated than expected and fueled the desire for higher-energy experiments to explore and clarify the situation.

In 1953 Bob Wilson asked the Office of Naval Research (ONR) for money to build a 1 GeV electron accelerator, which he described in these words.

> The laboratory has indulged itself in some high adventure. A new synchrotron has been designed which is to give over a billion volts of energy. The design is highly controversial in that it is exceedingly small and cheap for what it will do, hence there is considerable risk that it may not work at all. On the other hand, if we are successful, we shall have the largest electron accelerator in the world and new areas of research will be open to us.

To control the cost Wilson proposed to drastically reduce the space containing the beam. This space set the scale for the size of most of the components of the accelerator. The magnet gap, coil, iron, and power supply and the vacuum system were all reduced in size. The machine was smaller, the components easier to build, and the construction time shortened, all of which reduced the costs. One can get some feeling for how radical a change this was from the fact that the 300 MeV magnet weighed 80 tons and the 1 GeV magnet, with four times the radius, weighed only 20 tons.

There was, of course, a price to pay for all of these blessings. The smaller aperture complicated the acceleration and containment of the beam. One of the most serious problems was that the position of the beam could not be accurately measured without destroying it. Again Mac came to the rescue. He invented, or at any rate produced, an instrument to measure accurately the position of the beam throughout the acceleration cycle. This changed the tuning of the machine from an art to a science, and by 1957, with the machine running at about 750 MeV, experiments began. The higher energy paid off immediately. The first round of experiments revealed a new excited state of the nucleon at 1440 MeV.

Mac's wizardry was also seen in the experimental program. One of the important research programs was the study of the photoproduction of K mesons, one of nature's more interesting particles. The early work in this field was crude, largely because the efficiency for identifying the K mesons was very small. Mac designed and built an apparatus that identified K mesons with good efficiency and low background by accurate time-of-flight measurement (1963). Then, together with his students he carried out a series of precise measurements of K-meson photoproduction that are still among the best measurements in this field (1962, 1963).

Despite Wilson's warning, the 1 GeV synchrotron was a great success, reaching eventually 1.4 GeV, with good intensity and supporting an active research program. It was the first strong-focusing accelerator to accelerate a beam. The success of the 1 GeV machine and successive machines built by Wilson at Cornell and Fermilab had a great influence on accelerator design.

The next machine was originally conceived as a 3 GeV electron synchrotron to be built by an industrial firm with accelerator experience. The National Science Foundation (NSF) had approved the project, which was to cost $8 million. At some stage Mac concluded that the two 6 GeV electron synchrotrons operating at that time in Cambridge, Massachusetts, and Hamburg, Germany, would make the 3 GeV machine obsolete by the time we got it. He proposed building a 10 GeV accelerator, which would again give us the highest-energy electron synchrotron in the world. He estimated that the 10 GeV machine could be built in-house for about the cost of the proposed 3 GeV machine. Wilson enthusiastically embraced Mac's suggestion, called the NSF to return the money, and told them he would be proposing a 10 GeV accelerator for about the same cost.

In a daring move Bob and Mac proposed building two machines simultaneously; a quick upgrade of the 1 GeV machine to 2 GeV and a brand new 10 GeV machine in a new facility. Mac supervised the upgrade of the 1 GeV. After about a year spent in constructing such parts as the magnet (which was built in-house by Mac and one technician) and a new donut, the 1 GeV machine was turned off in January 1964. A little more than three months later, in April, the 2 GeV machine was ready. Less than a day after the beam was first injected, the accelerator reached full energy. Two GeV was a modest energy increase, but it opened important new physics possibilities. The 2 GeV synchrotron was active until the 10 GeV machine was finished.

The 10 GeV machine was also built in Wilson's "small is better" style. In fact, the magnet aperture was even smaller than that of the 1 GeV magnet. Helen Edwards, who had been a student of Mac's and was one of the key players in commissioning the 10 GeV machine, described Mac's role in the project in these words.

> Mac focused specifically on the magnet fabrication, the magnet string test, and seeing that everything got designed, built, and installed. Mac was superbly good at technical design and implementation (both mechanical and electrical). He had a remarkable knack for perceiving a problem, then with great determination, intense energy and speed, and no wasted effort, coming up with a solution. There was the problem; then there was the solution. It was awesome. If you couldn't solve something, well, Mac could.

Shortly after the 10 GeV project was funded, Wilson was appointed director of the National Accelerator Laboratory (now called Fermilab), a new laboratory to be built in Illinois, which immediately demanded much of Wilson's attention, and Mac took over most of the responsibility for building the 10 GeV machine. Mac was appointed director in 1967, when Wilson left to build Fermilab, and continued in that role until his retirement in 1985.

The 10 GeV machine was approved in 1964. Research began at 7 GeV in 1967 and reached design energy, 10 GeV, in 1968. Though Mac did not continue active participation in any research project, he paid close attention to the experimental program and was consulted frequently. Helen Edwards was not the only one who knew that when you were stuck on a problem, Mac was the one to go to.

Many individual experimental physicists visited Cornell, particularly from Italian laboratories with whom Cornell had a lively exchange program; however there were no "user groups" that worked at the lab. This changed with the 10 GeV accelerator. Harvard, Rochester, MIT, Syracuse, and a group from DESY, the German laboratory in Hamburg, had groups working at Cornell and made up a substantial part of the experimental program. Mac welcomed their participation and made no distinction between them and the in-house groups. Harvard, Rochester, and Syracuse have been part of CLEO (an experiment at the Cornell Collider) since its beginning in 1976.

In 1972 Wilson prevailed on Mac to take a leave of absence from Cornell to assist in commissioning the 400 GeV proton synchrotron. The project was experiencing serious problems with accelerating useable beams beyond 20 GeV, and frequent component failure resulted in intermittent operation. Mac threw himself into the work with his usual enthusiasm. When he left eight months later, the beam had been accelerated to 300 GeV, and the beam intensity had increased by a factor of one thousand, to 3×10^{12} protons per pulse.

Ned Goldwasser, associate director of Fermilab, speaking of Mac said, "Shortly after his arrival on his first visit he 'disappeared' into the main ring tunnel and was rarely seen above ground. He soon became the acknowledged leader of all the main ring crew. That happened, not by pushing on his part, but simply by his setting an example of clear thinking and hard work." Speaking of Mac's contribution, Bob Wilson said, "This bravura performance demonstrated Mac's skill for leadership as well as his celebrated sixth sense for finding sources of trouble and fixing them."

Mac's work at Fermilab became known to and admired by a worldwide audience. From that time on he was recognized internationally as a leading figure in high-energy physics. His advice was highly valued, resulting in his service on many high-level scientific advisory committees in particle physics, astronomy, synchrotron radiation, and cosmic rays.

In the early 1970s the laboratory, under Mac's leadership, began a search for its next project. Mac, Maury Tigner, and others were enthusiastic about an electron-positron colliding beam facility, but some questioned its utility for physics beyond testing the validity of QED. The discovery of the psi meson at Stanford, in November 1974, decisively removed any such doubts. At about the same time, Maury Tigner invented a very clever scheme to use the synchrotron as an efficient injector for a collider. At that stage the collider looked very attractive, and in May 1975 Mac submitted a proposal to the NSF to "convert" the 10 GeV synchrotron to an 8 GeV (16 GeV center-of-mass energy) electron-positron collider using the synchrotron as an injector and adding a storage ring in the same tunnel (1975). Since the only major components that had to be built were the storage ring and the RF system, the cost was low and the construction time short. The proposal was initially rejected, but Mac rallied the support of the high-energy community, and in 1977 the NSF approved the project.

Mac threw himself into the construction and by October 1979, just two years after the proposal was approved, the two experiments, CLEO and CUSB, were taking data (1979). The rich trove of 25 years of b-quark physics that followed was the ultimate reward for the daring, innovative, and low-cost style of physics practiced by Mac, Bob, and their Cornell colleagues.

After his retirement in 1985, Mac remained active and played an important role in both the Cornell electron storage ring and CLEO. In addition, he served on many advisory and visiting committees for the NSF and the U.S. Department of Energy. He was a trustee of the Associated Universities (1963-1975) and Universities Research Association (1971-1977); a member of the Department of Energy's High Energy Physics Advisory Panel (1975-1978); and a member of the Superconducting Supercollider Board of Overseers (1984-1991), which he chaired part of this period. He was elected to the National Academy of Sciences in 1981. His modesty, integrity, and sound judgment, and his passion for life, physics, and making things work were widely recognized and admired by the scientific community.

Some of the text of this memoir was taken, with some changes, from an obituary that we wrote for *Physics Today* (February 2003, p. 73). The text quoted from Bob Wilson was taken from a yearly report to the Office of Naval Research on June 15, 1953, and the texts of Val Fitch, Helen Edwards, and Ned Goldwasser from a memorial service for Mac at Cornell University on September 21, 2003.

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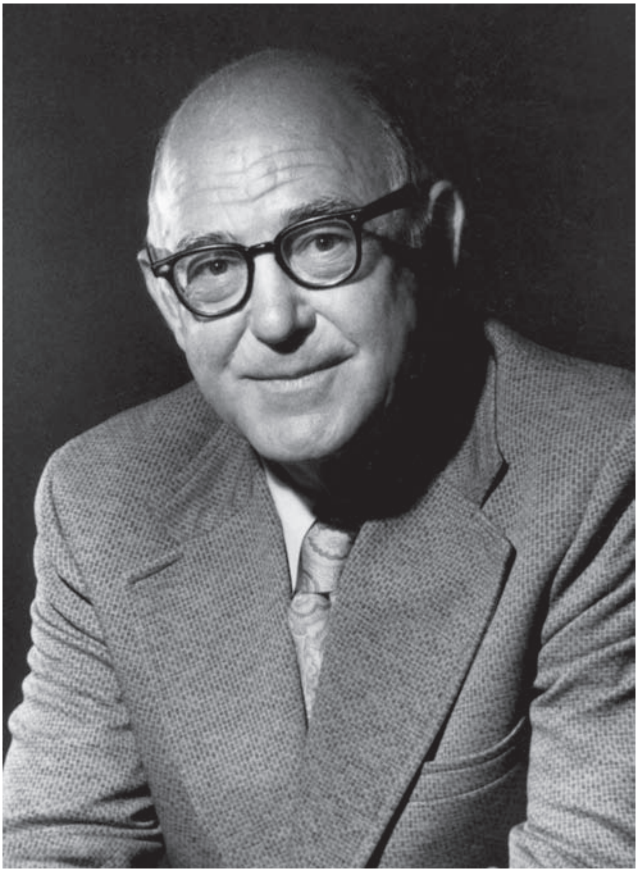

Photo by Anthony di Gesu, La Jolla, California

WILLIAM DAVID McELROY

January 22, 1917–February 17, 1999

BY J. WOODLAND HASTINGS

William David McElroy, a biologist who made groundbreaking discoveries in bioluminescence and was an administrator of great talent, died of respiratory failure at Scripps Memorial Hospital in San Diego, California, at the age of 82. He was an innovative and internationally prominent scientist and administrator, with a continuing agenda for experimental projects and research support for all areas of science, both basic and applied.

At the time of his death McElroy was a professor emeritus at the University of California, San Diego, having served as its chancellor from 1972 to 1980. He was on the faculty at the Johns Hopkins University, where from 1946 until 1969 he was the founding director of the McCollum-Pratt Institute, and from 1956 to 1969 the chairman of the biology department. He was a member of many professional scientific societies and served as president of several, including three of the largest: the American Society of Biological Chemists, the American Institute of Biological Sciences, and the 116,000-member American Association for the Advancement of Science. He served on the President's Science Advisory Committee under both Kennedy and Johnson (1962-1966), was elected to the National Academy of Sciences in 1963, was director of the National Science Foundation under Nixon

(1969-1972), and was a member of the President's Committee on the National Medal of Science Award (1972). He was the recipient of honorary degrees from some twelve institutions, including Johns Hopkins University, the University of California, San Diego, and the University of Bologna, Italy, and was an honorary member of Phi Beta Kappa. He was a member of the American Philosophical Society and the American Academy of Arts and Sciences, and he received from the latter the Rumford Prize, given in recognition of important discoveries concerning "heat or light." He was also a recipient of the Barnett Cohen award in bacteriology from the American Society for Microbiology.

McElroy was born on January 22, 1917, in Rogers, Texas, the son of William D. and Ora Shipley McElroy. He completed high school in McAllen, Texas, and graduated from Pasadena Junior College in 1937. In 1939 he obtained a B.A. from Stanford University, where he played right end on the football team in 1938 and 1939; he continued studies at Reed College in Portland, Oregon, where he received his M.A. in biology in 1941. There he met a fellow student, Nella Amelia Winch; they were married on December 23, 1940.

McElroy continued as a graduate student in biology at Princeton University with E. Newton Harvey as his mentor, receiving a Ph.D. in 1943, and continuing there with Harvey and others on a research project for the U.S. Office of Scientific Research and Development (1946) during World War II. From 1945 to 1946 he held a National Research Council Fellowship at Stanford, working with George Beadle, where he also developed close scientific relationships with Edward Tatum and Cornelius van Niel.

Mac, as he was called in the early days, began his career in 1946 as an instructor of biology at Johns Hopkins University, where the noted embryologist Benjamin H. Willier was chairman. With fireflies abundant on campus, he carried out an experiment that was to become the signature of his career (1947), in which he showed that light emission in extracts of the firefly lantern required adenosine triphosphate (ATP), a recently identified "high-energy" molecule in metabolism (Lipmann, 1941). He had earlier explored this possibility with Robert Ballentine using a different luminous species in a publication from Princeton (McElroy and Ballentine, 1942).

As it turned out, ATP alone could not be the energy source for light emission, as suggested by the title of his 1947 publication, since the energy released by ATP hydrolysis (~ 7 kcal) is far less than the energy of a photon in the yellow (~ 50 kcal). He later showed the role of ATP in the light-emitting reaction to be analogous to the activation of amino acids in protein synthesis, reacting with luciferin to form the luciferyl-adenylate intermediate (1968), which then reacts with oxygen to form a high-energy peroxy intermediate, the breakdown of which provides enough energy to emit light. This work highlighted the relatedness of different biochemical systems and the importance of understanding basic biochemistry. One should not recount this experiment without recalling that ATP was not found in the freezer or in a catalog in those days; indeed, few biochemical reagents could be purchased. McElroy obtained the ATP by purifying it himself from rabbit muscle.[1]

Research on the firefly system continued briskly in McElroy's laboratory at Hopkins over the decades of the 1950s and 1960s. Arda Green spearheaded the purification and crystallization of luciferase (1956), while other students pursued the discovery that coenzyme A could stimulate light emission (1958), which had its explanation later from the discovery that firefly luciferase and CoA ligase are homologous (Schroder, 1989). With White and McCapra in the

Hopkins chemistry department, the structure of firefly luciferin was determined (1961). Howard Seliger joined the lab in the late 1950s and determined the quantum yield of the reaction to be close to 1.0 (1960); he also measured the spectral composition of the light in relation to the structure of the luciferase (1964).

McElroy and his group also researched other luminescent organisms, including bacteria (1953, 1955), fungi (1959), and dinoflagellates (1961, 1971). The first and last are now biochemically well described. He later coauthored an influential book with Seliger (Seliger and McElroy, 1965), in which the evolutionary origins of bioluminescence were interestingly associated with the appearance of oxygen in geological time, promulgating the provocative hypothesis that luciferase originated as an oxygen scavenger. An article in *Scientific American* (1962) reached scientists in many areas, as well as laypersons.

Upon Willier's retirement McElroy assumed the chairmanship of the biology department at Hopkins and built a very strong department, in both research and teaching. Andre Jagendorf testifies to his effectiveness as chair, saying that Mac shielded faculty from administration, meaning, however, that faculty might not be consulted in many important decisions. Those who did not agree with this approach went elsewhere. In teaching, he himself was dedicated and effective. His lectures in biochemistry were dynamic and provocative, as recounted by Gilbert Levin, a NASA engineer who, while developing biology experiments for the first Mars mission, returned to Hopkins for a Ph.D. in biology. "He was certainly a far cry from the typical JHU prof! The first thing he did was to doff his suit coat and loosen his tie. [He used] . . . simple English, including plenty of vernacular . . . [and] taught us a lot of stuff in each session." Levin credits McElroy's lectures with providing him with the understanding of optical isomers that led to NASA's "labeled release" experiments in the search for life on the 1976 *Viking* mission to Mars (Levin and Straat, 1979). "The chirality issue started by Bill's lecture has kept the space biology community riled for 28 years."

McElroy's strong beliefs in quality education for all led to his involvement in the Baltimore city schools and junior college; he served as a member of the Board of School Commissioners for the city from 1958 to 1968 and chairman of the Board of Trustees for Baltimore Junior College in 1968 and 1969. He was also greatly concerned with the problem of overpopulation of humans and was active in promoting measures to address the problem (1969).

In the 1960s McElroy was divorced from Nell. He met and married Marlene Anderegg DeLuca, a fellow scientist at Johns Hopkins, in 1967. In 1969 he was appointed director of the National Science Foundation by President Nixon, and he moved to Washington, D.C. His tenure saw increases in funding from $400 million to $650 million, and the introduction of important new programs, especially in the area of applied science, with no compromises on basic research (1975).

Eloise Clark and Sig Suskind, who were at the National Science Foundation at the time, recall that "shortly after his arrival at NSF the Office of Management and Budget proposed a major reduction in the science education budget of NSF. There was speculation that this was simply the first step in further reductions on the path toward elimination of the agency. Recognizing that the decision to cut the education budget was irreversible, McElroy is said to have persuaded OMB to leave the money in the budget by orienting the targeted funds to applied research."

He was recently remembered by the later NSF Director Rita Colwell as "a visionary who expanded NSF's mission beyond the 'core' disciplines . . . [who] recognized that NSF could not fulfill its mission without incorporating fully the engineering and social sciences, and extending greatly its public outreach activities. [He] fought to keep the basic research 'edge' on a larger effort to link research investments with societal needs."

As director McElroy made bold moves. As an example, it was said that in a meeting to discuss research funding at Caltech, an undergraduate asked why students could not apply for grants. McElroy asked that they describe their proposed research and its cost on a single sheet of paper, then on the spot signed the authorization for a grant in support of the work.

In 1972 Mac, now more often called Bill, was appointed chancellor at the fledgling 12-year-old campus of the University of California, San Diego, in La Jolla. He oversaw the tripling of the budget, the expansion of arts, humanities, and social sciences, which he strongly supported, and significantly, he involved community leaders in the governance of the university by establishing a Board of Overseers. But his strong administrative style, which had been so successful earlier, met opposition from other administrators and faculty; therefore, in 1980 he returned to full-time research and teaching as a professor of biology. A distinguished lectureship in biology was endowed in his honor, and a garden and terrace adjacent to the Mandeville Auditorium named for him.

During those years as chancellor, McElroy maintained an active participation in research and scholarship. He organized a Southern California bioluminescence group, bringing together colleagues from as far north as Santa Barbara for discussions of research. Ken Nealson, then a faculty member at Scripps Institution of Oceanography remembers "some great meetings at the chancellor's house, where a lot of nice interactions were started due to him" and that he was "a strongly positive influence who appreciated the subtleties of the science across a wide range of disciplines, and encouraged everyone, but especially the young scientists."

During the next decade with his wife, Marlene, he directed work leading to the cloning of the firefly luciferase gene and its expression in several organisms, including tobacco plants, with many important implications for applications (de Wet et al., 1985, 1987; Ow et al., 1986). Queried on National Public Radio about "lighting up tobacco" with luciferase, Marlene joked that her next project would be to clone the gene in yeast and produce "lite" beer. Keith Wood and other students participated in those studies, which included the discovery in a single beetle of different genes coding for luciferase isoforms eliciting different colors of bioluminescence (1989). Thus the enzyme, not just the luciferin (the product of which is the emitter), has an important role in determining the color of the light emitted. Marlene's untimely death in 1987 was a shock, and his activities were greatly reduced in the 1990s. In 1997 he met and married Olga Robles, with whom he spent the last years of his life.

The many practical applications of firefly luciferase were already evident and underway by the early 1950s. Bernard Strehler, one of McElroy's first graduate students at Hopkins, promoted and exploited the use of luciferase for the determination of ATP (1957). At the Oak Ridge National Laboratory, with William Arnold, Strehler used luciferase to try to demonstrate photosynthetic phosphorylation in *Chlorella*. In that, they were frustrated by the occurrence of luminescence in controls lacking luciferase, which thereby led to their important discovery of delayed light emission in plants (Strehler and Arnold, 1951; Arnold, 1986). But its use in the detection of ATP became widespread, especially for the detection and quantification of living organisms (Chapelle and Levin, 1968; Holm-Hansen and Booth, 1976). NASA pioneered this work as a possible way to detect life on Mars; it was instrumented but never flown. It is now widely used to test for the presence of organisms contaminating foods, for example, soft drinks and beef carcasses after slaughter (Hastings and Johnson, 2003). Even more widespread is the use of luciferase as a reporter of gene expression in biology and medical research and diagnostics. In 2004 a search of literature on the Web gave hundreds of hits yearly for firefly luciferase, the great majority being concerned with clinical and research applications.

In La Jolla, Bill and Marlene established one of the early biotech firms, the Analytical Luminescence Laboratory, which provided many of the agents and reagents for various uses. Later, the ProMega Corporation (Madison, Wisc.) obtained rights and became a major supplier of luciferase-related reagents, with former student Keith Wood as director of research in that area.

I joined Mac's lab in the early summer of 1951 after completing my Ph.D. at Princeton, also with E. Newton Harvey. Mac put me in charge of firefly collection, the now-legendary operation in which children were paid a penny for each firefly collected. He instructed me in the fundamentals of negotiation and bluffing, which he sometimes made use of later at the higher levels of government and university administration, and always at the poker table. "It's good to over-pay, but when the claim is ridiculous, make it double or nothing on a number that you can win. If they claim 700 fireflies and it's clear that there are less than 300, offer to pay them for 400. If they do not agree, say that you will count them and pay for 800 if there are more than 400, and to pay nothing if there are fewer." After a few losses the word got around and the kids claimed more reasonable numbers.

The firefly collection operation had an enormous, positive impact on the perception of and interest in science nationwide. There were innumerable articles in newspapers and magazines describing the collection and explaining the importance of such research in basic science, even though significant applications were not so evident at the time (*Johns Hopkins Magazine,* 1952). Unfortunately, there were concurrent political efforts to ridicule basic science, for example, the "golden fleece" awards, promulgated by a member of the U.S. Congress, directed at deriding studies based on perhaps less appealing organisms, such as snakes. Years later, as director of the National Science Foundation, Mac strongly opposed such political intervention and was a strong and effective spokesman in supporting basic research in all areas.

Photometers were not commercially available in 1951, so based on Ted MacNicol's design, I built a photomultiplier photometer, which later became the first such instrument to be marketed. Baltimore's humid climate favored fireflies but not photomultipliers, so Mac agreed that I could buy a window air conditioner for my lab. These had just become generally available; indeed, this was the only air-conditioned room at the time in the biology department, probably in all of Johns Hopkins. So it came about that on certain very hot summer evenings my lab was reserved for a poker game hosted by Mac, with the dean of faculty, the provost, and a few other key players as guests. Thus the title of my salutatory address on the occasion of his honorary degree from Bologna was "Firefly Flashes and Royal Flushes: Life in a Full House" (Hastings, 1989). His was indeed a full life.

Later Mac transferred the poker game custom to Thursday nights in Woods Hole, where in 1956 he was appointed director of the renowned summer course in physiology at the Marine Biological Laboratory. His impact at the MBL was profound; he transformed the course, and it became the flagship offering, attracting many postdoctoral fellows and physicians for advanced training in research. He was elected a trustee of the MBL and participated actively in laboratory governance. The Thursday poker game still continues there, and derivative games are located in many places around the country. Veterans attest to Mac's uncanny ability to regularly finish the evening with sizable winnings, and he typically wore a cap with a green celluloid visor with Magic Marker writing, "Make checks payable to W. D. McElroy." Andrew Szent-Gyorgyi, a regular participant, also recalls that a bottle of champagne was transferred each week to the home of the host, so as to be prepared for a celebration if Mac should ever lose, which he eventually did, but only once according to legend. John Riina, Mac's editor at Prentice-Hall, says that "over 15 years, I recall only one game in which he was not a winner" and attributes to McElroy the observation that "the declared winnings always exceeded the sum of the reported losses." Riina characterized McElroy as "unique in many ways, he was interdisciplinary, thinking, acting and bringing together science, academia, government and society."

In 1948 John Lee Pratt, a trustee of the Johns Hopkins University and self-styled "plain dirt farmer" from Fredericksburg, Virginia (also, incidentally, a former vice-president of General Motors), donated $500,000 to establish a fund for the study of "micronutrients" in animal and plant nutrition. As recalled by Lawrence Grossman, who later occupied McCollum's chair, Pratt had served on a presidential committee during World War II with nutritionist Elmer McCollum, professor of biochemistry at the Johns

Hopkins School of Public Health, discoverer of vitamin A, and codiscoverer of vitamin D. Pratt mentioned that his cattle were suffering from "disease X," and McCollum suggested that this might be alleviated by adding small quantities of copper and molybdenum to the diet. This proved to be successful, probably triggering the idea for a gift; Hopkins President Bowman then appointed a committee, with both McElroy and McCollum as members, to recommend its implementation. Many on the committee were at a loss as to how a gift with such specific directives could be used (with McCollum retired there were no longer any Hopkins faculty working in the area of nutrition). Mac convinced the committee to establish a research institute associated with the biology department, and he volunteered to be its first director. Never mind that he had no special credentials or expertise in the area; good basic research, he argued, would inevitably lead to advances in an understanding of animal and plant nutrition. The success of the institute is well known; Mr. Pratt added another million to the fund in 1952, and upon his death in 1975, bequeathed $55 million to colleges and universities, including $5.5 million to Hopkins.

Who were McElroy's first appointments to the institute? Not nutritionists, but four of the best young biochemists in the country, now deceased: Sidney Colowick and Nate Kaplan, who later initiated the series "Methods in Enzymology" (Colowick and Kaplan, 1955), which is still going strong today, 50 years later, with over 380 volumes; Al Nason, a plant biochemist with whom McElroy later collaborated on effects of micronutrients on enzyme activity (1953); and Robert Ballentine, with whom he had authored the first paper on the possible involvement of ATP in luminescence. Over his career he engaged in collaborations with many other scientists, well exemplified by his work with Nelson

Leonard from the University of Illinois (Leonard, 1997), who recalls his "very pleasant and fruitful collaborations with Bill and Marlene."

McElroy was keen on the importance of communication; he wanted everybody to attend scientific meetings and to exchange results and ideas. In those years \$100 was equal to about \$1,000 today, and I recall him passing out \$100 bills to all graduate students who were planning to go to the Federation Meeting in Chicago in 1953. No accounting needed, but they could not submit a voucher and get more.

He was also keen on the importance of timely publication of the latest findings. He was one of the first to organize stand-alone topic-oriented symposia, doing so initially with funds from the McCollum-Pratt Institute on subjects closely related to vitamins and nutrition, in keeping with the institute's mission. Indeed, the first was on "disease X." Together with Bentley Glass, who wrote extended summaries that typically also clarified the obscure paper, they edited (in my air-conditioned lab) questions and answers recorded from the talks within weeks and had the volume published within months. Altogether there were nine such symposia, embracing such seemingly far-afield topics as heredity, development, and enzyme action, yet all were arguably, indeed evidently, of great fundamental importance for the progress of the mission of the institute. The last symposium, "Light and Life," included such topics as molecular structure and excited states, photosynthesis, vision, and, of course, bioluminescence (McElroy and Glass, 1961).

Believing that "the latest news" would greatly stimulate students and researchers, Mac was also very active in the promotion of publishing. He was very active on editorial boards of many professional journals and was a founding editor of *Biochemical and Biophysical Research Communications.* In 1958 he became the editor with Carl Swanson of

Prentice-Hall's "Foundations of Modern Biology" series, possibly the first paperback monographs that served as textbooks. John Riina, the Prentice-Hall editor with whom he worked, testifies to McElroy's leadership in identifying the best authors and convincing them to write. McElroy himself authored *Cellular Physiology and Biochemistry,* and with Carl Swanson provided the editorial direction for an outstanding list of books in the biological sciences.

McElroy was a true pro at obtaining funding for research. The National Science Foundation was started while I was in his laboratory, and I recall sitting with him for the 30 minutes or so that it took him to write his first proposal—for $10,000, I think. And it was successful!! In later years, as chairman of the biology department at Hopkins, he obtained some of the first grants from the NSF, National Institutes of Health, Office of Naval Research, and other agencies in support of term-time faculty salaries, relieving them of some teaching and allowing him to build a strong research department.

He was so imaginative and effective, one might infer, that the NSF decided that it would be wise to hire him as director, a post that he assumed in 1969. As the *New York Times* reported in its obituary (1999), he was the second choice, after an offer to Franklin Long of Cornell was withdrawn when it was learned that Long had opposed Nixon's proposed antiballistic missile system. McElroy was said to have been similarly opposed but was quiet about it, viewing government service as a way to promote science and to get increased funding for research. In this, and in his numerous other enterprises, and in his service to science and society, he was a leader and an achiever of the first rank.

William D. McElroy was survived by his wife, Olga Robles McElroy; his four children by his wife, Nell: Mary Elizabeth McElroy of Boston, Mass.; Ann Reed McElroy of Hickory, N.C.; Thomas Shipley McElroy of Glen Arm, Md.; William David McElroy, Jr., of Woods Hole, Mass.; and his son by his second wife, Marlene: Eric Gene McElroy of San Marcos, Calif. He also left a sister Lola Rector of Pismo Beach, Calif.; a nephew Mark Heinz; three grandchildren: Heather McElroy Holman, William D. McElroy III, and Michael James McElroy; and three great-grandchildren: Timothy Alexander Holman, Jr., Emily Madison Holman, and Nicholas Holman.

NOTE

1. As it happened, a business of isolating and selling ATP, stimulated by the firefly work, led to the founding of the Sigma Chemical Co. in St. Louis, subsequently and still now a premier biochemical supplier. This started as a side activity of Dan Broida's shoe polish company, and Broida became a good friend of many biochemists. In later years Sigma itself developed a network of children (some well over the age of 21) to collect fireflies, which it marketed to biochemists.

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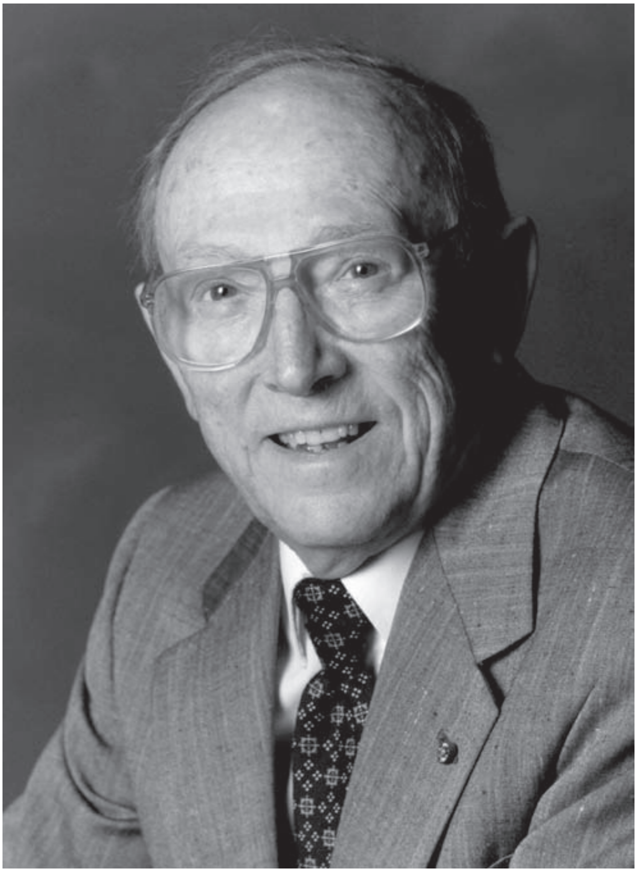

Edwin T. Mertz

EDWIN THEODORE MERTZ

December 6, 1909–February 1, 1999

BY JOHN E. HALVER

Ed Mertz was a professor emeritus in biochemistry at Purdue University and a member of Section 61 (Animal, Nutritional, and Applied Microbial Sciences) of the National Academy of Sciences. He is best remembered for codiscovering high-lysine corn, which dramatically increased available protein levels in the typical Central American corn and beans diet. He also developed the test for phenylketoneuria in newborn humans. He also coproduced a method to quickly and simply isolate pure native plasminogen from the plasma of practically any species. This is used to dissolve blood clots.

He was born on December 6, 1909, in Missoula, Montana, the son of a Lutheran minister, who was also a schoolteacher. His grandfather and an uncle were also Lutheran ministers. Both of Ed's parents were of German descent and spoke both German and English fluently. They were determined to stay in a university town so that their five children (Richard, Edwin, Art, Hildy, and Ethyl) could obtain an advanced education. Music was also important to them, and all five children were given piano lessons. Ed had piano lessons from ages 8 to 18 and excelled musically. The family had limited financial resources, as was typical of many families during the Depression. Ed had a paper route during grade school. During high school he became interested in chemistry. He was also the accompanist for the boy's glee club and took part in piano competitions. After high school graduation he attended the University of Montana. He paid his way through college and helped his parents financially by playing piano in a college dance orchestra. Sometimes the band would go out into the mountains, where a dance floor had been set up and a crowd was gathering. They would play until about one in the morning, and during intermission they would be served a delicious supper by ladies in the gathering. Then Ed would tape all his fingers, which were becoming sore. After supper the band would play until dawn and then head back to town with pockets full of silver dollars. His determination to succeed was coupled with a positive and happy attitude.

Mertz received his B.A. with a double major in chemistry and mathematics at the University of Montana in 1931; an M.Sc. at the University of Illinois at Urbana in 1933; and a Ph.D. at the University of Illinois in 1935. He was a research biochemist at Armour and Company in Chicago from 1935 to 1937. In 1937 he met and married Mary Ellen Ruskamp, with whom he spent 45 years. They had two children: Edwin T., Jr., and Martha Ellen (Marty). Ed was a devoted family man who is remembered as being gentle and full of fun.

He was an instructor in biochemistry at the University of Illinois in 1937-1938, working on the isolation of amino acids from animal sources, and a research associate in pathology at the University of Iowa medical school from 1938 to 1940, working on blood-clotting proteins under H. P. Smith. His first contribution to science came in 1939 with the development of a special buffer system, the imidazole system, for blood-clotting studies. This system soon became universally adopted by coagulationists and has been used extensively. He became an instructor in agricultural chemistry at the University of Missouri from 1940 to 1943. Too short for military duty during World War II, being about 5 feet and 4 inches tall, Ed worked as a research chemist in an explosives manufacturing factory, Hercules Powder Company in Wilmington, Delaware, from 1943 to 1946. This was dangerous work, as was demonstrated by the accidental explosive destruction of his research laboratory while working with Hercules Powder.

In 1946 he and his young family moved to West Lafayette, Indiana, where he became an assistant professor of agricultural chemistry at Purdue University (1946-1950), an associate professor of biochemistry (1950-1957), a professor of biochemistry from 1957, and then professor emeritus until 1999. For approximately 18 years he served as a consultant to the three Indiana state hospitals for the mentally retarded and helped staff biochemical research facilities in each hospital. He served on the U.S. Malnutrition Panels (1970-1973) and as a member of the Special Studies Section on Malnutrition of the National Institute of Allergy and Infectious Diseases.

His many honors included the Richard Newbury McCoy Award in 1967 from Purdue University; the John Scott Award in 1967 from the city of Philadelphia; the Hoblitzelle National Award in the Agricultural Sciences (Texas) in 1968; the Congressional Medal of the Federal Land Banks in 1968 for the discovery of high-lysine corn; the Kenneth A. Spencer award in 1970 from the Kansas section of the American Chemical Society for meritorious contributions to agricultural and food chemistry; the Osborne-Mendell Award in 1972 from the American Institute of Nutrition for outstanding basic research accomplishments in the science of nutrition; the Distinguished Service Award from the University of Montana in 1973; the Edward W. Browning Award in 1974 for outstanding contributions to mankind in the improvement of the food supply; and the Honorary Master Farmer Award of the Prairie Farmer Magazine for leadership and distinguished service to American Agriculture in 1975. He was elected to the National Academy of Sciences in 1975.

Mertz's diverse interests included amino acid requirements of humans, domestic animals, and fish; blood-clotting factors; and the biochemistry of mental retardation—all documented in his published research. This interest in amino acid requirements of human and monogastric animals led to an examination of the proteins of maize. Working with Nelson in the Agronomy Department at Purdue, he led ultimately to the identification of two maize mutants (*opaque-2* and *floury-2*) that had altered amino acid compositions of the coleorhiza seed proteins, specifically in higher contents of lysine and tryptophan. These limiting essential amino acids in maize for monogastric animals are doubled in the endosperm proteins of these two maizes. He and collaborators showed that this alteration had its basis in an expression in the amount of lysine and tryptophan, a low prolamine fraction, and compensatory over-synthesis of other proteins usually synthesized in smaller amounts. The greater content of lysine and tryptophan in the mutant seeds supports the growth of young monogastric animals at rates much superior to those achieved on normal maize when no other source of protein is present in the diets. These mutant strains of maize are increasingly used in Brazil, Colombia, and Central America to provide better protein sources for low-income populations.

> [F]eeding tests with undernourished children showed that opaque-2-maize could furnish protein of a quality equal in performance to that in milk at

one fifth the cost per pound of protein. The slight reduction in yield (10%) due to the opaque-2 gene is thus of little importance when compared with the large increase in the protein quality conferred on maize by this mutant gene. (*World Review of Nutrition and Dietetics* 48[1986]:222-262)

Dr. Mertz regarded this as one of the most important discoveries of his career.

He worked with William C. Rose at the University of Illinois on amino acid requirements of the rat, and later on phenylketonuria and brain development at the University of Iowa Medical School. His group perfected the strip test assay of ovarian fluid (now used nationally and internationally to detect phenylketonuria in the newborn human), which allows use of low-phenylalanine diets to prevent brain damage in these infants.

Ed improved Dunn's methods for L-amino acid assays of proteins from human and animal tissue and agricultural seeds and products. Later assays were incorporated into standard techniques in Moore-Stein amino acid analytical procedures.

He was the coordinator for amino acid requirement studies of the elderly and for the assay of amino acid losses involved. He established the reference standards for amino acid assay techniques for the "Big 10" Midwestern universities involved in the cooperative project. His graduate students monitored variance in assay techniques of the different laboratories on reference standards distributed periodically to the other universities, and these data established quality assurance for assays between laboratories.

Mertz was the author of a textbook on biochemistry and published over 100 scientific papers. Research in his lab was concentrated on high-lysine corn, high-lysine sorghum, development of high-lysine varieties of other cereal grains, and on the clot-dissolving enzyme fibrinolysin.

He liked to fish as a boy in Montana, and as an adult he would fish whenever time and location allowed. He often went with the crew during amino acid and tissue collecting at sea on the West Coast during the period he was conducting research in salmon with his graduate students who had been shipped to the Western Fish Nutrition Laboratory (Cook, Washington) for thesis research projects. One day on the Columbia River he caught a sturgeon as long as he was tall. He decided to ship it to his colleagues at Purdue University, to undermine the good-natured bragging of their angling stories. The sturgeon was shipped by rail and was iced several times during the trip. When Professor Roy Whistler opened the box at Purdue, the sturgeon when touched flipped his tail at Roy, and henceforth stymied further Whistler fish stories. Ed, though diminutive in physical stature, could hold his own in the fields of either sports or science.

In addition to his scientific interests, Ed was a classical pianist who relished Chopin and Beethoven. He began classical piano lessons at age eight. He was very talented and rapidly progressed until he could travel with his minister father on circuits to smaller churches, where he played the piano. He played all his life, teaching himself to improvise, and ultimately became a fan of Dixieland jazz.

He wrote a book *Harmony for Fun* for the piano. As soon as he arrived at Purdue he joined a jazz orchestra, named the Crusty Crumbs, made up of professors at Purdue University. They played for many years at civic and social functions, and after retirement he played Sunday dinner music at the Holiday Inn and at other restaurants for many years. His last gig was Mary Ellen's Tea Room in Dallas, Texas. His repertoire ranged from the classics to Joplin, and he really had fun playing for the crowd.

At Purdue, Ed made many friends with his peers in education. He was known as a gentleman and a good scientist. He loved people and was accepting of others. His daughter remembers that he never raised his voice to anyone, and had no false pride or vanity. He was quick to see humor, and he had a love of life, optimism, and a faith in the orderliness and logic of the Universe. He had many charities that were his special concern and was committed to training Ph.D. genetic nutritionists worldwide. One of his Ph.D. students, Ricardo Bressani, remembers Mertz as affable, understanding, and with a clear and intelligent vision and approach to research. Mertz treated his students with respect and understanding and helped them overcome the problems and difficulties most graduate students have. He began professor-student relationships on a strictly academic level, and over time through mutual research interests developed friendships. He provided advice, support, and encouragement when needed, even after the students had graduated from Purdue. He kept contact with many of his former Ph.D. students and visited some in their home countries.

His pursuits were his work, his family, and his music. He made sure to spend time in each of those areas, including extended family. Fond memories abound of him teaching his step-granddaughter to titrate in the kitchen when she did a science fair project on the nutritional value of carrots. During his later years Mertz spent summers with his son in Montana and winters with his daughter in Texas.

He died on February 1, 1999, from complications of pneumonia while visiting his daughter, Marty, in Richardson, Texas. Mertz was preceded in death by his first wife (of 45 years), Mary Ellen Ruskamp, and his second wife, Virginia Thomas Henry. His son, Edwin ("Ted") Jr., passed away in December 2002. Mertz is survived by his daughter, Martha Ellen West, and numerous relatives.

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Courtesy of Scripps Institution of Oceanography, UCSD

William A. Nierenberg

WILLIAM AARON NIERENBERG

February 13, 1919––September 10, 2000

BY CHARLES F. KENNEL, RICHARD S. LINDZEN, AND WALTER MUNK

Bill Nierenberg excelled in two scientific fields: physics, and oceanography. As a physicist, he worked on the Manhattan Project and contributed to molecular beam research and cascade theory. He helped to shape national policy in oceanography and to develop oceanography into a multidisciplinary, planetary science with a pivotal role to play in climate change research and earth science.

Bill Nierenberg was born February 13, 1919, in Manhattan to a family that lived on Houston Street in the Lower East Side and then moved up to the Bronx. His family was of Austro-Hungarian Jewish ancestry, and his first job was as a "floor boy" in the garment industry. The Bronx was near to his heart and still perceptible in his diction when he died 81 years later in La Jolla, California, after a long and distinguished career as a physicist and oceanographer. Late in his life Bill talked occasionally about how he made the transition from what we now call the South Bronx to California and gave great credit to Townsend Harris High School, where he was admitted by competitive examination in 1933. Townsend Harris was a citywide school for the gifted; it recognized and rewarded his prowess in mathematics, schooled him in physics, paid him small sums for grading papers, and prepared him for the City College of

New York. Bill knew he had a high IQ. Even his boyhood gang called him "the Brain." As a youth he was ambitious, competitive, and excited to be out and in the world; these characteristics stayed with him for life.

Bill had the advantage of growing up in a great city. He spent his free time at the Bronx Botanical Garden and developed an interest in science at the American Museum of Natural History. He went to high school with Herman Wouk and college with Bernard Feld, and he met Richard Feynman at an intercollegiate math contest. Physics was a small world then, and he quickly established himself at CCNY in a set that included Eugene Booth, William Havens, Jr., and teachers like Henry Semat, Mark Zemansky, and Walter Zinn. While CCNY was purely an undergraduate institution, students and faculty there participated in research at Columbia and New York University. Clark Williams took Bill to visit his lab at Columbia, and they became friends. The talk in physics at CCNY was all about the work of Enrico Fermi, I. I. Rabi, and John Dunning at Columbia. Bill first met Rabi in 1939, when he took his course in statistical mechanics.

Bill competed for and won many honors, medals, and prizes. He spent his junior year as the Aaron Naumberg fellow at the University of Paris, where he polished his physics and his French at the Sorbonne. His closest friend was a French classmate, Nicolas Zafiropoulo, who introduced him to new foods, music, and continental viewpoints. France broadened Bill's American outlook and made room for his big personality. France in 1938 was in a foreboding mood, however, and Bill went home expecting a European war.

Even during this period Bill was dismissive of the Left Wing at CCNY in the 1930s, and he was a committed antifascist. He expected to enter military service; naval aviation appealed to him, but his enlistment was delayed when, through Fermi and Dunning, he was offered an opportunity in 1941 for six months of war work in what turned out to be the Manhattan Project. Bill worked with Dunning and Clark Williams and had a role in the project, which he later said was closer to engineering than physics, but it placed him within the *haut monde* of physics and gave him opportunities and responsibilities unusual for a physicist who had just passed the qualifying examination for his doctorate. This work was cited when Bill was nominated for election to membership in the National Academy of Sciences.

His family responsibilities expanded at about the same time, when Bill married Edith Meyerson in 1941. Their daughter, Victoria, was born in New York, and their son, Nicolas Clark Eugene, at Berkeley. Nicolas was named in honor of Bill's French classmate, and his friends Clark Williams and Eugene Clark.

After Bill's graduation from CCNY in 1942, he was accepted at Columbia as a graduate student of I. I. Rabi and was received everywhere as a brilliant young physicist, although the acerbic Rabi told him he was too forward and brash. Bill listed Rabi first among those who influenced him, and Bill considered Rabi a great teacher, despite poor skills as a lecturer, because of the personal approach Rabi took with his students.

> He was always available to us in his office, singly or in groups of two or three, to work over some obscure or difficult point. He would spend several hours with us, if necessary. Some of the time, of course, was used to locate some reprint in the famous pile of papers on the table behind his desk.[1]

Rabi had a lifelong influence on Bill, but their relationship remained that of teacher and pupil, not an equal friendship. Rabi drew Bill into science advisory circles. Rabi was involved in the creation of the Hudson Labs at Dobbs Ferry, and Bill directed the labs in 1953-1954, his first contact with oceanography. Rabi introduced Bill to Alan Waterman and Manny Piore, then at the Office of Naval Research, and to the North Atlantic Treaty Organization science committee. Fred Seitz recommended that Bill succeed him in the position of assistant secretary general for scientific affairs at NATO in Paris from 1960 to 1962. Those years in Paris improved Bill's French accent and deepened his interest in French culture and literature. Bill's special interest in Turkey dates from these years. Rabi and Nierenberg were both interested in music, particularly opera, and Bill even briefly adopted Rabi's recipe for martinis: eight parts gin to three parts vermouth.

Bill also acknowledged the influence of his high school physics teacher, Ivan Hurlinger; the Sorbonne mathematician André Leon Lichnerowicz; and Maurice Biot and Enrico Fermi at Columbia. Bill wrote about Fermi in his unpublished autobiography.

> Fermi . . . was a most extraordinary lecturer on any branch of physics he chose. His most important series was his seminar on advanced nuclear physics that concentrated heavily on slow neutron phenomena. It was in these lectures that he demonstrated the utility of the scattering length and the virtue of his version of the Born approximation in scattering calculations that became known as the Golden Rule after the war among the graduate students. His most appealing feature was the revealing simplifications of what were normally displayed as extremely complex computations in the literature. A good example occurred in his course in geophysics that he had earlier given in Rome and then repeated at Columbia. This was a tremendous simplification of Jeffrey's treatment of the cooling of a spherical earth including the heating due to radioactivity.

Rabi got a National Research Council Fellowship for Bill in 1945, and Bill returned to his doctoral research as soon as the war ended. He reopened Sidney Millman's molecular beam laboratory and worked on an elucidation of the quadrupole broadened alkali resonances in the alkali halides. The committee for his orals included Rabi, Norman

Ramsey, Willis Lamb, and Hendryk Kramers. In 1948 Bill had a new Ph.D. and a letter of recommendation from Rabi.[2]

Nierenberg belongs to a small group of men who are capable both experimentally and theoretically. He is not a theorist in the sense of Nordsieck, but rather a man who gets a complete grasp of the theory of his field of experimental work and who can carry a problem right through to the end.

Bill received excellent offers from academic departments of physics but none from the place he wanted to go, Berkeley. Therefore, he went to Ann Arbor for two years and arrived in Berkeley during the summer of 1950 as an associate professor of physics. He planned to teach, work on the systematic measurement of the spins and magnetic moments of radioactive nuclei, and live near E. O. Lawrence on Tamalpais Road.

Bill contrasted American physics before and after World War II by comparing the work done by Rabi at Columbia with that of E. O. Lawrence at Berkeley. He said that Rabi did his great work with grants of a few hundred dollars from foundations and the loan of Navy electric submarine cells for magnet power supplies. This was "small" physics that concentrated on clean, spare problems that did not require complicated apparatus. Lawrence built huge and advanced physics laboratories by convincing the University of California and the federal government that research in physics strengthened the university and the country. Although Bill occasionally lamented the loss of community that resulted from postwar big physics, he agreed with Lawrence's vision.[3] In 1958 Bill was selected as the first E. O. Lawrence memorial lecturer by the National Academy of Sciences. In the 1980s, when some questioned whether funding for big science projects, like space science and the super accelerator, was justified when society had other pressing needs, Bill said he didn't understand the question. What he meant was that the commitment to science made by the United States after World War II was not merely a commitment of funds, it was a decision that American society would be knowledge-based with the expectation that research would build prosperity. Bill was, of course, being coy. He fully understood that the question itself marked a transition from the view that science was an essential part of the solution to society's problems to the view that science was simply another supplicant at the trough.

Bill started work at Berkeley by building a molecular beam apparatus, modeled on the one he had used at Columbia. His research included gaseous diffusion theory and experiment, cascade theory, atomic and molecular beams, the measurement of nuclear spins, magnetic moments, electric quadrupole moments, hyperfine anomalies with particular application to radioactive nuclei, and similar applications to atomic electronic ground states. He hoped to learn more about nuclear structure, and he became a leader in his field. He formed a group to measure spins and magnetic moments of radioactive nuclei, and over the course of his years at Berkeley he published a hundred papers in physics and trained 40 doctoral students. He developed an excellent reputation as a teacher. He established the atomic beam research group at Lawrence Radiation Laboratory. He worked with and admired Edwin McMillan and met Jerry Wiesner during these years. There were lots of parties and social interactions among the physicists in Berkeley. The McMillans introduced the Nierenbergs to Borrego Springs and encouraged them to explore the deserts of California and Mexico. Luiz Alvarez borrowed and played Bill's mandolin at faculty dinners.

When the physics department purchased an IBM 650 computer in the early 1960s, Bill taught himself how to program it with FORTRAN, and then taught FORTRAN to other members of the department. He was closely involved in the development of the applications of computers to nuclear physics and particle physics at Lawrence Radiation Lab. The short-lived radioactive nuclei were flown into his labs by helicopter for rapid measurement. One of the laboratory doors had a sign that read, "Every nucleus has its moment," and *Physics Today* published a poem on the laboratory wall,

Lament of an Ancient Beamist

There are moments to remember.
There are moments to forget.
There are moments to publish.
There are moments to regret.

Bill was responsible for the determination of more nuclear moments than any other single individual, as he was fond of telling visitors. This work was cited when Bill was elected to the National Academy of Sciences in 1971.

Bill built and flew model airplanes in Berkeley with his son, and Bill quickly moved to full-size aviation. He and his family purchased a vacation home in Borrego and he explored Mexico both from the air and on the ground. He was an avid traveler and a linguist. Bill and his family enjoyed their two years in Paris when Bill was on assignment for NATO. Bill also served as *professeur associé* at the University of Paris and traveled widely in Europe and the Middle East. His French was fluent; he became familiar with several European languages and began seriously studying Turkish.

While at Berkeley, Bill was recruited by Rabi and Piore to work on Project Michael, an Office of Naval Research effort to establish an academic base for use of long-range low-frequency sound in submarine detection. This led to the creation of the Hudson Labs at Dobbs Ferry, New York, and Nierenberg took a leave from Berkeley in 1953 to direct the lab for a year. While there he was responsible for the introduction of the concept of the vertical hydrophone array for the signal-to-noise improvement possible due to the special distribution of noise in the vertical plane in the deep oceans. He also made some contributions to anti-mine warfare. While in New York, Bill and his wife, Edith, attended the opera and theater and had an opportunity to see Jose Ferrer in the role of Cyrano de Bergerac at the New York City Repertory Theater. Bill adopted the French *physicien* as an alter ego, and researched and lectured on his life. He described his work on Cyrano as an obsession, but it was typical of Bill to pick a subject completely outside his academic interests and become an expert on it.

Low-energy nuclear physics and atomic beams was an exciting and promising field in physics in 1950, but by 1965, when Bill left the field, its promise was somewhat played out. Bill was interested in highly precise measurements, and these yielded some elegant clarifications, but they didn't produce new ideas. He told friends that he found the huge imbedded bureaucracy of physics objectionable and the process of writing lengthy proposals for research support debilitating. The Free Speech Movement had altered the social ambience of Berkeley, and stimulated Bill to become active politically. He was ready for a change. Ironically, he spent the next 21 years shepherding oceanography through a similar transition from small science to big science.

Bill formally became an oceanographer on July 1, 1965, when he assumed the directorship of the Scripps Institution of Oceanography. He was highly recommended by physicists and science administrators in Washington. Edwin McMillan praised Bill's intelligence and energy. Bob Frosch, who had succeeded Bill as director of the Hudson Labs, said he would enjoy working with him again. The only negative note came from Edward Teller, who complained that he could never get a word in edgewise in discussions with Bill at NATO.

Bill already knew many of the scientists in La Jolla. He had met Carl Eckart as a physicist in the 1940s. He had worked with John Isaacs on the Mine Warfare Committee. And he had long associations with the first faculty of the University of California, San Diego, including Harold Urey, whom he had first met at Columbia, and Keith Brueckner. Walter Munk and Bill had met as members of JASON, an independent group that advises the Department of Defense on scientific matters related to national security, which Bill chaired for six years.

Nevertheless, journalists often asked what a physicist was doing in oceanography. Bill had to explain that his naval connections dated back to 1947. He had served on the President's Science Advisory Panel on Antisubmarine Warfare from 1958 to 1960. He had conducted research on long-range low-frequency sound in submarine detection under contract to the Office of Naval Research at Berkeley. This gave him some familiarity with the field.

Scripps was one of the best-known centers for oceanography in the United States, and the first to offer a curriculum in the discipline. It had begun as a small private marine biological station, and then became part of the University of California in 1912, but it didn't become prominent until World War II, when researchers in La Jolla made very significant contributions to the war effort in the area of underwater sound, antisubmarine warfare, the development of methods of surf forecasting, and other research in support of amphibious and naval operations. During and immediately following the war it was virtually a Navy laboratory, but it gradually broadened its research interests and funding sources to emerge in the 1960s as a major center for geophysical research with a stellar faculty of biologists, geophysicists, and chemists. Its work contributed to the earth sciences revolution of plate tectonics, and its faculty had done some trailblazing work in geochemistry and atmospheric science. In particular, Walter Munk and Harry Hess had suggested a core-drilling program dubbed "Mohole" to answer key questions about the composition of the earth's mantle and the geological history of the planet. At Roger Revelle's initiative Charles David Keeling initiated measurements of atmospheric carbon dioxide in 1956 during the International Geophysical Year. These showed that carbon dioxide was building. Scientists began to speculate about possible environmental consequences. So Scripps was a famous and successful laboratory in 1965, but it was not a cohesive community.

There were a number of reasons for this. Bill arrived at La Jolla at a difficult moment. He succeeded a great and very popular oceanographer, Roger Revelle, who resigned when he was not named chancellor for the campus that he virtually founded, the University of California, San Diego (UCSD). The Scripps faculty was disappointed by Revelle's departure, exhausted by the effort of parenting a new general campus, fearful of being absorbed by UCSD, and divided into camps along disciplinary lines.

The student activism that Bill had already experienced at Berkeley was also evident at UCSD, and there was friction between the conservative La Jolla residents and the liberal academic community. The UCSD faculty was liberal, while Bill and Scripps were more conservative. Harold Urey had been a science advisor to the John F. Kennedy campaign, while Nierenberg supported Lyndon Johnson, because he considered Barry Goldwater reckless. Bill later supported and advised presidents Richard Nixon and Ronald Reagan. The 1960s were a difficult time in La Jolla. When student activists approached the Scripps campus to protest military-sponsored research, Bill had the campus police turn them away. The faculty at Scripps wanted a little peace and quiet, but Bill wanted action.

As director of Scripps, Bill planned a new initiative every year, but he started by trying to repair what he saw as shortcomings at the institution. His appointment as director included the rank of dean and vice-chancellor for marine sciences at UCSD, which helped to define the muddled relationship between Scripps and the general campus. Bill was amazed to find that computers were almost unknown on campus. A few pioneers had their own small computers at the Institute of Geophysics and Planetary Physics, but bathythermograph and other large datasets were still kept on computers at the University of California, Los Angeles. There was no central computer facility on the UCSD campus, and data was still recorded on Scripps ships, using paper and audiotape systems. Bill loaded IBM 1800's on the institution's largest ships, acquired a Prime computer for the Scripps campus, modernized the shore-based datacenters, and supported the creation of a supercomputer facility at UCSD. He streamlined the administrative and financial structure of Scripps, for the institution was expanding rapidly with the creation of the Deep Sea Drilling Project.

The Deep Sea Drilling Project (DSDP) rose like a phoenix from the idealistic but politically moribund Mohole Project. Scripps managed and housed the project from 1966 until 1986, under contract with the National Science Foundation for some $20 million. Bill negotiated the prime contract and oversaw the building of the drilling vessel *Glomar Challenger,* with its unique dynamic positioning technology. He fostered a strong science advisory structure and built the team that made the project operational. In doing so he pioneered a new type of scientific organization and guided the project from a national and institution-based effort to the first multi-institutional, international collaboration in science, a model for later projects from GEOSECS to ITER (international thermonuclear experimental reactor). The DSDP lived up to its objectives and fostered some of the major scientific advances of the twentieth century. Before DSDP most scientists thought hydrocarbons did not exist in the deep ocean basins, but they were found at the very first drilling site in the Gulf of Mexico. The Mediterranean was thought to be an ancient sea, but the DSDP found that it had been a closed basin and even a dry seabed in the past. The project verified that the present ocean basins were young and confirmed aspects of seafloor spreading and plate tectonics. The project greatly enhanced the prestige of the institution.

Bill knew how to capitalize on success, and he served as director during a fertile period. Plate tectonics and the environment took center stage in science in the 1960s and 1970s, and oceanography entered the mainstream of American science. Bill moved Scripps toward work in air-sea interaction and climate studies and established the remote sensing facility at Scripps, the first such facility at an oceanographic institution. Scripps acquired a DC-3 airplane for observations from above the sea, an acquisition that coincided with Bill's growing enthusiasm for flying his own plane. The climate program capitalized on Scripps's growing reputation in atmospheric science, which was based on the CO_2 work that had been done for years at Scripps by Charles David Keeling and others. The precise measurements done by Keeling were something that Nierenberg understood, and he relished the growing debate within the scientific and political worlds about the possible consequences of increasing atmospheric carbon dioxide and what, if anything, should be done. Nierenberg and Keeling held differing views about climate change, but they agreed about the necessity for continuous measurements. Keeling recalled with admiration the political skill Bill employed to ensure continued funding of the program at Scripps by the Department of Energy in 1981.[4]

Bill was director of Scripps for 21 years, the longest sitting director of the institution to date. During his tenure five vessels joined the research fleet and the institution's budget increased fivefold. Scripps scientists discovered the deep-sea hydrothermal vents. Bill worked to strengthen both the teaching and research programs at Scripps. He fostered international cooperation. For instance, with Saul Alvarez Borrego, Bill strengthened the relationship between Scripps and science institutions in Mexico, particularly with the two Baja marine institutions, the Escuela Superior de Ciencias Marinas of the Universidad Autonoma de Baja California, and the Centro de Investigaciones Cientifica y Educacion Superior de Ensenada. The interaction among these institutions strengthened them all, and Bill particularly enjoyed the soccer game that was a feature of the annual exchange visits. Bill retired from Scripps in 1986 but strongly continued his science advisory activities. When Charlie Kennel became director of Scripps in 1998, Bill initiated monthly lunch discussions with Charlie; Bill's purpose was to help his successor once removed to be scientifically rigorous in all his public interactions. These continued to within weeks of Bill's death and were much appreciated.

While fisheries had been a subject of great interest to the government of the United States since its founding, oceanography was rarely discussed in Congress before World War II. That changed beginning with the International Geophysical Year in 1956, and by 1969 the Stratton Commission recommended the creation of a new agency, the National Oceanic and Atmospheric Administration, and a new presidential advisory committee, the National Advisory Committee on Oceans and Atmosphere to oversee a national program in oceanography. Bill chaired NACOA from 1972 to 1977 and spoke forcefully in support of NOAA. This put him in close contact with legislators, and drew him into related matters of interest to Congress, including law of the sea and the earth observing system being promoted by the National Aeronautics and Space Administration. Bill served the White House during 1975-1976 as a member of the PSAC and during 1976-1978 as a member of the Office of Science and Technology Policy. He served on the NASA Advisory Council and was its first chairman from 1978 to 1982. However, he may be best remembered for influential reports he prepared on the Santa Barbara oil spill, acid rain,[5] and climate change.[6]

Bill delved seriously into scientific issues as the author of these reports, and never was this truer than his involvement with the climate change issue. His 1983 report *Changing Climate* was the first to introduce into public debate the concept of the "fingerprint" for detecting human-induced climate change, the possible release of methane hydrates because of warming, and carbon taxes. The *New York Times* covered the report on its front page, and Bill was proud that the newspaper published verbatim the report's executive summary, every word of which he had worried over.

For the remainder of his life Bill actively battled what he felt was exaggerated concern over the role of CO_2 in climate change. As the issue became politicized, Bill became identified with the political right, but Bill was always more idealistic than partisan. His priorities were the nation (he was patriotic to the core), science in both its methodology and institutions, and honesty and fairness. Given these priorities, he was often allied with conservatives, but his children—Victoria (who is liberal) and Nicolas (who is conservative)—both feel that Bill was supportive of their views. He was particularly proud of Victoria's contributions as an environmental consultant to the National Research Council report *The Policy Implications of Greenhouse Warming*. While working on the climate change report in 1983, Bill supported the participation of George Woodwell, a strong environmental advocate and activist, because George was concerned with the role of land processes in the CO_2 budget, a matter Bill felt was being underestimated by the marine geochemists. One of Bill's last e-mail messages to one of us (R.S.L.) was a reminder that a proper representation of climate feedback should also automatically eliminate climate drift in coupled models. This is a far deeper and subtler comment than one usually finds associated with this issue. The same e-mail message sought advice on purchasing a flat in Paris, something Bill had his heart set on.

Bill Nierenberg died of cancer at his home in La Jolla, California, on September 10, 2000. At the time of his death Bill was assembling a panel for the Marshall Institute in order to prepare a summary of the IPCC Third Assessment Report that would be more representative of the text itself. James Schlesinger eventually succeeded him in this effort, and the report was completed in 2001.[7]

Bill's family created the Nierenberg Prize for Science in the Public Interest in his honor. The Nierenberg Prize recognizes those who promote science in the public interest and reflects the mission of Scripps: to seek, teach, and communicate scientific understanding of the oceans, atmosphere, Earth, and other planets for the benefit of society and the environment. Bill would have enjoyed knowing that his prize was given to people of international reputation, like E. O. Wilson, Walter Cronkite, Jane Lubchenco, and Jane Goodall, who were also known to the layperson. The world of science will miss Bill's critical, perceptive, and supportive voice.

WE WOULD LIKE to thank Scripps Archivist Deborah Day for her critical and dedicated assistance with this memoir. Bill's correspondence and personal papers, including a brief autobiography completed shortly before his death, are at the Scripps Archives. These were invaluable in the preparation of this memoir. We would like to thank Jesse Ausubel, Edith Nierenberg, and Ken Watson for their comments and suggestions.

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Photo by Rutz, St. Moritz, Switzerland

C. L. Pekeris

CHAIM LEIB PEKERIS

June 15, 1908–February 24, 1993

BY FREEMAN GILBERT

CHAIM LEIB PEKERIS WAS born in Alytus A, Lithuania, on June 15, 1908. His father, Samuel, owned and operated a bakery. His mother, Haaya (née Rievel), was an intellectual and propelled Pekeris to excel. The family home was located at 4 Murkiness Street, near the corner with Ozapavitz Street. His older brother died at birth, a very sad event that led his parents to select the name Chaim, meaning life in Hebrew, for their second son. They added Leib, meaning life in Yiddish, for good measure. He was followed by four siblings, Rashka (Rachael), Jacob, Zavkeh (Arthur), and Typkeh (Tovah). The family name, Pekeris, is derived from the earlier name, Peker. Lithuanian authorities required the suffix, -as or -is, be added to the original name.

At a very early age Chaim exhibited his brilliance. By the age of 16 he was teaching mathematics at his high school and coaching 12-year-old boys how to prepare for the bar mitzvah. He was strongly encouraged to become a Talmudic scholar and a rabbi, but both he and his parents refused. By great good fortune one of Chaim's uncles, Max Baker, had emigrated from Lithuania to the United States and had settled in Springfield, Massachusetts. He was successful in the furniture business and was able to help Chaim and his two brothers to come to the United States for their continuing education. In addition, the prominent New England merchant and philanthropist, Edward Max Chase, came to the assistance of the Pekeris brothers. He, too, was an immigrant from Lithuania. He provided the young men with immigration affidavits, helped them find employment, and paid some of the tuition expenses.

The two brothers and Chaim, too, became U.S. citizens. Jacob, the older of the two younger brothers, became a teacher and spent the rest of his life in New England. He died at the young age of 31. Arthur became a specialist in agricultural products and settled in Denver, Colorado. Rachael, the older of the two sisters, became a Zionist and went to Israel (then Palestine) in 1935. Tovah, the younger of the two sisters was murdered by anti-Semitic townspeople in Alytus A, as were the parents, during the Holocaust. Furthermore, the family home was destroyed sometime during World War II.

Pekeris, as he was called by all but his most intimate friends, entered the Massachusetts Institute of Technology in 1925. At first he majored in mathematics but changed to meteorology and graduated with a B.Sc. degree in 1929. At that time meteorology was just being established at MIT, and it was a program administered in the Department of Aeronautical Engineering in the School of Engineering. Pekeris stayed at MIT for his graduate studies and became a student of Carl-Gustav Rossby. He graduated with his doctoral degree in 1933. It was the custom at MIT before World War II that doctoral graduates in the School of Science receive the Ph.D. degree and those in the School of Engineering receive the Sc.D. degree; thus, Pekeris received his Sc.D. The title of his Sc.D. thesis was "The Development and Present Status of the Theory of the Heat Balance in the Atmosphere." It was not published, but it was circulated in the form of informal class notes (MIT Meteorology Course No. 5, 1932).

During his time as a graduate student Pekeris won a Guggenheim Fellowship and studied in Oslo with some of the eminent meteorologists of the era. He was also an assistant meteorologist at MIT. Upon graduation he accepted an appointment as assistant geophysicist in the Department of Geology at MIT. In 1934 he published in meteorology but finished the year with a paper on the inverse boundary problem in seismology. For the rest of his career Pekeris seldom published on subjects in meteorology except classical fluid mechanics. Pekeris won a fellowship from the Rockefeller Foundation in 1934 and participated in academic travel and visits for the next two years. At this time Pekeris married Leah Kaplan. They had no children.

Louis B. Slichter joined the faculty at MIT just as Pekeris was completing his doctoral degree. Slichter had been a geophysical prospector in the 1920s but his company, Mason, Slichter and Gould, went bankrupt as prices for metals collapsed. Slichter had a Ph.D. in physics from the University of Wisconsin, but he returned to graduate school at the California Institute of Technology as a special student. After completing his studies in applied mathematics under the supervision of Harry Bateman, he joined the faculty of MIT to establish a program in geophysics. Pekeris was his first hire and was soon followed by Norman A. Haskell, a recent Ph.D. from Harvard. With these two appointments Slichter had established a strong program in geophysics at MIT.

For the second half of the decade of the 1930s Pekeris established his reputation in theoretical geophysics and made creative contributions to astrophysics and hydrodynamics. The debate about what was then called continental drift was in full bloom. R. A. Daly at Harvard opined that the mantle of Earth flowed at very long time scales. He persuaded N. A. Haskell to conduct a theoretical study of the uplift of Fennoscandia, a study that provided the first, and very accurate, estimate of the viscosity of the upper mantle. At the same time Pekeris published his study of thermal convection in the interior of Earth. Thus, well before World War II two young scientists had established part of the theoretical foundation of what would become the theory of plate tectonics.

In this period Pekeris published on oscillations of the atmosphere and of stars. This latter work was a precursor to his studies of the free oscillations of Earth some 20 years later. As the world entered World War II Pekeris began his work on the propagation of pulses and on the inverse problem of electrical resistivity. The former became important in the development of research in sonar and infrasound and the latter was, of course, commercially applicable as well as being intellectually stimulating. Pekeris's great productivity led to his promotion to associate geophysicist at MIT in 1936.

There is ample evidence that Pekeris was an avid reader of the literature on the propagation of pulses and waves; for example, he practically memorized the famous paper by Horace Lamb, "On the Propagation of Tremors over the Surface of an Elastic Solid" (*Philosophical Transactions of the Royal Society A* 203(1904):1-42). In 1939-1940 the signature card for that volume at the MIT Library was signed "C. L. Pekeris" every two weeks for several months. The paper led to Pekeris's work on synthetic seismograms in the 1950s and later. It is the key to his terse abstract in *Transactions of the American Geophysical Union* in 1941.

From 1941 to 1945 Pekeris was involved in military research, then unashamedly called war research. He worked as a member of the Division of War Research at the Hudson Laboratories of Columbia University, devoted to studies of acoustic pulse and wave propagation. In the years 1945-1950 he was the director of the Mathematical Physics Group. During that period he also had an appointment at the Institute for Advanced Study. Shortly after the war, Pekeris's wartime research was recognized by the U.S. Navy with the title of honorary admiral.

Following the war Pekeris's research productivity reverted to its former pace in both volume and quality. Yet science was not his only activity. In late 1945, after the end of the war and for a few years thereafter, there was a flood of surplus military equipment. In the United States and Canada supporters of the formation of a State of Israel, and Pekeris was one of them, formed a network of bankers, businessmen, farmers, shippers, engineers, and scientists. They would acquire the surplus equipment, move it to a "hachsharah" farm, recondition it, and ship it to a port near Palestine, such as Beirut, and deliver it to the Haganah units of the soon-to-be renamed Israel Defense Force. All of this completely clandestine work was done with seeming legality in every phase.

In the 1940s, as he reentered academic life following the war, Pekeris published several studies of microwave propagation and began his work on atomic physics. There are two publications that are preeminent. Both appeared in 1948. The first is the long paper on the propagation of explosive sound in shallow water. Pekeris considered the problem of a fluid layer overlying a fluid half-space of greater sound velocity. He provided a very thorough analysis of the normal modes of propagation, including a most penetrating study of dispersion. Later work on the propagation of dispersed seismic waves at both Columbia and at Caltech relied heavily on Pekeris's excellent memoir. That publication, Memoir 27 of the Geological Society of America, is one of the top two of his most cited papers.

The second paper, on the stability of flow in a circular pipe, was published in the *Proceedings of the National Academy of Sciences*. For decades it had been known from experimental studies that instability occurs at a Reynolds number of about 2000, but there was no quantitative explanation. Pekeris supplied the formal, quantitative derivation of the critical Reynolds number. This was a major contribution.

After the establishment of the State of Israel in 1948, the first President, Chaim Weizmann, asked Pekeris to come to the Weizmann Institute of Science and to be the founder of its Department of Applied Mathematics. Pekeris negotiated with the institute to build a digital computer for the new department. He then agreed to join the institute and moved to Rehovot in 1950. The computer was called WEIZAC (WEIZmann automatic computer).

The WEIZAC was built during 1954-1955. It is an early example of successful technology transfer, with the design of the von Neumann machine moving from the Institute for Advanced Study to the Weizmann Institute of Science (WIS) in Rehovot. WEIZAC's existence, its intense application to physical problems, and the cadres trained in digital hardware, software, and computational methods opened a market of concepts and practices outside the United States and Europe. The primary ostensible reason to build WEIZAC was to solve Laplace's tidal equations for Earth's oceans with realistic geographical boundaries. Pekeris insisted, however, that the entire scientific community of Israel, including the Defense Ministry, should have access to the WEIZAC.

One of Pekeris's first activities in Israel was the organization and management of Israel's first geophysical survey. This activity led to the discovery of oil near Heletz, northwest of the Negev. It also led to the establishment of the Institute of Petroleum Research and Geophysics.

Pekeris was constantly active in attracting graduate students to his department. He would visit other institutions of higher education in Israel, especially those having undergraduate studies, looking for bright students interested in higher degrees. He also toured centers for immigrants, bringing some of them to WIS to study for higher degrees, others to do alternative work such as becoming programmers. As a consequence of Pekeris's energetic activity, he and his department had a constant stream of excellent graduate students during his tenure as chairman.

Several of Pekeris's Ph.D. graduates became members of the faculties of Israeli universities, including WIS. Many stayed in geophysics or atomic physics and some entered other fields; for example, in the early 1960s Pekeris visited the Technion and met Amir Pnueli and persuaded him to do his Ph.D. at WIS on Earth's tides. Pnueli was more interested in compilers and other matters in computer science than tides and, with Pekeris approval and assistance, he went to Stanford as a postdoctoral fellow to work with Zohar Maneh. He is now a professor at WIS in the department that Pekeris founded. He received the Turing Award of the Association for Computing Machinery in 1996.

During his transition from the United States to Israel Pekeris published his first research note on the ground state of helium (1950). The foundation for this work was a series of papers in the late 1920s by E. A. Hylleraas who used a variational formulation for the energy levels. The wave functions are represented as a series of test functions or basis functions. The variational principal leads to a matrix of integrals of squares and cross-products of the basis functions and the eigenvalues of the matrix are upper bounds (by virtue of Rayleigh's principle) of the energy levels. The ground state is increasingly better approximated as the number of basis functions increases.

Pekeris was elected to the National Academy of Sciences in 1952. Shortly thereafter he visited the University of California, Los Angeles, where Louis Slichter had become director of the Institute of Geophysics. Just after World War II, Slichter and Rossby had tried to establish a geophysical institute at MIT, but administrative resistance led Rossby to go to the University of Chicago to found the Department of Geophysical Sciences. Slichter went to UCLA to become the first permanent director of the Institute of Geophysics. One of his first hires was Leon Knopoff, a recent Ph.D. in physics from Caltech, who had read Pekeris's 1941 extended abstract in the *Transactions of the American Geophysical Union*. Pekeris had claimed to have solved exactly the problem of the propagation of a seismic (SH) pulse near a boundary separating two media. Leon asked, "How did you do that?" Pekeris replied, "I don't remember." But he quickly refreshed his memory and published two pathway papers in 1955 on the propagation of seismic pulses. This work was to be the beginning of several papers and Ph.D. theses in theoretical seismology.

At the same time Pekeris returned to his early work on the nonradial oscillations of stars and began to extend that work to the free oscillations of Earth. Early work with Hans Jarosch and subsequent work with Zappora Alterman led to what has become the canonical formulation of the problem. After separation of variables, the ordinary differential equations are not combined into wave equations. Rather, they are left in first-order form. This is ideal for numerical calculations and allows the use of the matrix theory explicated by F. R. Gantmacher, in particular the theory of compound matrices. The great earthquake in Chile on May 22, 1960, soon gave Pekeris and his colleagues real data. No longer was the subject fit only for theoretical and computational seismology. It had become a major branch of observational seismology, too. Pekeris coined the term "terrestrial spectroscopy" for the new subject. The State of Israel issued a commemorative stamp.

As the 1950s drew to a close Pekeris and his students entered a new area of theoretical seismology, to compute synthetic seismograms for layered media by using generalized ray theory. There is some analogy with the "rainbow expansion" of B. Van der Pol and H. Bremmer. A formal expression of the wavefield can be done in the form of so-called generalized rays, each of which must be evaluated by methods stated in his 1941 abstract and more clearly developed in the *Proceedings of the National Academy of Sciences* papers (1955). Yet, in a fundamental way, it all goes back to H. Lamb in 1904. The use of generalized ray theory is computationally very intensive and the use of WEIZAC, and subsequently the more powerful GOLEM, was crucial to the success of the effort. WIS built GOLEM I in 1964 and GOLEM II in 1972. Golem is the Hebrew word for "shapeless man" and comes from a legend about Rabbi Loew in Prague in the sixteenth century, who is said to have made a golem from a lump of clay. In computer science the golem is a symbol for artificial intelligence.

In 1958 Pekeris published his fundamental paper on the ground state of two-electron atoms. That paper is the second of the top two of his most cited papers. The second major computational paper on the lower energy states of helium appeared in 1959. By using more than 1,000 basis functions in the variational formulation Pekeris was able to obtain very accurate upper bounds for the lower energy states. Experiments at Brookhaven National Laboratory confirmed the computational results. This work would occupy his interests from time to time for the rest of his life.

In the Cold War years many Jews were still living behind the Iron Curtain. Many of Pekeris's distant relatives were in this category, as were those of his colleagues. Exit visas could be obtained, but the cost was prohibitive for most would-be émigrés. Pekeris and his wife were untiringly selfless in helping many of these people to leave Eastern Europe and to come to Israel.

In the decade of the 1960s there were more papers about the lower energy states of helium and lithium and the beginnings of a series of papers on Laplace's tidal equations. Pekeris was the first to solve, numerically, these equations for the world's oceans (1969). Prior work had used unrealistic boundaries, such as meridians and parallels, in order to achieve results analytically. Pekeris used the analytical results to verify his codes and then moved on to the realistic boundaries of the major ocean basins. There were initial problems with some of the amphidromes (they went the wrong way), but the use of observed boundary conditions led to a more successful result.

By the early 1960s Pekeris had a superb international reputation in theoretical and computational geophysics. He was a major participant in the first meeting of the Committee on Geophysical Theory and Computers (in 1964 in Moscow and Leningrad) and was the host for the second meeting the following year in Rehovot. In addition to his major contributions to the study of Earth's tides, Pekeris had renewed his interest in geophysical inverse problems, and this subject was one of the themes of the meeting. The Committee on Geophysical Theory and Computers was sponsored by the International Union of Geodesy and Geophysics.

Pekeris took mandatory retirement at age 65 in 1973. By that time the Weizmann Institute of Science had established a Faculty of Mathematics and Computer Science that included the Department of Mathematics and the Department of Computer Science and Applied Mathematics. Pekeris was considered to be the founding father of that faculty.

As a widely respected senior scientist Pekeris began to receive formal recognition for his many contributions and accomplishments. He was elected a foreign member (*socio straniero*) of the Accademia Nazionale dei Lincei in 1972, and he received the Vetlesen Prize of Columbia University in 1974. Unfortunately, his wife, Leah, died in 1973, just as the awards and honors were beginning to occur. In 1980 the Royal Astronomical Society awarded him its Gold Medal and in 1981 the State of Israel granted him the Israel Prize.

By 1990 the Committee on Geophysical Theory and Computers had been renamed the Committee on Mathematical Geophysics, computers having become so ubiquitous. The meeting that year was held in Jerusalem to honor Pekeris for his many excellent contributions. At the closing banquet the speaker was Teddy Kollek, the mayor of Jerusalem. Kollek had been born in the Hungarian village of Nagyvaszony in 1911 and had visited Palestine in 1934. In the years just before the founding of the State of Israel he was active with the Haganah, just as Pekeris had been active in supplying that group. In his speech Kollek reviewed Pekeris's career, lauded him for his accomplishments both scientific and administrative, and closed with the remark, "I have told you a lot about Chaim Pekeris tonight and there is much more that I could tell, but you will understand that there are reasons that I can't. Let me simply say that Chaim Pekeris played a most significant role in the establishment of the State of Israel."

On February 25, 1993, Chaim Pekeris fell on the stairs of his home in Rehovot and died from the trauma of his injuries. He was hailed as a major figure in the physical sciences and in the history of the State of Israel. The following year the Faculty of Mathematics and Computer Science of the Weizmann Institute of Science established the Pekeris Memorial Lecture, which is presented annually.

THE MATERIAL in the memoir has come from many sources. The Web site (http://itzikowitz.20m.com/Index.html) maintained by Jacob Itzikowitz, a nephew of Pekeris's, has been informative. Discussions with Professor Walter Munk about C.-G. Rossby and about the stability of flow in pipes were helpful, as well. Samuel Itzikowitz, also a nephew, kindly provided the photograph. Professor Lee Segel of the Weizmann Institute of Science provided most of the bibliography. In particular, I am most grateful to Professor Flavian Abramovici of Tel Aviv University for assistance with family history, activity at WIS, and with the bibliography. My wife, Sarah, was very helpful with matters of grammar and style.

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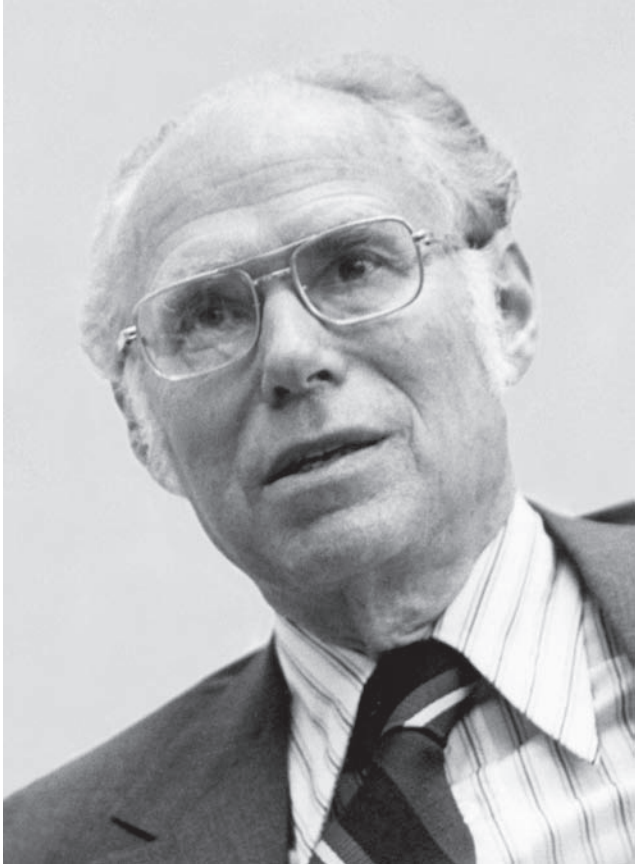

J. R. Pierce

JOHN ROBINSON PIERCE

March 27, 1910–April 2, 2002

BY EDWARD E. DAVID, JR., MAX V. MATHEWS, AND
A. MICHAEL NOLL

JOHN ROBINSON PIERCE is most renowned for being the father of communications satellites, namely, *Echo* and *Telstar*. He was also an active stimulator of innovative research in his division at Bell Labs from the mid-1950s to 1971. He was able to challenge and inspire many of the brightest researchers in communication science and technology, leading to a host of discoveries and innovations that created today's digital era. All who knew him were affected by his wit and quick, intelligent grasp of science and technology. He was a gifted author, not only of books that explained communication science and technology to nontechnicians but also of science fiction. His many keen comments are treasured memories of him that continue to inspire his many friends and colleagues. This wit led him to coin the term "transistor" for the device that his colleagues at Bell Labs had invented. We have all benefited from his innovativeness, intelligence, energy, and enthusiasm for communication science and technology.

John Robinson Pierce was born on March 27, 1910, in Des Moines, Iowa, an only child of John Starr Pierce and Harriet Ann Pierce. Although neither parent had gone beyond high school, they recognized their son's talents and worked to put him through the California Institute of Technology, where he earned his doctor of philosophy degree. Pierce spent most of his childhood in St. Paul, Minnesota. The family then moved in 1927 to Long Beach, California, where his parents worked in real estate sales, earning the money to pay for his education. They later moved to Pasadena so John could live at home to save money while attending Caltech and studying electrical engineering and physics.

During John's childhood, his father was frequently away from home for weeks at a time as a salesman. His mother had to cope with the mechanical problems of managing a household, which exposed John to all sorts of mechanical interests. "My mother encouraged me in all sorts of technical play," John said at one time, adding, "I was really my mother's child." Then, "As an only child with a certain amount of timidity, I led a somewhat sheltered life. I should have been learning more from other people and less from books." He clearly outgrew any timidity, eventually constructing and flying gliders until one of his acquaintances fell from such a machine and was killed. After that he ceased flying these homemade flyers. He quit because at the funeral of the friend, he thought about how many such funerals he had attended involving the glider community.

Reading excited him, at first science fiction and subsequently murder mysteries. The science fiction stories he wrote helped finance his education, and he would later state, "I wished that I could be a writer, but I thought it would be more practical to be an engineer." Even after he became one of the great research engineers at Bell Labs, he continued to enjoy writing, not only technical memoranda and books about communication but also science fiction under the pseudonym J. J. Coupling. He would later say, "I enjoy writing. . . . I also enjoy being known as the author." Clearly, writing was great fun for John. When he received the Marconi Award in 1979, he used the money to finance the writing of a book, *The Science of Musical Sound*.

Pierce was married three times. His first marriage, to Martha Peacock, the mother of his two children, John Jeremy Pierce and Elizabeth Anne Pierce, ended in a divorce in 1964 after 26 years. His second marriage, in 1964, was to Ellen Richter McKown, who died in 1986. Brenda Katharine Woodard, whom he married in 1987, survives him.

Upon graduation from the California Institute of Technology with a Ph.D. magna cum laude in 1936, John went to work at Bell Labs in its facility on West Street in New York City, where he performed research on vacuum tubes, particularly electron multiplier tubes and the reflex Klystron tube that was used in X-band radars during the Second World War. While at Bell Labs, John shared an apartment in New York City with Chuck Elmendorf (Charles Halsey Elmendorf III, later a vice-president of AT&T). They became fast friends over the next decades at Bell Labs and interchanged information and experiences.

In 1944 Pierce visited England, where he met Rudy Kompfner, inventor of the traveling-wave tube (TWT). Kompfner moved to Bell Labs in 1951, and they continued to perfect TWTs. While Kompfner saw the TWT chiefly as a low-noise amplifier, Pierce saw its application as a broadband amplifier. The Bell Labs' research organization and John moved from West Street to Murray Hill, New Jersey, in 1949, and John's work on TWTs continued until 1959.

As early as 1954 John had studied the practicality of using communications satellites to relay signals back and forth from Earth. In the summer of 1958 Pierce and Kompfner attended a summer study in Woods Hole, Massachusetts, sponsored by the Air Force. There they promoted the idea of a balloon satellite for communications, work that John would later say "had the most impact of anything I have ever done." A signal was to be sent to the satellite and bounced back to Earth. But Mervin Kelly, then president of Bell Labs, was not enthusiastic and refused to pursue it. His reasons involved the hostility of the U.S. Department of Justice and its aversion to the Bell System's "monopoly." Kelly retired in 1959, and his successor as president of Bell Labs, James Fisk, thought it was proper to proceed with the idea; *Echo* thus became reality. The *Echo* passive satellite was launched on August 12, 1960, and a message recorded by President Eisenhower was bounced off it. Pierce then went on to promote the idea for an active communications satellite, *Telstar*, which was to use transistors and a traveling-wave tube. However, the government then decreed that the Bell System, which was a regulated monopoly, should not work in satellite communications, just as Kelly had feared. (Kelly also foresaw the Justice Department's antitrust suit against the Bell System.) So *Telstar* was not deployed as a communications business. John would later state, "I took that hard . . . [but] I liked Bell Labs better than I liked satellites."

John, Claude E. Shannon, and Bernard M. Oliver described the idea of digital encoding of speech and other communication signals under the term "pulse code modulation" (PCM) and in 1948 published a paper entitled "The Philosophy of PCM" describing this technique in the *Proceedings of the Institute of Radio Engineers*. This paper and the ideas that led to and followed from it were the beginnings of today's digital era.

In 1952 John was made director of electronics research at Bell Labs, reporting to Harald Friis. John greatly admired Friis, who was very much his mentor at Bell Labs. Upon Friis's retirement, William O. Baker, then vice-president of research at Bell Labs, promoted John to executive director. Friis had formed a microwave laboratory in Holmdel, New Jersey, where the Bell System's highly successful long-distance microwave telephone transmission technology was developed. The microwave towers spaced about 30 miles apart throughout the entire country are still a visible reminder of this system. Kompfner took over the management of this laboratory, working under John.

John had a considerable affection for Bell Labs and a strong appreciation of the skills and talents represented there. The environment and mission of Bell Labs, which was to improve the performance of telecommunications across the world, profoundly influenced him. John always believed that any subject, no matter how complex, could be made understandable, and the creation of this clarity often required his skills and his ability to avoid becoming trapped in trivialities.

John spent over three decades of his professional life at Bell Labs. As executive director of communications research he reported directly to William O. Baker, the vice-president of research. John and Bill were a tremendous team, working together in a unique intellectual environment in which John could flourish, free from the bureaucratic intricacies that seem to grip so many organizations. Baker felt that Pierce's biggest contribution to Bell labs was "his ability to inspire and lead people." John retired from Bell Labs in 1971.

After retiring from Bell Labs, John joined the engineering faculty of Caltech, living in Pasadena in a stunning Japanese-style home with naturalistic pool and small waterfall. The layout was very graceful with *shoji* screens and sliding panels, but it lacked a private guestroom. John cured this deficiency by excavating a room under the house with his own hands. Nevertheless, after decades at Bell Labs, he found it hard to adapt to university life—raising research money and doing formal teaching, but he much enjoyed interacting with individual Caltech students.

He became emeritus at Caltech in 1980 and accepted the part-time post of chief technologist at the Jet Propulsion

Laboratory from 1980 to 1983, but his real interest in this last phase of his life turned to the technology of electronic and computer music. In 1983 he moved to Stanford as visiting professor of music associated with the Stanford Center for Computer Research in Music and Acoustics, CCRMA (pronounced "karma"). In 1987 Max Mathews joined him at CCRMA. They spent a wonderful decade working together until John's failing eyesight made computers inaccessible for him. In 2000 Parkinson's disease forced him to move to an assisted living facility.

John had a long-time interest in music. He studied the piano while a student at Caltech and later installed a pipe organ in his home near Bell Labs. John, Claude Shannon, and Shannon's wife, Betty, who was a pianist, carried out several ingenious experiments to estimate the information content of music. The results were interesting but not successful, and the essence of music continues to this day to elude quantification as information.

John and Mathews attended a piano concert in 1957, which included pieces by Schoenberg and Schnabel. They both felt that the Schoenberg was great and the Schnabel was horrible. During the concert, John said to Mathews, "Max, with the right program your equipment should be able to synthesize better music than this. Take some time and write a music program." This sojourn into computer music was possible because to facilitate research on speech coding, Mathews with Ed David and H. S. McDonald had recently developed equipment to put digitized sound into a computer and to recover processed sound from a stream of numbers generated by the computer. John's support and inspiration led Mathews to write a series of programs, "Music 1" through "Music 5," which started and set the course of present-day synthesized music. John, frustrated by his limitations as a pianist, took up the computer with great zest and composed about a dozen early pieces and exercises for the computer—more original compositions than anyone else.

AT&T administrators, when it came to their attention, were not enthusiastic about the public success of music programs. They asked for an explanation as to the appropriateness of the work in a telephone company laboratory. With the strong support of both John and Bill Baker, Mathews was able to show them how music synthesis grew directly out of vital speech compression research and how music synthesis techniques fed back useful technology to speech synthesis. Without the support and encouragement from John and Bill Baker, computer music would not have begun when, where, and how it did. Similar comments can be made about radio astronomy and the measurement of the 3° Kelvin background noise that supports the big bang theory of the beginning of the Universe. The measurement required Harold Friis's very-low-noise horn antenna at Holmdel, New Jersey.

During his decade at Stanford, John's interests focused on the perception of music. He created a new course in musical psychoacoustics He also invented a new musical scale based on a new chord, the 3:5:7 chord, which has many properties similar to the conventional major triad, the 4:5:6 chord. The 3:5:7 chord leads to a different harmony since its scale does not contain octave intervals (2:1).

In addition to his scientific contributions to music, Pierce was the most important patron of computer music. He attracted support for this field during its adolescence from 1970 through 1985. Without the funds he secured, computer music certainly would have progressed much more slowly and might not have survived.

John was a very social person. He was also very practical and efficient. He loved to write. Some of his books served multiple purposes. *Man's World of Sound*, written with David, is a good example. He and David had recently been given the task of managing speech research at Bell Labs, a domain new to both men. On a trip to attend a seminar on the subject in New York City they discussed their concerns. John said, "Ed, what do you know about speech and hearing?" David answered, "Very little." Pierce replied, "Then let's write a book about that." Ed concurred with enthusiasm. After the meeting, John called his editor; they went downtown and signed a book contract. The result was not only a fine book but a lifelong friendship.

Another example of an authorship, which served multiple purposes, was the rewrite of *Signals* with Noll. The original book, still useful for teaching, was out of print and needed revision. John also was glad to have a reason to work with Noll, a long-time friend whose work on computer graphics and arts John particularly admired. After agreeing to the collaboration, John, as he always did, crashed ahead as if to win a race with Noll to see who could write the quicker.

John was like an electron, a package of energy that seemed everywhere, yet was indefinable. His fast mind was quick to grasp concepts, and his energy was inexhaustible. He ran up and down stairs, always in a hurry. His speech seemed unable to catch up with the thoughts in his mind. He was very impatient, and would have little time for those who dallied or delayed the forward progress of science and technology. John always seemed restless, and this could make him seem forbidding in his dealings with people.

John certainly had strong views and a gift for summarizing these views in one-line statements. During a conference on the use of computers, including people from his division, much to John's disapproval, John dismissed the project saying, "What is not worth doing is not worth doing well." Another famous John one-liner was his dismissal of research into artificial intelligence, saying, "Artificial intelligence is mostly real stupidity."

John was always very modest. He had little patience with Washington and its bureaucracies, and never created a lucrative consulting business around himself. Asked why he did not do so, he responded, "I didn't promote myself."

JOHN R. PIERCE IN HIS OWN WORDS

On technical journals

I will say this of our multitude of technical journals, they beat the hell out of ideas mathematically and erect an awful lot of mathematics about things. And whether they really find out anything, I don't know. I will say that one of my criteria in life is that things have to be good enough. But after they're good enough, they get a little boring.[1]

On music

I like striking and effective music. I think that one of the troubles with avant-garde is that they don't know what else to do to be different.[1]

Electronically produced sounds should not be part of electronics; they should be a part of the evolution of musical sound, from drum, lyre, and Stradivarius to some of today's entirely new sounds.[5]

On information theory

Make no mistake. Information theory is not nonsense just because so much nonsense has been written about it.[4]

On communications satellites

Communications satellites were more important than I could have realized.[1]

On Bell Labs and administration in general

Doing things right is awfully important. But that wasn't my part of Bell Laboratories. My part was finding either new ways to do or rather drastically different ways of doing them.[1]

[I]n the university, no one can tell a professor what to do, on the one hand. But in any deep sense, nobody cares what he's doing, either. . . . But in the Bell Laboratories . . . research department . . . people cared about everything.[1]

[T]he Bell Labs, where I worked for 35 years, was the best industrial research laboratory in the world, and perhaps the best laboratory in the world.[2]

When I was Executive Director, the person who appeared at my door or who called me had precedence over anything else.[1]

On his life and creativity

I've really had a lot of good fortune in my life. But you'll never have good fortune unless you believe you're fortunate.

I've never been a good experimenter. I did a lot of tinkering.[1]

I've described myself as a low-grade theoretician.[1]

Night thoughts or dreams seldom solve problems correctly or definitively, however great the inspiration may seem at the time.[2]

My view of getting something new done was always that you started small with somebody who had done something real. With good luck, that would grow.[3]

Some problems are so difficult that they can't be solved in a hundred years, unless someone thinks about them for five minutes.

On universities

It takes a great deal of a lot of things to operate successfully on a university campus. If you really want to be successful, you have to set up a stream of graduate students and government support.[1]

On the application of science

Valid science is never old or out of date. It is only speculation about science, the "application" of science to philosophy, and false analogies between science and other matters that become old almost as soon as they are new.[6]

Surely, it is wonderful if a new idea contributes to the solution of a broad range of problems. But, first of all, to be worthy to notice a new idea must have some solid and clearly demonstrated value, however narrow that value may be.[7]

On knowledge and the future

Knowledge is hard learned. But, without knowledge, we can do no more than fantasize, which is childishly easy. The knowledge that can take us beyond fantasy requires an exercise of the mind, an exercise that can be as invigorating as exercise of the body.[4]

Whatever we may say of the future, it is open to us. That is, if we are knowledgeable enough to act, and if we leave ourselves free to act.[4]

I do feel sure that the future will be different, and I hope that it will be better. All of my experience tells me that the way to make it so is to work hard on present problems, with an eye always open for the unexpected.[6]

John Pierce was an extraordinary person with many skills and an awesome intellect. He contributed to the productivity of the many people, institutions, and corporations that came into contact with him. Above all, John Pierce was a person of strict integrity. He knew the difference between speculation, wishful thinking, and factual evidence. Pretense was not his way. This attitude permeated his life, his contributions to science and engineering, and his personal relations. We will not often see his kind again.

HONORARY DOCTORATES

1961	D.Eng., Newark College of Engineering
	D.Sc., Northwestern University
1963	D.Sc., Yale University
	D.Sc., Polytechnic Institute of Brooklyn
1964	E.D., Carnegie Institute of Technology
1965	D.Sc., Columbia University
1970	D.Sc., University of Nevada
1974	LL.D., University of Pennsylvania
	D.Eng., University of Bologna (Italy)
1978	D.Sc., University of Southern California

HONORS

1955	Elected to membership in the National Academy of Sciences
1960	Stuart Ballantine Medal (Franklin Institute)
1962	Elected to membership in the American Academy of Arts and Sciences
1963	National Medal of Science
	Edison Medal (IEEE)
1965	Elected to membership in the National Academy of Engineering
1974	John Scott Award (Franklin Institute)
	Marconi Fellowship Award
1977	Founder's Award (National Academy of Engineering)
1985	Japan Prize
1987	Arthur C. Clarke Award
1995	Charles Stark Draper Prize
2003	National Inventors Hall of Fame (posthumous)

NOTES

1. Interview conducted by Andy Goldstein on August 19-21, 1992. Interview no. 141, IEEE History Center, Rutgers University, New Brunswick, N.J.

2. John R. Pierce. *My Career as an Engineer: An Autobiographical Sketch*, September 22, 1985, written on receipt of the Japan Prize in 1985.

3. IEEE History Center Legacies. *John R. Pierce.* Available online at www.ieee.org/organizations/history_center/legacies/piercej.html.

4. John R. Pierce and A. Michael Noll. *Signals: The Science of Telecommunications.* New York: Scientific American Library, 1990.

5. John R. Pierce. *The Science of Musical Sound.* New York: Scientific American Books, 1983.

6. John R. Pierce. *Electrons, Waves and Messages.* Garden City, N.J.: Hanover House, 1956.

7. John R. Pierce. *Symbols, Signals and Noise: The Nature and Process of Communication.* New York: Harper, 1961.

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A collection of the papers of John R. Pierce are at the Huntington Library in San Marino, California, courtesy of the generosity of John R. Pierce, Brenda Pierce, and the American Heritage Center at the University of Wyoming. Pierce received 90 patents and published 300 research papers. The following are among the 20 books he wrote.

1956

Traveling Wave Tubes. New York: Van Nostrand.

1958

With E. E. David, Jr. *Man's World of Sound*. Garden City, N.J.: Hanover House.

1961

Symbols, Signals and Noise. New York: Harper and Rowe.

1964

Electrons and Waves and Messages. Anchor Books.

With A. G. Tressler. *The Research State: A History of Science in New Jersey*. New York: Van Nostrand.

1968

Science, Art and Communication. Clarkson N. Potter.

1980

With E. C. Posner. *Introduction to Communication Science and Systems*. New York: Plenum.

1981

Signals, The Telephone and Beyond. New York: Freeman.

1983

The Science of Musical Sound. New York: Scientific American Books (A second edition published by W. H. Freeman, New York, in 1992).

1984

With H. Inose. *Information Technology and Civilization*. New York: Freeman.

1990

With A. M. Noll. *Signals: The Science of Telecommunications*. New York: Scientific American Library.

JOHN H. REYNOLDS

April 3, 1923–November 4, 2000

BY P. BUFORD PRICE

JOHN REYNOLDS, A MAN of many parts but foremost a geophysicist, died of complications from pneumonia in Berkeley on November 4, 2000. The modern sciences of geochronology and nuclear cosmochronology grew in large part out of the work of Reynolds and his students. He was the first to detect isotopic anomalies, the study of which culminated in overwhelming evidence for preservation in the meteorites of micron-size grains of stellar origin. In 1960 he detected the xenon isotope of mass 129 trapped in meteorites, and from that discovery inferred that the extinct radioactive isotope iodine-129 (half-life 16 million years and probably generated in a presolar supernova) was present when the meteorites formed. This indicated that the meteorites appeared in the early history of the solar system. In later studies he and collaborators showed that other short-lived species were present in the cloud of gas that turned into our solar system 4.6 billion years ago. For decades he kept his laboratory in the forefront of the field of cosmochemistry. He will be remembered as the "father" of extinct radioactivities.

John was born in Cambridge, Massachusetts, on April 4, 1923. His father, Horace Mason Reynolds, was educated at Harvard, taught English in various colleges in the Boston area and at Brown University, and wrote for newspapers and magazines. His interests were the Irish literary renaissance and American folklore. His mother, Catherine Whitford, entered Wellesley College, but her education was interrupted by the death of her father in 1918. His parents met in Cambridge when she was secretary to Dr. Roger Lee, a physician who later became one of the members of the Harvard Corporation. His mother wrote articles for the *Christian Science Monitor's* Home Forum Page. Literary people often visited John's parents, and these contacts predisposed him toward the academic life. John lived in Cambridge for most of his boyhood, with occasional periods in Providence, Rhode Island, when his father taught at Brown and where his sister, Peggy, was born, and in Williamsburg, Virginia, when his father taught at William and Mary College.

The family was disrupted when his mother contracted tuberculosis and spent the years ~1930 to 1932 in a sanatorium in Charlottesville, Virginia. During that period his father continued his graduate studies at Harvard; his sister lived with her maternal grandmother in Westboro, Massachusetts; and John attended a small boarding school in South Sudbury. The family was reunited in 1932 in Cambridge, where John continued his education in the public schools. Although his strongest aptitudes were in math and physics, he enjoyed most subjects and took piano and organ lessons. At school and college he tinkered with electricity and radio, as did so many who later became physicists. He studied harmony in high school and college and sang in choirs and the college glee club. Much later, at Berkeley, he joined the Monks, who sang annually at the traditional holiday feast at the Faculty Club and at other affairs. At an early age he developed his lifelong interest in hiking and camping.

He entered Harvard in 1939. As an undergraduate he worked with his physics tutor, Leo Beranek, on his World War II defense research project in electroacoustics. After graduating summa cum laude in 1943 with an A.B. degree in electronic physics, he was commissioned a Navy ensign and entered active service on June 28, 1943, as an ordnance officer; he worked on an antisubmarine project at island bases in the South Pacific. He was honorably discharged as a lieutenant on June 11, 1946.

Inspired by his reading about the Manhattan Project, John decided to do his graduate physics studies at the University of Chicago. His selections of mass spectroscopy as a topic and of Mark Inghram as a thesis advisor were based mainly on a friendship with Joseph Hayden, who was doing research with Inghram at the time. John was captivated by the enthusiasm of the stellar roster of geochemists and cosmochemists: Harold Urey, Harrison Brown, Hans Suess, and the relative youngsters Clair Patterson, George Tilton, and Sam Epstein. Gerry Wasserburg and George Wetherill were graduate students there a bit later. Like the other Chicago physics graduate students, John was strongly influenced by Enrico Fermi, and he audited two of Fermi's courses, never missing a lecture. Much later in life, while on sabbatical in Western Australia, he gave a talk to undergraduate students on how to make back-of-the-envelope estimates, using as examples some of the problems Fermi gave students. The most famous of Fermi's questions was, "How many piano tuners are listed in the Chicago telephone directory?" None of the students in John's lecture had any idea how to estimate the number, until John led them through the reasoning and arrived at a solution that was correct to better than 50 percent. At Chicago he and Inghram discovered the double beta-decay of ^{130}Te by way of ^{130}Xe production in tellurium ores. The topic remains one of great interest, in view of the possibility that the decay might occur without neutrino emission, which would violate lepton number conservation. Also at Chicago John discovered ^{81}Kr, the long-lived isotope of krypton, which later became the basis of the most precise cosmic-ray exposure-dating method for meteorites and lunar rocks. Nineteen-fifty was an exciting year for John. He completed his Ph.D. thesis, married Genevieve Marshall, took on a short-term appointment as associate physicist at Argonne National Laboratory, and accepted an assistant professorship and moved to Berkeley.

In 1950, by virtue of Lawrence's cyclotron and the presence of a number of outstanding nuclear physicists such as Alvarez, Segré, and Chamberlain (all future Nobel laureates), the Physics Department at Berkeley had been able to hire the pick of the crop of faculty trained in high-energy nuclear physics, and the department had become unbalanced. It was clear that a serious effort would have to be made to hire faculty members in other fields. On an extensive recruiting trip Francis ("Pan") Jenkins interviewed over a hundred students across the country and produced an ordered list of his top choices for the faculty to consider. While interviewing at Chicago, Jenkins was influenced by suggestions by Francis Turner and John Verhoogen of the Berkeley Geology Department that a physicist skilled in isotope spectroscopy would be a useful adjunct. In the days long before official advertisements and affirmative action became the required mode for all faculty hiring, Chair Raymond Birge simply talked with the faculty and with their endorsement, requested the dean for the position. "With our present enormous number of graduate students, it is imperative that there be more fields of research and more instructors under whom graduate students may work. . . . The department has agreed to look for no new men in the high energy nuclear physics field, because of this need for broadening our offerings." Within the same year or so the Physics Department hired five new faculty members who would later be elected to the National Academy of Sciences:

John Reynolds, Walter Knight, Erwin Hahn, Carson Jeffries, and Bill Nierenberg.

The university provided funds for John to set up his own laboratory for mass spectrometry. Two years later he requested funds from the Office of Naval Research for "studies by rare gas mass spectrometry of reactions of transmutation," specifically to study transmutation of iodine into isotopes of xenon by deuterons; to search for absorption of solar neutrinos in bromine and its transmutation into krypton; and to increase the sensitivity for K-Ar geochronology so that rocks containing only a moderate potassium concentration could be dated. These funds enabled him to design and construct the first static (nonpumped) all-glass mass spectrometer, which he used for isotopic analysis of the noble gases. Others have since referred to it as the Reynolds-type mass spectrometer. A key ingredient in that instrument was his incorporation of a bakeable ultrahigh-vacuum system, which had just been invented by Daniel Alpert, and without which he could not have achieved the sensitivity he sought. Prior to his development, analyses were made dynamically, with the sample leaked through the ion source and analyzed with very low efficiency en route to the vacuum pumps.

The idea to use the decay of long-lived potassium-40 into argon-40 as a dating tool can be traced back to C. F. von Weizsäcker in 1937. Because of the complicated decay scheme of ^{40}K, its poorly known half-life, questions concerning its retention in rocks, and the inadequacy of mass spectrometers, the method developed slowly, and no one scientist can be said to have invented the K-Ar technique. By the time Reynolds appeared on the scene, it was being used in laboratories around the world to date 10^8- to 10^9-year-old potassium-rich rocks. In 1956, by virtue of the factor 10^2 higher sensitivity afforded by his static mass spectrometer, he and graduate student Joe Lipson in Physics, together with Garniss Curtis and Jack Evernden in Geology, were able to date rocks as young as ~10^6 years. This capability opened the door for Richard Doell, Allan Cox, and Brent Dalrymple, who had been trained at Berkeley and were then at the U.S. Geological Survey in Menlo Park, to determine the timescale for geomagnetic reversals. Applying that timescale to the stripes of alternating paleomagnetic polarity in lavas as a function of distance from a mid-ocean ridge, they showed that lava ages increased in both directions from zero at the ridge, from which a seafloor spreading rate of a few cm/yr could be inferred. This provided a quantitative proof of plate tectonics. Another important application of the Reynolds spectrometer was to hominid anthropology. In 1963 Professor Richard Hay (Geology, Berkeley) used it to date the volcanic ash layers at Olduvai Gorge, from which he was able to fix closely the fossil sequence of man's prehistory.

The first of John's many sabbatical leaves was in 1956-1957 as a Guggenheim fellow at the University of Bristol, England, in Cecil Powell's cosmic-ray physics group. Although this stay did not tempt him to shift his interest away from geochronology, it did inspire in him a love of England, to which he and his wife, Ann, returned many times in later life.

In 1947 Harrison Brown had suggested that meteorites could be used to determine quite accurately the age of the elements if the daughter of an extinct natural radioactive nuclide could be found there. What one would actually measure would be the time delay between nucleosynthesis of elements in the solar system and the freezing-in of long-lived radioactive nuclides in solar system bodies. A number of leading mass spectrometrists, including Gerry Wasserburg, had hoped to be first to detect ^{129}Xe, the decay product of ^{129}I, an isotope with a 16-million-year half-life.

John's discovery of extinct ^{129}I in 1960 was the crowning achievement of his career. His promotion to full professor that same year was a shoo-in. Letters in support of the promotion were glowing: "His work on meteorites . . . has revolutionized much of cosmological theory. His latest result is the most important single event in the whole field" (Willard Libby). "Reynolds has made an exceedingly important discovery, namely that there is a variation in the abundance of the isotopes of xenon in meteorites. The nature of this variation is two-fold: first, there is a special anomaly due to the decay of iodine-129 which shows that the meteorites were formed within a couple of hundred million years after the last important synthesis of the elements; and second, there is a general anomaly which indicates that nuclear processes of some kind were different for the meteorites than they were for the material of the Earth. . . . I regard this as a very important discovery" (Harold Urey). "One can point to one particular accomplishment in his investigation of the xenon content of meteorites. The isotopic composition of xenon has led to most striking conclusions concerning the conditions under which our planetary system must have formed" (Edward Teller).

It is worthwhile to ask how one professor, without graduate students, could make a discovery of that magnitude. Before 1960 little was known about the age and time interval for formation of the solid bodies of the solar system except that the Earth was 4.6 billion years old, as measured with the uranium-lead technique by Clair Patterson, Harrison Brown, George Tilton, and Mark Inghram in 1953. Thanks to the prediction by Harrison Brown, John (as well as other geoscientists) knew exactly what to look for in order to estimate the age (i.e., the time since nucleosynthesis) of the elements that made up the solar system. A key ingredient in his success was his static all-glass mass spectrometer, which made it possible to pass the same rare gas atoms through the instrument many times in search of an isotopic anomaly. Thanks to fully funded leaves in residence, funded by Berkeley's Miller Institute, he was able to devote full time to the search. Furthermore, as an assistant professor he was not burdened with the numerous service responsibilities with which senior faculty are saddled. It was partly good fortune that so many chondritic meteorites contain xenon-129 up to 50 percent in excess of the atmospheric xenon-129 concentration. His observation of the large excess of ^{129}Xe in the Richardton chondrite, memorialized in introductory physics textbooks, constituted the "home run" that led to his election to the National Academy of Sciences eight years later.

Shortly after John's discovery of excess ^{129}Xe, Bob Walker, Bob Fleischer, and I at General Electric Research Laboratory were discovering the multifarious uses of nuclear tracks in solids, including fission track dating, and we visited John, as did the many others who regarded his lab as Mecca. During my visit in 1962, I remember the stir created when John brought me to the Faculty Club for lunch. The geologists hollered for us to come to their table, and later Jack Evernden drove me to the helicopter pad for my shuttle to the airport.

In preparation for his second sabbatical, 1963-1964, John studied Portuguese for a year and obtained both a National Science Foundation senior postdoctoral fellowship and NSF funding to set up a complete K-Ar laboratory at the University of Sao Paolo, Brazil. In preparation Professor Umberto Cordani of that university spent six months learning mass spectrometry at John's Berkeley laboratory. Cordani wrote: "All of us South American geochronologists will be forever indebted to John Reynolds. It was his idea, back in the late fifties, to set up what was to be the first geochronology laboratory on our continent. We valued his conveyance of ethics, respect, and humility toward knowledge and science." During his stay, the group measured ages of Brazilian rocks that fitted with age patterns seen along the coast of Africa. The agreement in ages supported the theory of continental drift. An important visual clue that South America was once part of Africa is the jigsaw puzzle-like fit of their coastlines, and the match in ages of coastal rocks provided strong evidence that South America separated from Africa some 10^8 years ago. While in Sao Paolo, John stimulated interactions with colleagues of other Brazilian institutions and from neighboring countries.

John was proud of the training he gave his students, and the freedom he gave students and postdocs to try their own ideas. While he was in Brazil, his students Grenville Turner and Craig Merrihue, working in John's laboratory, discovered the ^{39}Ar-^{40}Ar method, which has since become the most important and most versatile dating method. Their idea was to irradiate the rock sample with neutrons in order to transmute some of the stable isotope ^{39}K into ^{39}Ar. Analysis of the two Ar isotopes thus gave both the potassium content and the radiogenic argon. The idea of using neutron irradiation traces directly back to John's earlier use, with Peter Jeffery, of neutron activation of ^{127}I to produce stable ^{128}Xe, from which a correlation can be made between radiogenic ^{129}Xe and ^{127}I. Knowing the initial ratio of ^{127}I/^{129}I produced in nucleosynthesis leads to the I-Xe dating method.

John was well aware that most of the Physics faculty felt that his research was far afield from mainstream physics. For example, Luis Alvarez once wrote in support of a merit increase for John: "I remember wondering when John Reynolds first came to Berkeley why any bright young physicist would want to work in the field of mass spectroscopy, which I considered to be a rather dead field at that time.

But in the hands of John and several other innovative young physicists, the field came back to life, and it has been a very exciting and productive branch of science in the past 10 or 15 years." Despite his success in single-handedly creating a new field, John longed for another Physics faculty member with interests in geophysics. After a failed campaign to move Bob Walker to Berkeley, in 1969 he succeeded, to my delight, in getting me an appointment. For a few years while we were studying lunar samples and I was searching for fossil spontaneous fission tracks of superheavy elements ($Z \approx 110$) in meteorites and lunar rocks, we ran a joint seminar; but I moved gradually into astrophysics and started a separate weekly seminar. John then got a faculty appointment for Rich Muller, who had switched from high-energy physics into geophysics, but his and Rich's researches never converged.

For his next sabbatical year, 1971-1972, with the award of a Fulbright Fellowship, he decided to refresh his knowledge of Portuguese by setting up a mass spectrometric laboratory to do K-Ar dating of rocks at the University of Coimbra, Portugal. In recognition of his success in establishing geochronology there, in 1987 the University of Coimbra awarded John the degree of doctor, *honoris causa*. He brushed up on his Portuguese and gave his acceptance speech in that language.

Xenon is particularly rich in stable isotopes that provide cosmochemical information. For example, a second extinct radioactivity amenable to a mass spectrometric search was the spontaneous fission of ^{244}Pu with an 82-million-year half-life. It was certain that fragments of its fission should include ^{136}Xe, ^{134}Xe, ^{132}Xe, and ^{131}Xe, but the relative amounts were not known. A number of geoscientists had found reproducible excesses of those isotopes in uranium- and rare-earth-rich minerals in meteorites, but the case for a ^{244}Pu origin was not airtight. In a beautiful experiment on ^{244}Pu artificially produced at Oak Ridge, in 1971 Reynolds and his colleagues conclusively demonstrated that the relative abundances of the heavy xenon isotopes resulting from spontaneous fission of ^{244}Pu agreed with those found in various meteorites. From the ^{129}I and ^{244}Pu results they concluded that the meteorites formed within ~10^7 years of each other, some 10^8 years after cessation of nucleosynthesis of the material that formed the solar system. Excess heavy xenon isotopes in certain carbonaceous chondrites with an apparent fissiogenic origin different from ^{244}Pu were thought by some to be due to spontaneous fission of an unknown superheavy element, but that intriguing possibility has never been confirmed.

In his typical fashion, when he agreed to be joint organizer of a U.S.-Japan symposium on cosmochemistry at Hakone National Park in 1977, he began preparing a year in advance by auditing a course in Japanese. By the time of the symposium, he could read comic books in Japanese and write in Katakana.

With support from a National Science Foundation Cooperative Research Award, John spent a sabbatical year (1978-1979) at the University of Western Australia, Perth, where he collaborated with Peter Jeffery in the development of a miniaturized mass spectrometer. With this instrument they were able to study excesses of certain xenon isotopes in microgram-size mineral grains in the Allende carbonaceous chondrite and to add to the momentum of John's Berkeley group in the competition to decipher the message in the micronuggets carrying the anomalous gas in carbonaceous chondrites. He did this by developing a small tungsten conical heating filament and installing the then-new Baur-Signer ion source and an ion-counting system. This new technique allowed the isotopic measurement of as few as ~10^4 atoms of xenon to be determined. Besides his research activities John enjoyed early morning jogging and 1-mile ocean swims seaward of the reef at Cotteloe Beach. I recall him telling me how surprised he was when he encountered Prince Charles, who happened to be enjoying a swim along the same stretch of ocean.

John's next big project, which occupied him for the better part of the decade 1978-1988, was to design, construct, and do research with a new type of mass spectrometer, which along with electronics and computing facilities, was carried in a 25-foot trailer. I remember seeing their trailer, painted with the giant acronym RARGA (for Roving Automated Rare Gas Analyzer) parked year after year between two of the Physics buildings (Birge and LeConte halls). With this facility John and his group were able to do precise in situ measurements of isotopes of rare gases from deep terrestrial fluids, mainly geothermal. The fluids included primordial gas from the Earth's mantle, radiogenic gases from the crust, gases that were products of nuclear reactions induced by neutrons and alpha particles from radioactivity, and recirculated atmospheric gases. The primordial gases, which were the most interesting because they dated back to the formation of the solar system, were excesses of ^{3}He and ^{129}Xe from decay of extinct ^{129}I. Among the sites from which they collected samples were geothermal areas within Yellowstone Park. For the culmination of the RARGA project John was awarded a second Guggenheim Fellowship in his 1987-1988 sabbatical year. With the trailer parked on the perimeter of Los Alamos National Laboratory, he and his students studied gases from various geothermal sources, mainly in New Mexico, with his wife, Ann, cooking for the entire group. Eventually, after nearly a decade, Berkeley bureaucracy caught up with John. He was told that RARGA was parked in violation of the campus's policy on temporary buildings, and he was required to move it to another temporary location for a three-year period, after which it had to be permanently moved off campus.

In parallel with the RARGA project he and his group followed up on an important 1975 discovery by Chicago geochemist Ed Anders and coworkers (which included former students of John's) of traces of gases with exotic isotopic composition that had been trapped in tiny extrasolar grains in the most primitive carbonaceous meteorites. The Chicago group was able to isolate the sources of the exotic rare gases by dissolving mineral grains in strong acids and then extracting almost all the exotic gases from the 0.5 percent remaining solid matter. They proposed a spectacular explanation for the heavier xenon isotopes: that they were the products of spontaneous fission of some extinct superheavy element. John and his students found that the trapped rare gases were predominantly associated with the carbon in the meteorite. They offered a less bizarre but still very exciting explanation: that the exotic gases had been trapped in presolar grains with a different nucleosynthetic history from the gases in our solar system. The study of such grains, made in stellar atmospheres and later incorporated into the material of which our meteorites were formed, is on the forefront of cosmochemistry today.

Soon after John arrived at Berkeley, the president of the university appointed a number of volunteers as faculty fellows, with the goal of improving contacts between undergraduate students and faculty. John enjoyed serving as a fellow at Bowles Hall, one of the university residence halls. I remember visiting him in 1966 and joining the students at lunch, where John represented me as an expert who could answer any of their questions on physics. When I could not give a satisfactory answer to one of their questions, I realized that I had better move from the ivory tower of industrial research to the real world of involvement with students.

As often as his other duties would permit he swam at the pool and basked in the sun and conversed afterwards with the regulars. He was a member of Little Thinkers, a diverse group of faculty who met monthly for serious conversation at the Faculty Club.

His societal concerns extended beyond the walls of academia. He was a staunch believer in representative democracy and ran for the Berkeley Rent Board out of a sense of civic duty. He was also active in the League of Women Voters. For an example of his political orientation as well as his felicity in written expression, I quote from his contribution to the Harvard twenty-fifth yearbook in 1968: "Berkeley has recently been a focal point in this kind of conflict [the Free Speech Movement], because there the University is especially good and the public is especially bad (at least I am forced so to conclude)—the latest important acts of the electorate have been to strike down a 'fair housing' enactment of the state legislature and to install a movie actor in the high office of Governor."

John played an active part in the Cal Sailing Club. As an assistant professor with a modest income he shared ownership of a boat with two other professors. Later he acquired a much grander boat of his own. Shortly after I joined the faculty, he and his second wife invited me and my wife to sail with them to Tiburon and have a picnic lunch there. Unfortunately, we were becalmed in the middle of the San Francisco Bay. Unfazed, they brought out the champagne and lunch, and we had a pleasant time on the bay before the wind returned.

In 1989 he partly fulfilled his dream of returning under sail to the South Pacific where he was stationed during World War II. As a crew member aboard a sailboat in the California to Hawaii Transpac Race, he sailed from Long Beach to Honolulu. After his retirement he cruised extensively with

Ann in their 30-foot sailboat. They made summer cruises to Alaska as well as a two-year passage through the Great Lakes and New England, during which time he stopped whenever possible to send his many friends detailed e-mail messages recounting their adventures. As a reward for her putting up with long periods of life in relatively primitive quarters, he would take her on long visits to England. He had just completed his second round-trip sailing voyage to Alaska several weeks before his untimely passing.

John took the responsibility of service to the university seriously and served as chairman of the Physics Department in the 1980s. During this time, he made several changes that benefited the department in lasting ways and promoted a feeling of harmony and cohesiveness among the faculty. He believed strongly in U.C. Berkeley's tradition of faculty governance, and he encouraged his colleagues on the Physics faculty to participate in meetings and committees of the Academic Senate.

He was president of the Faculty Club in 1968-1970 and served on the board in 1996-2000. He enjoyed singing at the Club with the Monks. During his final hospital stay he was concerned that the Club piano be tuned for the Christmas parties in the month to follow. His interests also extended to amateur astronomy, bread making, beer brewing, and harvesting California fungi. Ann and he often hosted dinners and parties both for international visitors and for Berkeley friends. He remained intellectually curious and eager for new knowledge to the last. On the morning of his passing, he called a colleague for a book he intended to read on the life of Ulysses S. Grant.

He is survived by his wife, Ann Reynolds; his children, Amy, Horace, Brian, Petra, and Karen; and his sister, Peggy. Amy, Horace, and Brian were children by his first wife;

Karen was his daughter by his second wife; John and Ann adopted Petra, the daughter of his second wife.

HONORS

1965 Awarded the John Price Wetherill Medal of the Franklin Institute
1967 Awarded the J. Lawrence Smith Medal of the National Academy of Sciences
1968 Elected to National Academy of Sciences
Golden Plate Award of the American Academy of Achievement, Dallas
1973 Awarded the Leonard Medal of the Meteoritical Society
NASA Exceptional Achievement Award
Selected to give the Faculty Research Lecture, awarded to a professor each year. (He spoke on "Telling the Aeons of Forgotten Time.")
1980 Elected to American Academy of Arts and Sciences
1987 Doctor, *honoris causa*, University of Coimbra, Portugal
1988 Awarded the National Order of the Southern Cross, with grade of Comendador
Awarded the Berkeley Citation, for outstanding research, teaching, and service

I AM INDEBTED to Ann Reynolds for her friendship and for her help with this memoir, to Professors Bruce Bolt and Richard Packard for contributing part of the text, and to the Physics office at Berkeley for use of its extensive records.

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LOUIS BYRNE SLICHTER

May 19, 1896–March 25, 1978

BY LEON KNOPOFF AND CHARLES P. SLICHTER

Louis Slichter was one of the foremost geophysicists of the twentieth century, an outstanding leader, scholar, and teacher. He was a pioneer in studies of inverse problems of geophysics, heat flow and cooling of the earth, free oscillations of the earth, solid-earth tides, crustal seismology, and the application of physical methods to the exploration of mineral deposits. His was the first work in many of these fields.

Slichter was born in Madison, Wisconsin, the second of four sons of Charles Sumner Slichter, professor of mathematics and dean of the Graduate School at the University of Wisconsin, and Mary Louise (Byrne) Slichter, also a teacher. The family life centered on the university, and the family environment offered enormous stimuli and challenges to excellence, always within a framework of mutual respect and good humor. All four brothers succeeded to positions of eminence in the professional and academic worlds. Louis received his undergraduate B.A. from the University of Wisconsin in 1917. He later recalled with pleasure the weekly coaching he had from Professor Max Mason of the Physics Department at Wisconsin. Mason himself had been a student of Louis' father.

The United States was already a participant in World War I at the time of Louis' graduation. Immediately after his graduation Mason, who was already active in antisubmarine warfare research, enlisted Louis' participation in a project to detect enemy submarines acoustically. Mason's solution to improve the signal-to-noise problem was to set up phased, linear arrays of sonic receivers on each side of the bow of a destroyer.[1] In the summer of 1917 Lake Mendota, next to the Wisconsin campus, was the site of the first experiments on this project. Later that year Louis and his work on submarine detection moved to the Naval Experiment Station at New London, Connecticut; he was commissioned Ensign Louis Slichter, USNR. For a time he was assigned to the sub-chaser base in Plymouth, England. Testing was done in the dangerous zone of the Atlantic. The shipboard installation of instruments was becoming a reality by the time the war ended.

Louis returned to Wisconsin in 1919 for graduate studies under Mason; he received the Ph.D. in physics in 1922. His dissertation concerned the construction of a device to display the waveform of an acoustic signal, which he did by mechanically linking the motion of a conical aluminum diaphragm to a mirror whose deflection was recorded photographically. His approach was characteristic of his entire career in instrumentation: He developed and solved the coupled differential equations for the vibration of a diaphragm into the fore and aft acoustic spaces and used the results to control the design. After receiving his Ph.D., Louis was a physicist with Submarine Signal Corp. in Boston from 1922 to 1924, where he worked on problems of echo sounding. In 1925 he applied his echo-sounding experience to locate a major, dangerous leak in the Dix Dam in Kentucky, at 287 feet the highest earth-filled dam in the world at the time it was built.

In 1924 the United Verde Copper Co. asked Mason to conduct research on the problem of finding ore by "remote sensing." The partnership of Mason, Slichter, and Gauld had contracts with United Verde and with a number of other well-known firms until 1930. In 1925 Mason became the president of the University of Chicago, and the responsibility for the work of the partnership was largely Louis'. The focus of the work was the detection of electrically conducting and magnetic ore bodies by magnetic profiling, applied DC electrical potential methods, and electromagnetic induction methods, the latter by measuring the perturbation of the field of AC signals from small local antennas at frequencies up to 1 kilohertz. As usual the fieldwork had a theoretical foundation. Slichter's first prospecting paper, in 1928, was a calculation of the susceptibility of dispersed magnetic particles as a function of their concentration, and showed that the great Kursk magnetic anomaly and his own magnetic profiles over the Falconbridge nickel body in Canada could be explained in terms of these susceptibilities. His theory of the electromagnetic response of a conducting sphere was in accord with the field profiles at Falconbridge. The company carried out fieldwork on ore bodies in eastern and western Canada, the western United States, Mexico, and Peru. In the Peruvian instance Slichter worked at an elevation of 16,000 feet. The citation for the award of a lifetime honorary membership in the Society of Exploration Geophysicists includes these words: "It is relatively easy to imagine the amazement of mining geologists in those early days of geophysical prospecting at the success of applications of Maxwell's equations to the location of buried ore bodies."

Slichter was interested in prospecting problems in his later career as well. He showed that probabilistic models could be of great use in developing strategies for exploration (1955, 1959, 1960). He argued that both our petroleum and mineral natural resources were exceedingly underpriced (1959). He estimated that the cost of burning gasoline at 1959 prices was equivalent to paying a manual laborer 1 cent per eight hours of work to expend the same amount of energy; in 2003 gasoline prices the equivalent hourly rate is 1 cent per hour. Slichter was concerned with global population growth and with the acceleration in the rate of consumption of our nonrenewable resources. He proposed that geophysical exploration to discover new deposits would help extend the world's metal resources.

By 1930 the Great Depression had put a damper on the mining industry's enthusiasm for finding metallic ores, and the firm of Mason, Slichter, and Gauld was a casualty. In 1930 Louis had a one-year appointment as a research associate at Caltech and used the opportunity to sharpen his mathematics, physics, and geophysics skills. Most of his theoretical papers from this time forward on inverse problems, heat flow, and free oscillations display elegant skills in applied mathematics. Waldemar Lindgren, chair of the Geology Department at MIT, wanted to start a program of geophysics in his department, and during the Caltech year Louis was invited to join the MIT faculty. (His Dix Dam experience served as one of the core examples of geophysics in his recruitment lecture at MIT.) Louis served at MIT as associate professor from 1931 to 1932 and then as professor of geophysics from 1932 to 1945. His was the first appointment in solid-earth geophysics at MIT.

At MIT Slichter originated the study of the geophysical inverse boundary value problem, which is that of the determination of the distribution of properties of the earth in its interior from measurements over the surface. He published papers on the inverse problems of travel-time seismology (1932), electrical resistivity (1933), and electromagnetic induction (1933). He returned to some of these problems a number of times in later years. Slichter recognized that the solutions to these problems are non-unique. A notable example was his demonstration that it is impossible to obtain the velocity cross-section uniquely in a low-velocity zone from the travel times of seismic waves. His postdoctoral student Chaim Pekeris published the solution to the inverse problem for seismic waves, which is to determine the elastic constants and density at depth from observations of the motion of the surface.

For an application of the inverse conductivity problem, Louis carried out an audacious experiment to measure the electrical conductivity of the earth at depth under Massachusetts. Thirty miles of public utility electric power lines, from Clinton to West Roxbury, were removed from public use and reconnected as a source circuit around midnight when public consumption was low. Ten to 25 amps DC commutated about once per second were passed through the ends of the grounded power lines; the earth completed the circuit. The potentials at roughly 100 electrodes to a range of 50 miles were measured by temporarily taking over the toll telephone lines of the New England Telephone and Telegraph Co. throughout the state. These were used as leads to one terminal of a potentiometer, with the other terminal connected to a reference ground. He succeeded in inverting the observations and obtained a conductivity profile to a depth of 8 km in the earth's crust (1934); below that depth the conductivity increased significantly and could not be resolved. Not only would extension to greater depth have required a greater separation of the source current electrodes and hence greater power but also the one-dimensionality of the model would no longer have been valid because of the proximity of the conducting Atlantic Ocean. It is debatable whether the successful result or the preliminary arrangements were the more remarkable.

Slichter's explosion seismology studies in New England and Wisconsin were the first crust and mantle refraction seismology experiments from controlled explosion sources with sufficient range to explore the thickness of the crust and the seismic velocity in the mantle below. He invented a portable array of three-component, short-period seismographs (1936) that made use of the innovative zero-length spring developed by Lucien LaCoste shortly before. The magnification was up to 100,000. Recorder times were synchronized to radio signals. Twelve such portable three-component seismographs were used in six large time-controlled quarry blasts in New York and Connecticut in 1938 and 1939; forty-two useful records were obtained to distances of 205 km. Six timed blasts in the Upper Michigan Peninsula, Wisconsin, and Iowa were also observed (1952). He obtained a 6.32 km/sec crustal P-wave velocity and a mantle velocity of 7.82 km/sec with a crustal thickness of 23.5 km under the Connecticut River Valley (1939). The structure he deduced under the Upper Michigan Peninsula was that of a crust of thickness 42 km with P-wave velocity from 6.0 to 7.0 km/sec below a surficial sedimentary layer 1.6 km thick, and a mantle P-wave velocity of 8.17 km/sec. The findings of later investigators were in excellent agreement with these values. The campaign of 1938-1939 showed that a sensitive, portable system of seismographs, not tied to fixed observatories, was a practical tool for exploring the structure of the earth's crust and upper mantle. Crustal and upper mantle explorations of this type became a hugely popular, widespread activity in Europe and North America in the 1960s and 1970s.

Slichter recognized that the problem of temperature distribution in the earth was fundamental to any discussion of the development of the earth and the formation of its surface features. His 1941 paper on the thermal history of the earth, which took into account radioactivity as a source of internal heat, was an important influence in developing our present concepts of the earth's internal processes. He solved the problem of heat transfer within a sphere in the presence of a variable heat source distribution and showed that only a small proportion of the heat produced within the earth would reach the surface if the mechanism of heat transfer were that of thermal conduction. In this model the thermal time constant of the earth is very long compared to its age, and only the outer layers contribute to the surface heat flow. He showed that even a minute amount of convection in the mantle, as small as 1 mm per year, could transport 100 times more heat to the surface than the amount transferred by conduction (1940). This was the beginning of his belief that mantle convection was an important process in the dynamics of the earth's mantle. He argued that heat flow measurements by themselves were inadequate to resolve the state of the earth's interior and proposed that a study of atomic vibration theory might prove useful for understanding conductivity at depth (1940). His graduate and postdoctoral student, Norman Haskell, determined the effective viscosity of the earth's mantle from the rebound of the surface due to the removal of the glacial load. Both the heat flow and the viscosity studies were important precursors to later work on modeling mantle convection.

Well before the start of World War II, Louis' academic life at MIT was interrupted by his involvement once again with issues of national defense; as a member of Division 6 of the National Defense Research Council, he was again concerned with the problems of enemy submarines. In the fall of 1940 he was a member of a Navy department subcommittee that studied the effectiveness of the Navy's program for submarine detection. In the spring of 1941 he flew to Britain to establish collaborative liaison between the antisubmarine research establishments of the U.S. and British navies at the request of President Jewett of the National Academy of Sciences. He was responsible for the development of magnetic and electromagnetic devices for the detection of ships and submarines. In Pasadena he was a member of a group that worked to understand why torpedoes launched from low-flying aircraft ricocheted off the sea surface. Unfortunately, scaled models plummeted. Louis solved the latter problem by an ingenious modification of the air flow around the nose of the model.[1]

In 1944-1945 a new Institute of Geophysics at the University of California was proposed by members of the faculty at UCLA. Once the proposal was approved, a committee of faculty from both the northern and southern campuses of the University of California, appointed by President Sproul, agreed in October 1945 to locate the headquarters at UCLA, to define the activity of the Institute as research in "the physics of the atmosphere, of the ocean and of the solid earth," and to select Slichter, by then a member of the National Academy of Sciences, having been elected in 1944, as their first choice for director. In November 1945 and again in March 1946 the committee inquired into Slichter's interest in the directorship. At the war's end Louis had taken a professorship at the University of Wisconsin, where he initiated instruction in geophysics. Later he stated, "While in the war work, I made a firm promise to myself not to engage in any major research project until I'd taken about a half year off and looked around a bit." His replies to the inquiries from UCLA were that it had been too short an interval since his arrival at Wisconsin for him to contemplate another move. He hoped that "major decisions such as the UCLA one should be deferred if possible." His response "left the door open." In July 1946 the Institute was formalized, and Joseph Kaplan, one of the authors of the original proposal and a member of the committee, was appointed acting director. Kaplan's view was of a partitioned Institute in which atmospheric research would be carried out at Los Angeles, oceanographic research at the Scripps Institution of Oceanography (then administratively a part of UCLA), and solid-earth research at Berkeley. To this end he made offers of positions to atmospheric scientists C. E. Palmer and R. E. Holzer and oceanographer W. H. Munk.

In the summer of 1946 Slichter began seven months of visits to centers of geophysics in California and the two Cambridges[2] with the support of a fellowship from the Rockefeller Foundation. Immediately upon returning to the United States in the first week of January 1947, Slichter contacted the chair of the UCLA committee, V. O. Knudsen, to indicate his interest in the directorship. The travel of 1946 was evidently a time of decision making with regard to the Institute and thinking how its program might be organized if he were the director. The committee recommended that the appointment be made as quickly as possible. In mid-July Slichter accepted an appointment as professor and director of the Institute of Geophysics, effective July 1, 1947.[3] The Slichter family arrived in September.

With Louis' appointment the model of an Institute at UCLA devoted exclusively to atmospheric science was moot. In 1948 David Griggs, a world-class solid-earth experimentalist, began a professorial appointment in the Institute. The Institute on the Los Angeles campus now consisted of two atmospheric and two solid-earth geophysicists. All budgeted positions had been filled. It took a number of years for the Institute of Geophysics to make additional new appointments. Except for the short-lived appointment of an atmospheric scientist to an assistant professorship in 1949, no appointments were made until 1954 with the addition of two faculty members in geochemistry. From then on, the Institute grew with distinction. Slichter's view was that most areas of geophysics should be represented in the Institute, with special emphasis on areas of research that were outside the mainstream at any given time. From the beginning the Institute was to be a university-wide organization. During Louis' tenure as director, the annual scientific meetings of the Institute, held in rotation on each of the La Jolla, Berkeley, and Los Angeles campuses, were the premier national meetings in geophysics. A branch of the Institute of Geophysics on the La Jolla campus came into being in 1960 under Munk's directorship. This is the first example of a multicampus, multidisciplinary institute at the University of California, a model that was emulated in other fields only decades later. Slichter was the director of the statewide Institute as well as director for the Los Angeles branch. Today there are seven branches of the Institute at campuses and laboratories of the University of California. An insightful account of the difficult early years of the Institute and of Slichter's unselfish dedication to its development has been given by C. B. Palmer.[4]

By the time of his retirement, the Institute was a distinguished model for other departments and institutes of geophysics. His view of the Institute was that distinction, independent of subdiscipline, should be the qualification for membership, and through his distinguished leadership it came to be. At the time of his formal retirement from the faculty at UCLA in 1965, 11 members of the Institute of Geophysics and Planetary Physics at UCLA were members (7) or future members (4) of the National Academy of Sciences. (The name had been changed in 1960.) "The Institute of Geophysics and Planetary Physics was Louis' greatest achievement."[5] He brought distinguished scientists from a variety of disciplines to UCLA to participate in a continuing colloquy in the interdisciplinary fields of the physical sciences of the natural environment. The Institute is the chief monument to his genius with people, and the multidisciplinary Institute mirrored and expressed his own breadth.

Louis organized the famous conference at Rancho Santa Fe on the "Evolution of the Earth" (1950), which synthesized ideas from diverse fields in understanding the basic problems of the earth. Participants were a Who's Who in seismology, geochemistry, geochronology, petrology, heat flow, physics, chemistry, astronomy, and fluid mechanics. A product of the conference was to put forward the argument that fractionation of the continents might provide enough differential heating to drive convection processes in the earth's mantle.

The task of building the Institute did not deter Louis from continuing his research on problems of fundamental importance. At UCLA he began research on the gravity field of the earth. Influenced by recent observations by F. A. Vening Meinesz and by the models of Griggs, both strong advocates of mantle convection and continental drift, Louis organized expeditions to measure variations of gravity over topographic features of the sea floor by pendulum observations in submarines.

The earliest information about the elastic properties of the earth's interior was derived by Kelvin on the basis of studies of ocean and solid-earth tides. Despite the fundamental information that could be derived from tide studies, these were largely neglected until Slichter began a program of observations using modern instrumentation. Starting in 1950, through his contact with LaCoste, Louis acquired a number of LaCoste's ultrasensitive earth-tide gravity meters; these were deployed to study the earth's tides and later in seismological pursuits. He was soon recognized as a world leader in the analysis of the solid-earth tides. In 1957 twelve temporary earth-tide measuring stations were installed in equatorial regions around the world during the International

Geophysical Year (1963). He discovered that the oceans were a potent influence on measurements of the diurnal and semidiurnal vertical solid-earth tides at coastal sites, and could produce phase shifts in the observations of as much as three hours (1953). From observations made during a total eclipse of the sun, he was able to place an upper bound on the cross-section for shielding of gravitational fields by matter (1965).

The tidal gravimeter at UCLA was an ideal instrument for recording the first observations of the spheroidal free oscillations of the earth excited by the great Chilean earthquake of 1960 (1961). The lowest frequency observed was that of the quintuplet mode ${}_0S_2$ with a period of 54 minutes. All modes except the purely radial ones had uncertain central frequencies because of the unequal excitation of the multiplets, broadened due to attenuation. These observations of the earth's resonance spectrum formed a basic dataset for inversion to obtain earth structure. The ground state, which is the spectral triplet ${}_1S_1$ corresponding to the Foucault pendulum oscillation of the inner core about the center of mass, was not observed. Slichter showed that the period of this spectral term, estimated to be around five hours, would provide the most direct evidence for the density of the inner core, as it depends critically on buoyancy effects (1961). The elusive "Slichter mode" remains undetected to this day.

In the more than 15 years following his retirement Slichter led an active research program that focused on gravimetric and seismological measurements at the South Pole. There were two motivations for this research. First, the high rigidity of the earth's mantle at periods as long as 54 minutes, and the low rigidity of a viscous mantle with a time constant of the order of 10,000 years, suggested the presence of a crossover between these timescales. The rigidity measurements could be extended to fortnightly and bi-fortnightly periods through a study of the tides. Measurements at the South Pole were a strong option because of the absence of the strong diurnal and semidiurnal tides present at lower latitudes. Second, the multiplet splitting of spectral resonances due to the earth's rotation and flattening, would be absent at the Pole, and it might be possible to measure the spectrum with greater accuracy than at lower latitudes. Louis was able to make the first direct observation of the fortnightly solid-earth tide, a considerable achievement because uninterrupted records of deformation over many fortnightly cycles were needed with a stable instrument at the Pole; the work appeared posthumously (1979). The result showed that the rigidity of the earth at these periods was consistent with seismic values and that the crossover was at still longer periods. A strong earthquake during the years of operation of the instruments at the Pole never took place in his lifetime, and the seismic part of his Antarctic program was not completed. He was very disappointed to be denied permission to travel to the South Pole to visit the installation because of his physical condition in these later years of his life.

Because his genius at defining new fields, his theoretical skills, and his devotion to rigorous data gathering were more widely known, his skills as an instrumentalist were not as often spoken of. He invented and patented a number of important geophysical devices that included the electromagnetic induction apparatus for location of buried conducting ore bodies, and downhole resistivity-measuring devices in boreholes; the latter patents were sold to the Schlumberger Company. The Slichter seismographs were operated at several seismological observatories for a number of years. He invented a suspension system to minimize the minute but significant effects of tilt of the ice platform at the South Pole on gravimetric measurements; the invention was, as always, buttressed by extensive mathematical calculations. At the time of his death he was developing a tiltmeter for installation in the ice at the South Pole to measure the horizontal component of the earth's deformation at tidal periods.

Louis was a man of great, good humor, as illustrated by remarks about parental discipline after childhood exploratory excursions onto thin ice on nearby Lake Mendota. "We were sternly forbidden to get out on the lake during those times. This restriction was strongly enforced by the switch. Each brother had to be taught independently about the laws of the land. The other three always enjoyed these lickings rather well, so I think it all contributed to the greatest happiness of the greatest number."[5] Louis, more so than the other brothers, exhibited mechanical talents in his early years, a forerunner of his later skills as an instrumentalist and experimentalist. He was encouraged to use his father's workshop. In his teens he gave a preview of coming attractions by designing and building an ice boat driven by an aircraft propeller and motor rather than by sail. Unfortunately, this engineering marvel slipped beneath thin ice on its first voyage. Louis jumped to safety. Knowing of its meaning to its designer, his father arranged to have it recovered from the bottom of the lake. For many years Louis kept the propeller in his office.

Louis was an outstanding sailor and iceboater. In July 1947, just before coming to UCLA, Louis and his family spent a holiday in Maine, where, of course, he rented a sailboat and entered the races. The rental craft was reputed to have had a very bad racing record over the years, but Louis quickly used his mechanical, fluid mechanics, and strategic skills to overcome its shortcomings. The usual racing crowd expected little from the rental craft, skippered by someone from the Midwest with little experience on the ocean. Louis won all the races in which he was the skipper.

During two successive summers while in graduate school, Louis ventured west for a mountain-climbing career in the high Sierra with the noted mountaineer Norman Clyde. They made the first ascent of the West Vidette in August 1920.[6] Louis was an avid swimmer. In his mid-sixties he astonished his colleagues at the Institute upon his return from a family vacation in Hawaii by displaying his certificate as a qualified surfboarder.

Louis was identified in the minds of his colleagues with his continuing spirit of good humor, which was a real part of his creativity and leadership. His friendship, warmth, and contagious enthusiasm were an integral part of his ability to generate an extraordinary affection for him among his colleagues. His office was filled with his wit and his wisdom. It is difficult to say whether Louis' own brilliant scientific achievements or his kindly personal influence over those associated with him were of greater significance. His post-doctoral students included leading geophysicists both in the United States and abroad. Through his warmth and vitality, as well as his penetrating insights, he stimulated his colleagues to identify good science and to avoid the temptations to join the ranks of those he characterized as the "great windbags of science." Despite a long battle against ill health caused by adult-onset diabetes, he maintained a cheerful manner. His almost daily appearances at his laboratory to the very end of his life were punctuated by enthusiastic and stimulating discussions with his colleagues, who continued to learn from this great teacher to the end of his career. Martha, Louis' companion for more than 50 years, was closely identified with him in the affection of his colleagues. She and their two daughters, Mary Lou Slichter Whaling and Susan Merry Slichter, survived him at the time of his death.

Among the honors that Louis Slichter received were the Presidential Certificate of Merit (1946), a Rockefeller Foundation Fellowship (1946), the Jackling Award of the American Institute of Mining and Metallurgical Engineers (1960), the William Bowie Medal of the American Geophysical Union (1966), and an honorary life membership in the Society of Exploration Geophysicists (1959). The Distinguished Service Citation "in recognition of eminent professional services" was presented to the four Slichter brothers by the University of Wisconsin (1957). He was the Thirty-eighth Annual Faculty Lecturer of the University of California, Los Angeles (1963). He was awarded the LL.D. by the University of Wisconsin (1967) and the D.Sc. by the University of California, Los Angeles (1969). He was elected to membership in the National Academy of Sciences (1944) and was the chair of its Geophysics Section (1960). He was a fellow of the American Academy of Arts and Sciences, a fellow of the American Physical Society, and a fellow of the American Geophysical Union. Slichter Hall at UCLA and Slichter Foreland on the Martin Peninsula in Antarctica have been named for him. There is a collection of scientific papers in his honor.[7]

NOTES

1. Slichter's detailed account of the acoustic submarine detection work of World War I and the torpedo entry work of World War II can be found on pages 212-217 and 227-229, respectively, of W. Weaver. Max Mason, 1877-1961 *Biographical Memoirs of the National Academy of Sciences* 37(1964):205-236.

2. Rockefeller Foundation Archives.

3. Louis' appointment came only after a series of events in the first half of 1947 in which he played no direct role. Upon receiving Louis' expression of interest in the position in the first week of January, the committee, now renamed Advisory Board, was polled and by January 13 unanimously and strongly endorsed the offering of the directorship to Slichter (letter from Knudsen to President R. G. Sproul, February 7, 1947, recommending appointment). On January 31 Kaplan received a letter from Sproul, which stated in part, "The form covering your change in status . . . to Professor of

Physics and Director of the Institute of Geophysics, which you submitted recently, has been approved . . ." (Knudsen to F. A. Brooks, April 11, 1947). The board declined Kaplan's request that it ratify the change in status (Knudsen to Sproul, February 7, 1947). Kaplan stated that the change in title came as a surprise (Kaplan to Knudsen, April 8, 1947); Sproul's letter of January 31 indicated that it had come at Kaplan's initiative. Kaplan accepted; two weeks earlier he had voted to confirm Slichter. The formal paperwork for the change in title arrived at the end of March 1947: Kaplan had been appointed director for one year retroactive to July 1946. Upon learning of the change in Kaplan's title, some on the board expressed concern, since the action was contrary to their recommendation (Brooks to Knudsen, March 31, 1947). Knudsen promised to protest in behalf of Slichter's nomination if it became necessary. (Knudsen to Brooks, April 11, 1947). There is no record that a protest was necessary. After completion of the appointment process, Sproul sent a cordial letter of appointment in early 1947. Louis accepted in late July. Kaplan's appointment as director was indeed only for one year. "Kaplan has had an unfortunate experience in some ways because he was not told that his directorship was acting. However there seems to be no one on the committee who thought that he should have been named director." (P. Byerly to Slichter, September 2, 1947). As a consequence of these events, an unfortunate rift between Slichter and Kaplan developed that was irreparable.

4. C. E. Palmer. Louis Byrne Slichter: Builder of the Institute of Geophysics and Planetary Physics. *J. Geophys. Res.* 68(1963):2867-2870.

5. T. J. Tugend, interviewer. *Mr. Geophysics, Louis B. Slichter* (An oral history). Los Angeles: University of California, 1984

6. H. Voge, ed. *A Climber's Guide to the High Sierra.* San Francisco: Sierra Club, 1954.

7. Papers in Geophysics Honoring Louis Byrne Slichter. *J. Geophys. Res.* 68(1963):2867-2983, 3627-3634.

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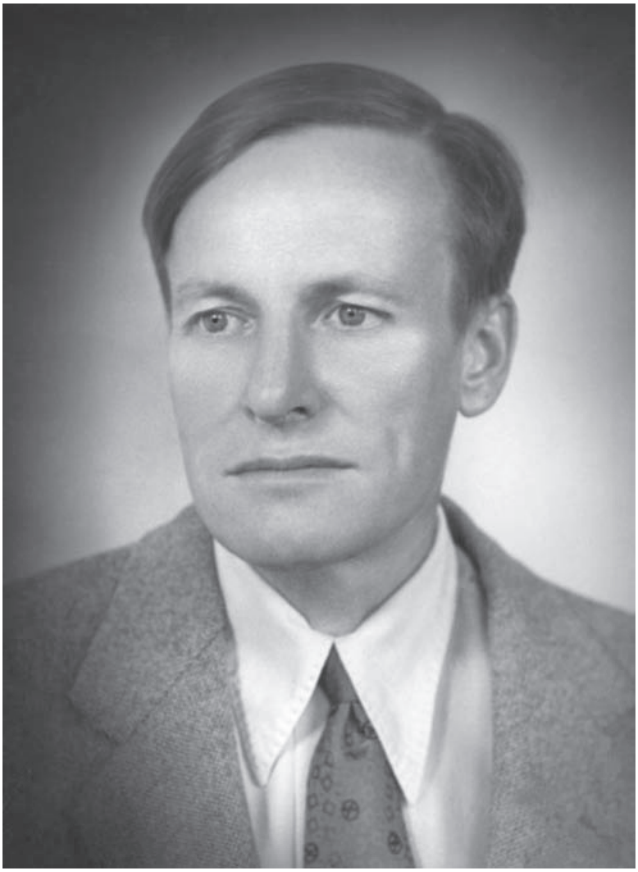

G. Ledyard Stebbins Jr.

GEORGE LEDYARD STEBBINS

January 6, 1906–January 19, 2000

BY VASSILIKI BETTY SMOCOVITIS AND
FRANCISCO J. AYALA

GEORGE LEDYARD STEBBINS'S most important scientific contribution was the publication in 1950 of *Variation and Evolution in Plants,* the last of a quartet of classic books that in the second quarter of the twentieth century set forth what became known as the synthetic theory of evolution or the modem synthesis. The other books are Theodosius Dobzhansky's *Genetics and the Origin of Species* (Dobzhansky, 1937), Ernst Mayr's *Systematics and the Origin of Species* (Mayr, 1942), and George Gaylord Simpson's *Tempo and Mode in Evolution* (Simpson, 1944). The pervading conceit of these books is the molding of Darwin's evolution by natural selection within the framework of rapidly advancing genetic and biological knowledge. *Variation and Evolution in Plants* distinctively extends the scope of the other books to the world of plants, as explicitly set in the book's title. Dobzhansky's perspective had been that of the geneticist and he set the tone for the others, Mayr's that of the zoologist and systematist, and Simpson's that of the paleontologist. All four books were outcomes of the famed Jesup Lectures at Columbia University. Plants, with their unique genetic, physiological, and evolutionary features, had been all but left completely out of the synthesis until that point. In 1941 the eminent botanist Edgar Anderson had been invited to write botany's analogue to Mayr's *Systematics and the Origin of the Species* and to publish it jointly with Mayr's book. Anderson did not fulfill the task, and Stebbins was thereafter invited to deliver the Jesup Lectures in 1947. *Variation and Evolution in Plants,* the outgrowth of the lectures, is the most important book on plant evolution of the twentieth century.

Stebbins's scientific contributions comprise botany, genetics, and evolution (Solbrig, 1979). His research was mostly done in the field, with laboratory work focused on cytological and genetic investigation of collected plant specimens, seeking, for example, to determine the number of chromosomes or whether hybridization had occurred. In the 1970s and 1980s he became much interested in the interaction between plant development processes and their evolution. His discoveries in this area were modest, but his vision was not: By the early 1990s "evo/devo," as this research subject has come to be known, had become one of the most active and productive areas of evolutionary research. Stebbins was an accomplished synthesizer of biological knowledge (Smocovitis, 1997). Throughout the decades he wrote numerous review articles. He was considered a master of this genre (Crawford and Smocovitis, 2004). Stebbins was elected to the National Academy of Sciences in 1952. George Ledyard Stebbins, Jr., was born on January 6, 1906, named after his father, although he preferred to be called Ledyard and dropped the "junior" from his name after his father died. He wrote about his early years:

My home background was upper middle class white protestant in New York and New England. My Father was a reasonably prosperous produce merchant and real estate owner and agent. He ran the Seal Harbor (Maine) Realty Co. for 30 years (1899-1929) and was largely responsible for the development of that summer resort on Mt. Desert Island. He also played an important role in setting aside the land that later became Acadia National Park. For the first 8 years of my life our home was, for six months, Seal Harbor, and for the other six months Woodmere, Long Island, New York. Later, we had to spend winters in California and Colorado because of my mother's health: She was a semi-invalid from 1914 until her death in 1952, but from 1922 (my age was 16) until 1929, I spent summers regularly in Seal Harbor. Neither parent was a naturalist, biologist or academician, but both had amateur interests in Natural History. My interest was stimulated by them as well as by a neighbor and close friend, Professor Edward S. Dana (geologist) of Yale.*

At the age of three Stebbins had already shown a marked tendency to enjoy time out of doors and developed a love of plants and natural history, but he had also begun to manifest a behavior pattern for which he later became renowned: a tendency to quick, sudden outbursts of anger, especially as a means to gain attention or win arguments. As an adult his temper tantrums became the stuff of legends and amusement among friends and acquaintances. A much repeated but probably apocryphal story had Stebbins as an adult throwing a secretary's typewriter out the window in a fit of anger. As an adult he quickly recovered from his bursts of anger and meekly apologized, but the recovery often was much more difficult and slower for those subject to them. Distinctive traits that were expressed early also included a distaste and inability for "manual training and other occupations that required muscular coordination and manual dexterity. I was very poor at athletics and team games."* Vestiges of his early childhood that remained with him until the end included a New England "preppie" accent and a

*These quotations are from autobiographical notes available in the archives of the National Academy of Sciences.

schoolboy sense of humor, frequently seen in his love of rhyme and silly verse.

Ledyard "attended only private schools: Lawrence School on Long Island, Polytechnic Elementary in Pasadena, CA, St. Stephens Boarding School in Colorado Springs, Colorado, and Cate School, Carpinteria, CA [near Santa Barbara]."* He later wrote that "most of my formative years (from 8 yrs. on) due to my mother's invalid condition, were supervised by governesses or masters at boarding school. I took a considerable interest in hiking, mountain climbing and general natural history. . . . I was also much interested in music, and took piano lessons from the ages of 8 to 15. My lack of manual dexterity made me always a poor performer, but I grew to an avid like of and appreciation for classical music."*

An especially important period of Ledyard's life was the four years spent at Cate School in Santa Barbara. There he learned to ride horseback, explored the Santa Inez Mountains, and fell under the influence of the botanist Ralph Hoffmann, who taught him much about the plants and natural history of that beautiful region. In 1924 Ledyard enrolled at Harvard University, a choice dictated by family background, but also because his older brother Henry was enrolled there. At first he had difficulty defining his major, but the summer between his freshman and sophomore years, spent investigating the plants around Bar Harbor, Maine, the family home, brought him into contact with Edgar T. Wherry, professor of botany from the University of Pennsylvania and a specialist in ferns, who encouraged his botanical interests. When he enrolled for the fall semester of 1925 at Harvard, he had decided to pursue a botanical career.

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*These quotations are from autobiographical notes available in the archives of the National Academy of Sciences.

He began graduate work in Harvard's botany department in 1928. The experiences in graduate school shaped his subsequent research style. He acquired a lifelong tendency to move from one vital area of research to another as the science demanded, even when this meant overcoming practical obstacles and difficult personalities. His initial training was in floristic botany with the renowned systematist Merritt Lyndon Fernald in the Gray Herbarium. He quickly lost interest in Fernald's outdated taxonomic methods and was disappointed by Fernald's rigid personality. Stebbins derided his teacher as one of the "eminent exsiccatae in the Gray Herbarium." His interests turned instead to the newer, more modern approaches of Karl Sax, who was applying cytogenetic principles to gaining a deeper understanding of plants and their reproductive processes. For his doctoral research he began anatomical and cytological studies of megasporogenesis in the ovule and microsporogenesis in the pollen of the plant genus *Antennaria* with the eminent morphologist and cytologist E. C. Jeffrey. He collected *Antennaria* easily in the nearby environs so that he could also study geographic variation in the genus; this work was satisfying, but he began to fall out with his graduate advisor. Jeffrey, who was frequently referred to as "the stormy petrel of botany," hated with a vengeance the work of the noted plant geneticist Karl Sax and others, which emerged from the school of genetics associated with Thomas Hunt Morgan. He preferred instead the idiosyncratic hybridization theories made popular by J. P. Lotsy. Jeffrey actively campaigned against Morgan's genetics and vigorously discouraged Ledyard from pursuing that subject.

The growing interest in genetics took on a life of its own, however, especially since plant genetics, systematics, and evolution and the zones of contact between the three were becoming exciting new areas of research in the 1920s

(Hagen, 1982, 1984). Stebbins made frequent visits to the library of Harvard's Bussey Institution to keep up with genetics journals, such as *Hereditas* and *Genetica,* that contained articles by A. Muntzing and C. Leonard Huskins. He sought out E. M. East and other geneticists at the Bussey and took courses with W. E. Castle, although Castle's mammalian genetics was not immediately helpful to a student of botany. Stebbins's growing interest in the genetical literature became serious after he began to work with Karl Sax, who had recently been appointed to the Arnold Arboretum. His growing collaboration and friendship with Sax, one of the leaders of plant genetics in his generation, did not, however, sit well with Jeffrey. When Sax located a serious error of interpretation in Stebbins's chromosomal studies in his doctoral research, Jeffrey threatened to resign from the thesis committee in retaliation for what he viewed as intrusion into his direction of a student. Stebbins was caught in the crossfire between Jeffrey and Sax. His dissertation was finally approved, thanks to judicious efforts by Ralph Wetmore and others, but it had been amended so many times to meet the demands of a squabbling committee that it bore numerous scissor and paste marks masking the "offending" passages. It still stands in the Harvard archives as a testament to the contentious Harvard personalities in the botany department of those times. The dissertation was completed in 1931 and was published as two papers in 1932. Stebbins had already published some articles on the flora of Mt. Desert Island with the assistance of Fernald in his journal *Rhodora.*

One of the key events in Ledyard's early career was his attending the International Botanical Congress at Cambridge, England, in 1930. There he met Edgar Anderson, who was to become a lifetime friend and colleague, Irene Manton, and C. D. Darlington. These and other contacts greatly encouraged his interest in and enthusiasm for botany, which was to be sustained for the rest of his life. Edgar Anderson was at the time a fellow at the John Innes Horticultural Institute. Anderson's and Stebbins's strong and colorful personalities complimented each other. In 1930 Anderson was about to begin his work on detecting and measuring variation patterns in plants, such as *Iris,* that were frequent hybridizers. He eventually went on to do pioneering work on hybridization and was the first to articulate the notion of introgressive hybridization, a phenomenon seen often in plants (Kleinman, 1999).

Stebbins spent the four years after he obtained his Ph.D. (1931-1935) at Colgate University. He later described these years as unhappy ones, one reason being the heavy teaching load assigned to him, and the other being the emphasis the school placed on its athletic program over its academic mission. Despite the difficult environment, Ledyard found time for research, concentrating on cytogenetic studies of *Paeonia.* He collaborated with Percy Saunders (of the Canadian Saunders family of wheat breeders) at nearby Hamilton College. Saunders was a keen collector and breeder of peonies, and his backyard was a profusion of these beautiful plants. Stebbins and Saunders engaged in chromosomal studies of hybrids of *Paeonia* of both Old World and New World forms. The cytogenetic work was mostly done in a makeshift laboratory in the basement of the Saunders house. This was the first of a series of essentially biosystematic investigations of diverse plant groups that dominated much of Stebbins's subsequent research career. During this time he discovered complex structural heterozygosity in the western North American species of the genus, an exciting find that was to fuel his enthusiasm for further cytogenetic investigations.

With Saunders, Ledyard attended the 1932 International Congress of Genetics in Ithaca, New York. Stebbins later reminisced that he had seen the famous posters that Sewall

Wright had set up displaying his shifting balance theory of evolution, but he could not understand at the time what precisely they represented. Only later, after reading Dobzhansky's *Genetics and the Origin of Species,* would Stebbins understand the significance of the graphs. He attended one particularly memorable session that featured Sax and the English cytogeneticist C. D. Darlington, who engaged in a heated debate about chiasma-type theory. He listened closely to Thomas Hunt Morgan's famous address on the future of genetics. He also studied John Belling's poster demonstration of chromomeres, which had mistakenly been identified as genes. Most exciting of all, however, was Barbara McClintock's presentation of some of her cytological studies of maize. McClintock showed the linear pairing of parental chromosomes at the mid-prophase or pachytene of meiosis and the crossing over between chromosomes. She also demonstrated beautifully the presence of inversions and translocations. A few years later Stebbins replicated some of the same studies in *Paeonia* and was the first to detect chromosomal ring formation in this genus. This work was not groundbreaking, but it confirmed what McClintock and others had described (1939).

In 1935 Professor Ernest Brown Babcock of the University of California, Berkeley, offered Stebbins a research position in connection with his investigations of the genus *Crepis,* which he accepted cheerfully. Met at the train station by his fellow Harvard student Rimo Bacigalupi, he plunged into this project with enthusiasm. Stebbins had been recommended for the position by the Washington-based expert on the Compositae, Sidney F. Blake. At Berkeley, Stebbins began his lifetime preoccupation with Democratic politics, working actively in the 1936 Roosevelt election, and from there onward.

Babcock was engaged in an ambitious team-oriented project to find a plant equivalent of *Drosophila.* Very much eclipsed by his contemporary T. H. Morgan at the California Institute of Technology, Babcock was one of the most important figures in establishing and institutionalizing genetics within the agricultural college at Berkeley. One of the first genetics departments in the country was established there, thanks to the efforts of Babcock, who was convinced that genetics generally, and agricultural genetics in particular, was a vital part of the mission of the University of California. Babcock's vision for genetics at Berkeley was that it should rival the success of the Morgan school's investigations with *Drosophila melanogaster.* He chose the genus *Crepis* to be the plant equivalent of *Drosophila,* even though it was a weed and not an important crop plant, mostly because this genus with its diverse geographic variation patterns could be used to understand the genetic basis for evolutionary change, which could then form the basis for taxonomic studies (Babcock, 1920). Preliminary work had begun as early as 1917-1918, but the project continued into the late 1940s until Babcock's retirement. Babcock considered his monograph on the genus *Crepis* to be the centerpiece of his life's work (Babcock, 1947).

Stebbins's assignment assisting Babcock was to perform chromosome counts in some of the nearest relatives of *Crepis* in the tribe Cichorieae. He quickly developed an interest in Babcock's own research with the New World species of *Crepis,* because he recognized patterns of evolution that resembled *Antennaria* and *Paeonia.* Like these other genera, *Crepis* was a commonly hybridizing group of species that displayed polyploidy and could reproduce apomictically.

In 1938 Babcock and Stebbins jointly published a monograph on the American species of *Crepis.* It laid the foundation for understanding polyploidy complexes and the role of apomixis in the formation of some of them; for this reason they first termed the American species of *Crepis* an agamic complex. They recognized clearly that certain plant genera consisted of a complex of reproductive forms that centered on sexual diploids and that had given rise to polyploids; sometimes, as in *Crepis,* these were apomictic polyploids. Polyploids that combined the genetic patrimony of two species usually had a wider distribution pattern. The articulation of the polyploid complex was considered path-breaking work at the time. Not only did it demonstrate in detail the complex interplay of apomixis, polyploidy, and hybridization in a geographic context but it also offered insights into species formation, polymorphy in apomictic forms, and knowledge of how all these complex processes could inform an accurate phylogenetic history of the genus. Stebbins extended these ideas further in articles in 1940, 1941, and 1947. "Types of Polyploids: Their Classification and Significance," published in 1947, became a classic review article that synthesized knowledge bearing on polyploidy in plants.

In 1939 Stebbins was appointed to the faculty at Berkeley as an assistant professor in the genetics department in the College of Agriculture. Babcock, who was impressed with Ledyard's energy and industry, was instrumental in making the appointment. Earlier Stebbins had a significant disappointment in that he had failed to obtain the replacement position for Willis Linn Jepson in the botany department, which went instead to Lincoln Constance. Although he made himself at home with the botanists at Berkeley, Stebbins's interests were considered primarily genetic by his colleagues in botany, who did not feel that he would sufficiently focus on the curatorial work that the position demanded. The vacancy of a position in the genetics department that required the teaching of the general course on evolution was opportune for Stebbins, whose interests were shifting to the exciting areas in evolutionary genetics emerging in the late 1930s.

Stebbins's growing interest in evolution was fueled by two additional circumstances: his association with a unique group of biologists all concerned with evolutionary approaches to systematics, who called themselves "the biosystematists," and his special relationship with the eminent evolutionist Theodosius Dobzhansky. Beginning in the mid-1930s, the San Francisco Bay area became a hotbed for evolutionary activity. A new generation of systematists who incorporated insights from genetics and ecology had taken root in the Bay area at several institutions, notably Stanford University, the Carnegie Institution at Stanford University, the California Academy of Sciences, and the University of California, Berkeley. The biosystematists met at alternating locations every month to share in the methods and research that were characterizing the "new systematics" (Hagen, 1982, 1984). Ledyard was a prominent member of the group nearly from the start. He was active in inviting speakers, some of whom included visitors from other states, like his close friend Edgar Anderson and his fellow plant systematist at the University of California, Los Angeles, Carl Epling. The critical players in the biosystematists were the interdisciplinary Carnegie team that included the Danish genecologist Jens Clausen, the taxonomist David Keck, and the physiologist William Hiesey. By the mid-1930s this team was engaged in long-term systematic studies of patterns of variation in plants as these adapted along the steep altitudes in the California landscape. Ledyard followed this work closely and visited the team in their experimental sites at Stanford and at Mather Station near the entrance to Yosemite National Park.

The friendship with the evolutionary geneticist Theodosius Dobzhansky began in the mid-1930s. Stebbins met Dobzhansky on a visit to the California Institute of Technology in the spring of 1936, when Dobzhansky was already engaged in investigating the genetics of natural populations using *Drosophila pseudoobscura.* The two interacted further when Dobzhansky frequented the Berkeley campus to see his close friend, the geneticist I. Michael Lerner (then in the poultry husbandry department). Stebbins interacted with Lerner in a fortnightly journal club called Genetics Associated. Ledyard enjoyed listening to them discuss their mutual interests in evolutionary genetics, even though Lerner and Dobzhansky frequently turned to speak in Russian. The friendship with Dobzhansky was to prove critical to Stebbins as his own interests were shifting more and more to evolutionary genetics, stimulated in part by the teaching demands made by the evolution course. Dobzhansky, who published his own pathbreaking synthesis of evolutionary genetics, *Genetics and the Origin of Species* in 1937, fostered Ledyard's evolutionary interests. Through the 1940s they occasionally came in closer contact when they both met for fieldwork at the Carnegie Institution's field site at Mather. Both were avid horseback riders.

Dobzhansky, who had moved from Caltech to Columbia University in 1940, played the single most important role influencing Stebbins's career as an evolutionist. Columbia University invited Stebbins to deliver the prestigious Jesup Lectures at Columbia University in October and November 1946. During his stay of nearly three months, Stebbins was Dobzhansky's house guest. Stebbins had been selected because of the need for a comprehensive synthesis of plant evolution. In 1941 Edgar Anderson had delivered the Jesup Lectures with zoologist Ernst Mayr. Mayr subsequently published his lectures under the title *Systematics and the Origin of Species,* but Anderson never completed his set of lectures (Kleinman, 1999). The botanist's viewpoint was needed in what was emerging as the new synthesis of evolutionary theory. The book version of the lectures was published by Columbia University Press in 1950 under the title *Variation and Evolution in Plants.* Stebbins upheld the importance of most of the tenets emerging as the new consensus on evolutionary theory and was heavily influenced by Dobzhansky's *Genetics and the Origin of Species.* Stebbins stressed the centrality of natural selection but left plenty of room for random genetic drift and nonadaptive evolution, which had gained importance in the 1941 second edition of Dobzhansky's 1937 book. Stebbins also upheld Dobzhansky and Mayr's notion of the biological species concept (BSC), but the concept did not fit as well with plants. Weighing in at 643 pages and with a bibliography of more than 1,250 references, *Variation and Evolution in Plants* was the longest of the four books associated with the evolutionary synthesis. The book received instant recognition for its ambitious synthesis of a broad range of research areas, which opened up a new field of research for younger scholars who would consider themselves as plant evolutionary biologists. Of great significance is that *Variation and Evolution in Plants* effectively killed any serious belief in alternative mechanisms of evolution for plants, such as Lamarckian evolution or soft inheritance, which were still upheld by some botanists. *Variation and Evolution in Plants* has been assessed as "the most influential single book in plant systematics this century" (Raven, 1974).

Stebbins's second most important book was *Flowering Plants: Evolution Above the Species Level,* published in 1974, based on the Prather Lectures he gave at Harvard, which may be seen as an update of his 1950 book. Other books included a widely adopted undergraduate textbook on evolution, *Processes of Organic Evolution,* which went through three editions from 1966 to 1977, and a more advanced text written with Dobzhansky, Francisco Ayala, and James Valentine, *Evolution,* published in 1977. In 1965 he and Herbert Baker edited *The Genetics of Colonizing Species,* derived from papers presented at an Asilomar Conference. Additional books are *The Basis of Progressive Evolution* (1969); *Chromosomal Evolution in Higher Plants* (1971), which was also adopted as an advanced textbook; and his 1982 semi-popular *Darwin to DNA: Molecules to Humanity.* He published more than 250 scientific papers.

In 1950 Stebbins accepted an invitation from the university to establish a department of genetics at the Davis campus, which became his home until his death. He loved Davis. He remained as chairman of the genetics department until 1963. Stebbins was proud of his work at Davis, proud of the growing campus as it matured, pleased with his own contributions, and was always contented living there. He was particularly proud of having attracted to Davis from Rockefeller University in 1971 his admired friend Dobzhansky and his associate Francisco J. Ayala (Raven, 2000). He was the major professor of 33 graduate students. He enjoyed teaching undergraduate students, who received his courses with enthusiasm, which very much pleased Ledyard. He taught a popular evolution course to several hundred students every year. He was an engaging teacher. When he delivered his last lecture in 1973, a few months before retirement, he received a long standing ovation. Stebbins cared deeply about the teaching of evolution, and in the early and mid-1960s worked closely with other biologists involved in the Biological Sciences Curriculum Study (BSCS), which published textbooks for the teaching of evolution in U.S. high schools. He actively fought the rise of “scientific creationism” groups in California and in the nation. Between 1960 and 1964 he served as secretary general to the International Union of Biological Sciences.

Stebbins was also an early conservationist. In 1967, while president of the California Native Plant Society, he was influential in efforts to conserve native plants and habitats. He organized weekly field trips that got people into the habit of the conservationist's credo of "taking nothing but pictures, leaving nothing but footprints." In 1967 he prevented the destruction of a raised beach on the Monterey peninsula that he dubbed Evolution Hill, now called the S. F. B. Morse Botanical Area, where Stebbins said all the problems and principles of evolution could be seen played out among the plant species. Stebbins had an encyclopedic knowledge of plant taxonomy, and had particularly detailed knowledge of the flora of California, which he enjoyed displaying with students, colleagues, and friends. In the obituary published by the *New York Times* on January 21, 2000, one of us (F.J.A.) is quoted: "Stebbins seemed to know every plant in the world, not just scientifically, but personally."

Stebbins retired in 1973 but remained active for 20 more years, conducting research and publishing papers and books. After retirement Professor Stebbins was visiting professor at the Universidad de Chile in Santiago in 1973, which was sponsored by the Convenio of the University of Chile and the University of California, and a visiting professor at the Australian National University in 1974, which was sponsored by the Australian American Educational Foundation. In 1975 he spent six weeks in the Soviet Union visiting scientific institutions, as a fellow of the Exchange Program, National Academy of Sciences, and the Soviet Academy of Sciences. In 1974 he was research scientist and visiting professor in Montpellier, France, sponsored by the Commission Nationale des Recherches Scientifiques (CNRS). He was also at various times visiting professor at the following institutions: Carleton College, Northfield, Minn.; San Francisco State University; Ohio State University; St. Olaf College, Northfield, Minn.;

and Universidad de Leon, Leon, Spain. Stebbins's strong personality spilled over into his professional life. He was industrious, intensely focused, and always enthusiastic. He seemed constantly excited by some new insight that usually came out of his voracious reading. The new insight usually made its way into his latest project almost immediately. He loved following the work of younger people and supported them generously. At times he seemed almost desperate to please people who mattered to him. At other times, however, he could be self-absorbed and insensitive to the thoughts and wishes of people around him. His whole life was evolution and evolutionary botany. Working in the field, he would pay no attention to the proper time of eating or anything else. Often he would be oblivious to the world around him. An episode involving Stebbins and one of us (F.J.A.) is the following. We were collecting in Pope Valley, near Napa, and in the process we killed a rattlesnake with a stick. Not knowing what to do with it, we put it on the hood of the car we were sharing. We continued working. After it grew dark, Ayala drove the two of them back to Davis. (Ledyard was kept from driving, whenever possible, because he was prone to see some hybrid plant by the roadside and forget about keeping the car on the road.) The next morning Stebbins drove the car 60 miles to UC Berkeley, delivered a lecture, and drove it back home to Davis with no notice of the rattlesnake that was still resting on the hood. Upon his return, he told Ayala, "I think something strange is wrong with this car. When I came out of the lecture, about 30 students were standing around looking at it."

By the second half of the 1940s Stebbins was emerging as a leader in evolutionary biology. He was an active member of the recently established Society for the Study of Evolution and became its third president in 1950. National and international recognition in the form of awards and honors would pile up over the years. His election to the National Academy of Sciences in 1952 was followed by elections to the American Philosophical Society, the American Academy of Arts and Sciences, the Royal Swedish Academy of Sciences, the German Academia Leopoldina, and the Linnaean Society of London, among others. He received the Lewis Prize of the American Philosophical Society in 1960, the Verrill Medal of Yale University in 1967, and the Gold Medal of the Linnaean Society of London in 1973. In December 1979 he was awarded the National Medal of Science, by President Carter. In 1980 the University of California regents named a UC natural reserve in his honor, the Stebbins Cold Canyon Reserve, a 577-acre parcel about 20 miles from the Davis campus, which Stebbins received with enormous joy as well as honor, and where he frequently went to botanize until the last few years of his life.

The coming of the new millennium served as the opportunity to appreciate Stebbins's contribution to the history of the biological sciences. In 1998 a past-president's symposium in honor of Stebbins titled "G. Ledyard Stebbins and Evolutionary Biology in the Next Millenium" was held at the Baltimore, Maryland, meetings of the Botanical Society of America; in 1999 he was honored with a special ceremony at the banquet of the XVI International Botanical Congress in St. Louis, Missouri, where he received the American Institute of Biological Sciences Distinguished Service Award for 1999, with zoological colleague Ernst Mayr. Ledyard was able to attend this ceremony and in a moving speech delivered from the confines of his wheelchair, he reflected on a lifetime of experiences as a botanist; it was to be his last public appearance. A colloquium ("Variation and Evolution in Plants and Microorganisms: Toward a New Synthesis 50 Years After Stebbins") sponsored by the National Academy of Sciences was held January 27-29, 2000, in Irvine, California, at the Arnold and Mabel Beckman Center of the National Academies. The 17 papers presented at the colloquium were published in *Proceedings of the National Academy of Sciences* (97:6941-7057) and as a separate book (*Variation and Evolution in Plants and Microorganisms)* edited by F. J. Ayala, Walter M. Fitch, and Michael T. Clegg, published by the National Academy Press in 2000. The colloquium celebrated the fiftieth anniversary of the publication of Stebbins's classic 1950 book by examining current knowledge about the same topics of the 1950 book plus some related subjects that have become subjects of investigation owing to recent advances. Ledyard, although frail for the last few years, intended to attend the colloquium. Alas, he became ill about one month before the colloquium was held and died on January 19, 2000, two weeks after his ninety-fourth birthday.

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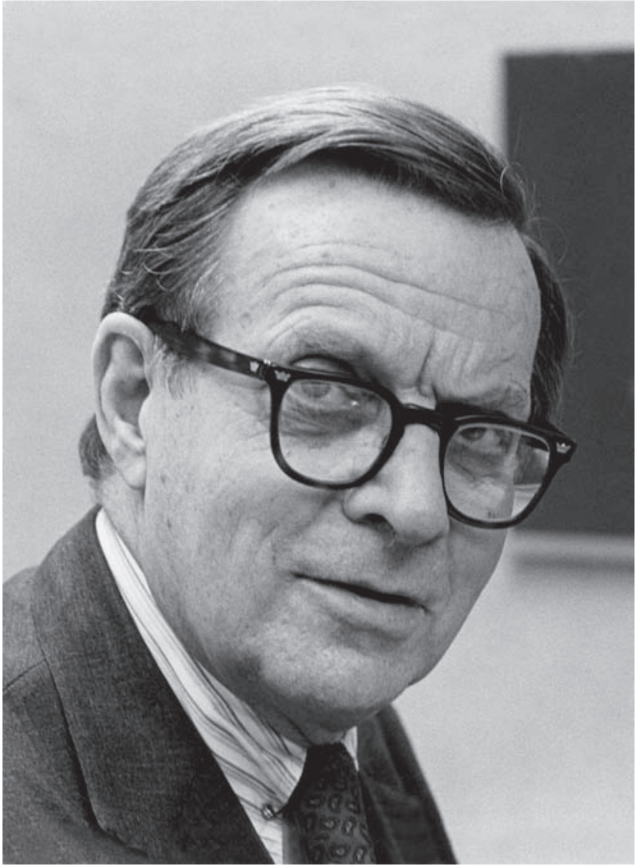

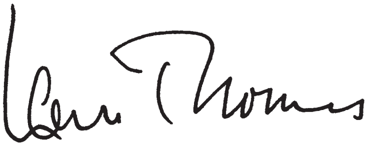

LEWIS THOMAS

November 25, 1913–December 3, 1993

BY GERALD WEISSMANN

THE LIFE OF LEWIS THOMAS spanned a golden age of American medicine, an era when, in his words, "our oldest art became the youngest science." Thomas played a major role in that transformation; he was known among scientists as an innovative immunologist, pathologist, and medical educator. He became known to the wider public as a deft writer whose essays bridged the two cultures by turning the news of natural science into serious literature. Witty, urbane, and skeptical, he may have been the only member of the National Academy of Sciences to win both a National Book Award and an Albert Lasker Award. He is certainly the only medical school dean whose name survives on professorships at Harvard and Cornell, a prize at Rockefeller University, a laboratory at Princeton, and on a book (*The Lives of a Cell)* that is eleventh on the Modern Library's list of the best 100 nonfiction books of the century.

Thomas made several important discoveries in the field in which he was a pioneer, immunopathology. He found that neutrophils were important mediators of fever and shock brought about by bacterial endotoxins or antigen/antibody reactions; these launch cascades of limited proteolysis in the blood. Therefore, if animals are depleted of neutrophils or given heparin they are protected against tissue injury as in the Arthus reaction or Shwartzman phenomenon. He made the dramatic observation that intravenous papain causes collapse of rabbit ear cartilage; similar damage results when excess of vitamin A induces host cells to release endogenous proteases. The search for endogenous, papain-like ferments pointed to neutrophils, complement, and immune complexes as the culprits in rheumatoid arthritis. With Philip Y. Paterson he contributed to our understanding of acute allergic encephalomyelitis, and he teamed up with H. Sherwood Lawrence and John David to define soluble mediators of delayed hypersensitivity: the first inklings of what we now call "cytokines." A prescient suggestion, published only in conference proceedings years before the HIV pandemic, was that our immune system constantly surveys our body to find and destroy aberrant cancer-prone cells; we now attribute Kaposi's sarcoma and other AIDS-related tumors to defects in Thomas's "immune surveillance." Those discoveries resulted from a very intense period of bench research (1946-1964) at Johns Hopkins, Tulane, Minnesota, and NYU before he turned his attention to broader issues of medical education and to his writing.

THE EDUCATION OF LEWIS THOMAS

Lewis Thomas grew up as a bright lad in a loving family in a comfortable house in Flushing, Queens. His father, Dr. Joseph Simon Thomas (Princeton, 1899; Columbia P&S, 1904), was a good-natured, hard-working doctor who had met and married the love of his life, Grace Emma Peck of Beacon Falls, Connecticut, at Roosevelt Hospital, where she was a nurse and he was an intern. They were married in New York City on the 30th of October in 1906, and thereafter, in the words of her son, Emma Peck's nursing skills were "devoted almost exclusively to the family."

Lewis Thomas was born on November 25, 1913. As were his three older sisters and younger brother, Lewis was sent to the local schools. But soon the family decided that Flushing High School was not quite ready to prep another Thomas for Princeton. After three semesters in Queens, Lewis Thomas transferred to the McBurney School, a less than exclusive prep school in Manhattan. He graduated in 1929 in the top quarter of his class. Medical practice was to protect the Thomas family against the worst of the Great Depression, which began on Black Tuesday, exactly one month after 15-year-old Lewis left for Princeton in September 1929.

At Princeton he "turned into a moult of dullness and laziness, average or below in the courses requiring real work." He took little interest in physics or inorganic chemistry and dismissed athletics as a general waste of effort. By reason of youth and family standing, he ranked low in the eating club hierarchy of prewar Princeton and was grateful to find safe haven at Key and Seal, a club that was literally the furthest out on Prospect Avenue. But high spirits and natural wit brought him to the offices of the *Princeton Tiger,* where Thomas soon published satires, poems, and parodies under the *nom de plume* of ELTIE. "After the crash of '29, we were in thrall to Michael Arlen; we slouched around in Oxford bags and drank bootleg gin from the tub like Scott and Zelda," Thomas recalled. "They told us we'd go out like a light from that stuff. Out like a light. I think I did a piece on bootleg gin for the *Tiger* about that." He had; it's unreadable. Then on one winter weekend visit to Vassar in 1932, Lewis Thomas met a young freshman from Forest Hills. Her father was a diplomat, her name was Beryl Dawson, and after years of separation for one or another reason they were married a decade later. By then the moult had spread its wings.

Years later his editor, Elisabeth Sifton, asked me, "When was it that Thomas became so wise?" Thomas attributed his metamorphosis to his senior year at Princeton and a biology course with Professor Wilbur Swingle. Swingle's discovery of a life-saving adrenal cortical extract—a crude version of deoxycorticosterone—had won wide acclaim. Thomas recalled that Swingle sparked his lifelong interest in the adrenals. Swingle also introduced him to Jacques Loeb's literary/philosophical speculations on ions and cell "irritability" in *The Mechanistic Conception of Life* (1916). Five years out of Princeton, young Thomas would sign up to work with Jacques Loeb's son.

In his senior year the Depression hit home and Thomas knew that getting into medical school was one solution to the unemployment problem. He also confessed he had a leg up on other applicants: "I got into Harvard . . . by luck and also, I suspect, by pull. Hans Zinsser, the professor of bacteriology, had interned with my father at Roosevelt and had admired my mother, and when I went to Boston to be interviewed in the winter of 1933 [Zinsser] informed me that my father and mother were good friends of his, and if I wanted to come to Harvard he would try to help."

Help he did and Thomas entered Harvard at the age of 19 in the fall of 1933. Thomas's career at the Harvard Medical School turned out just fine; he received grades far better than at Princeton. When asked in 1983 which member of the Harvard faculty had the greatest influence on his medical education, Thomas replied, "I no longer grope for a name on that distinguished roster. What I remember now, from this distance, is the influence of my classmates." Nevertheless, some on that roster made a lasting impression. Hans Zinsser in bacteriology showed that it was possible to function both as a laboratory scientist and a respected writer; Walter B. Cannon in physiology taught him that the details of homeostasis held the keys to "*The Wisdom of the Body";* David Rioch in neuroanatomy had him build a wire and plasticene model of the brain, which Thomas trekked about for 15 years; and in Tracy Mallory's office Lewis Thomas came across a pickled specimen that "like King Charles's head" would haunt his investigative career for decades to come.

At one of Mallory's weekly pathology seminars in the depths of Massachusetts General Hospital, Thomas leaned back in his chair and by accident knocked over a sealed glass jar containing the kidneys of a woman who had died of eclampsia. Replacing the jug, he noted that both organs were symmetrically scarred by the deep, black, telltale marks of bilateral renal cortical necrosis. Thomas remembered having seen something like those pockmarked kidneys before. They had been provoked in rabbits by two appropriately spaced intravenous injections of endotoxin: It was called the generalized Shwartzman phenomenon, and he would tussle with it for the rest of scientific career.

Thomas graduated cum laude from the Harvard Medical School in 1937 and began an internship at the Harvard Medical Service of the Boston City Hospital. A history of the Harvard Medical Unit at Boston City Hospital documents that of the 71 young physicians who trained there between 1936 and 1940, 52 became professors of medicine, while 6 went on to the deanships of medical schools. Thomas waxed eloquent on those days: "I am remembering the internship through a haze of time, cluttered by all sorts of memories of other jobs, but I haven't got it wrong nor am I romanticizing the experience. It was, simply, the best of times."

He remained at Boston City until 1939, when the confluence of his interests in neurology, adrenal hormones, and the Loeb mystique brought him to New York. Halfway through his internship in Boston, Thomas heard that Dr. Robert F. Loeb was becoming director of the Neurological Institute in New York and resolved to study with him because "Loeb was a youngish and already famous member of the medical faculty in the Department of Medicine at P&S, recognized internationally for his work on Addison's disease [and] the metabolic functions of the adrenal cortex and the new field of salt and water control in physiology."

He served as a neurology resident (his only specialty training) and research fellow at P&S from 1939 to 1941, with time out to marry Beryl at Grace Church in New York in January 1941. Robert Loeb abruptly moved to the chairmanship of medicine, but Thomas found that there was a fellowship with John Dingle awaiting him back at the Thorndike and jumped at the chance.

Almost as soon Lew and Beryl had established themselves back in Boston, Thomas was sent by Dingle on a month-long medical mission to Halifax, where an outbreak of meningococcal meningitis had struck the wartime port. Beryl served as lab assistant. Those four weeks in the field, a publication of the effects of sulfadiazine in meningitis, and a thorough analysis of the prozone phenomenon in Dingle's lab were a prelude to a naval commission after Pearl Harbor. Thomas reported in March 1942 to the Naval Research Unit at the Hospital of the Rockefeller Institute and on January 12, 1945, landed with a detachment of that unit headquartered on Guam. Thomas and Horace Hodes were put to work on Japanese B encephalitis on Okinawa and quickly identified horse blood as a reservoir for the virus.

At war's end, August 1945, Thomas was left with no further official tasks. The unit had unused research equipment, an ample supply of laboratory animals, culture media, and stock microbes. So Thomas went to work on a problem of major interest to him and the Navy, the pathogenesis of rheumatic fever. He tried to reproduce rheumatic myocarditis by injecting rabbits with streptococci plus or minus ground-up heart tissue. These experiments continued until he was demobilized in January 1946 and led directly to his most stunning observation.

LEWIS THOMAS THE SCIENTIST

Most scientists have one discovery that is dearest to their heart; for Thomas it was the use of the floppy-eared rabbits. He first put it all together in 1955 on the fifth floor of the Medical Science Building of New York University. As NYU's new professor of pathology, Lewis Thomas (age 42) pulled an albino rabbit out of the cage, turned to the group of second-year medical students perched on stools around the bench, and asked them, "Notice anything?"

They didn't, immediately. "It's a healthy bunny, if that's what you mean," one of students volunteered.

The professor smiled in reply, "You know, I didn't notice anything either when I first did this a few years ago. But last night, I gave this little fellow some papain by vein. Let's sit him next to one that hasn't been injected with papain. Here. Look."

He pulled out another rabbit. "Here's the control." Finally, he reached for the third. "And here's another rabbit I also injected with papain."

The students looked at the two papain bunnies side by side with the control. Now came the burst of recognition.

"Of course. Gosh! The papain bunnies' ears are droopy!"

"I'll be darned. They look like dachshunds!"

"No, spaniels."

"What is papain?"

"What made you inject the animals with papain?"

"Is it cartilage that's wilting?"

Lewis Thomas did his best to answer the questions. He'd done this many times before and it happened like clockwork. Sure, the rabbits will be fine. In a day or so the droopy ears will become erect again. Three days later, you won't be able to tell the papain bunnies from the controls. No. He's found no other ill effects of any kind in rabbits given papain. He has looked at sections of ears from the injected animals for days on end under the microscope and found nothing of interest. Nothing. Papain? It's a proteolytic enzyme from the papaya plant. A protease best known as a meat tenderizer. Only cartilage? He didn't know. He guessed that cartilage is a "quiet, inactive tissue." But why inject rabbits with papain in the first place? Well, he told the students, it's a long story, but it's still the most reproducible phenomenon he's ever seen in the lab. He told them that he "couldn't really explain what the hell was going on." But the students' questions led to him to the answer. "I was in irons on my other experiments. I was not being brilliant on my other problems. . . . Well, this time I did what I didn't do before. I simultaneously cut sections of the ears of the rabbits after I'd given them papain and sections of normal ears. This is the part of the story I'm most ashamed of. It still makes me writhe to think of it."[1]

He didn't know what the hell was going on because he hadn't done the controls! Doing it right took quite an effort. Two hundred and fifty rabbits were sacrificed, hundreds of sections were taken, but in the end the differences under the microscope were clear and striking. Although sections from the ears of papain-treated rabbits showed perfectly normal cells, the basophilic, metachromatic matrix between cells seemed to have melted away. But papain had no effect on the cells themselves and there was no evidence of cellular infiltration. Thomas correctly deduced that the dramatic ear droop was a direct attack by papain on cartilage matrix, "the chondroitin sulfate or component to which it is bound."

On the morning after the rabbits had received papain, most of the matrix had been leached out. Happily, when the ears snapped back to normal in a few days, the bluestaining material was back in force. Thomas figured out that cartilage, far from being a dead or inert tissue, could survive a withering attack and make new matrix by the earful. Thomas immediately understood the implications of this finding. Perhaps, he reasoned, tissue injury in general was due to the uncontrolled release of the body's own papain-like proteases, whether released from cells of the tissue itself or from white cells escaping from the circulation. He was ready to go to press. In 1956 the work appeared in the *Journal of Experimental Medicine,* and its opening lines became an instant classic of scientific description.

> For reasons not relevant to the present discussion rabbits were injected intravenously with a solution of crude papain, and the following reactions occurred with unfailing regularity: Within 4 hours after injection, both ears were observed to be curled over at their tips. After 18 hours they had lost all of their normal rigidity and were collapsed limply at either side of the head, rather like the ears of spaniels. After 3 or 4 days, the ears became straightened and erect again. . . . Apart from the unusual cosmetic effect, the animals exhibited no evidences of systemic illness or discomfiture, and continued to move about after the fashion of normal animals of the species.

Thomas also noted that cortisone, given after papain, kept the animals' ears limp: strong proof that cortisone inhibited resynthesis of what we now call "proteoglycans." When cortisone inhibits the synthesis of proteoglycans and/or collagen in humans, osteoporosis results. Thomas's rabbits taught us why.

The "unusual cosmetic effect" was pictured in newspapers countrywide: "An accidental sidelight of one research project had the startling effect of wilting the ears of rabbits," wrote the *New York Times.* The droopy-eared rabbits became a Picture of the Week in *Life* magazine and reporters flocked to NYU wanting to know what the research project was that these bunnies were the sidelight of. Why was that research "not relevant to the present discussion?" Why papain?

The papain story began in Guam. Thomas found that rabbits receiving a mixture of streptococci and heart tissue became ill and died within two weeks; histologic sections of their hearts showed lesions that frankly resembled the myocarditis of rheumatic fever. Control rabbits injected with streptococci alone or with heart tissue alone remained healthy and showed no cardiac lesions. Thomas was entirely confident that he had solved the whole problem of rheumatic fever. He hadn't. On his return to the Rockefeller Institute in New York, he couldn't repeat those experiments, sacrificing "hundreds of rabbits, varying the dose of streptococci and heart tissue in every way possible." He was vastly relieved that he hadn't rushed into print on the basis of those rabbits on Guam.

Thomas's first faculty position after discharge from the Navy in 1946 was as an assistant professor of pediatrics at the Harriet Lane Home for Invalid Children of Johns Hopkins. Thomas tried once more to repeat those rabbit experiments, mixing streptococci and heart tissue with Freund's adjuvants. Bad news for Assistant Professor Thomas: The rheumatic fever experiments failed once again. But Thomas could not shake off those experiments that had worked so well on Guam. Perhaps the host—the rabbits in Guam, for example, but not those in New York or Baltimore—had been "prepared" by an earlier insult as by that endotoxin prep in the Shwartzman phenomenon.

He tackled the problem with Chandler ("Al") Stetson, a lifelong friend who was to become his colleague in Minnesota and his successor as the professor of pathology at NYU. Thomas and Stetson "prepared" rabbits with endotoxin from meningococci. The prepared skin had an excess of lactic acid, and they reasoned that lactic acid might activate tissue proteases, the cathepsins. But they were neither able to measure cathepsin activity nor obtain purified cathepsins, so they injected rabbits with off-the-shelf trypsin or papain from papaya pulp. Trypsin was ineffective, but papain produced lesions in the skin that looked very much like the local Shwartzman reaction.

When Thomas left Hopkins, he took the problem with him. He served a brief stint at Tulane, where he became a professor of medicine and the director of the Division of Infectious Disease. He was diverted for a while by studies of humoral antibodies in allergic encephalomyelitis but returned to rheumatic fever when he was appointed as American Legion Professor of Pediatrics and Medicine at the University of Minnesota in 1951. In quick time he put together a team of young investigators, most of whom were soon at work on the Shwartzman phenomenon and the streptococcus: Robert Good, Floyd Denny, Lewis Wannamaker, Richard Smith, and Joel Brunson. Al Stetson came on board as well.

He reverted to the notion that proteases, either secreted by the streptococcus or released from the victims' own cells, caused damage in a "prepared" heart or joint. With a young Minnesota pediatrician, Robert A. Good ("the smartest investigator I ever met," he once told me), he found out that if one removed white cells from the Shwartzman equation (e.g., by nitrogen mustard), kidney injury would be prevented. The kidneys were also spared if one gave heparin, which prevented blood vessels from becoming plugged by fibrin, platelets, and white cells. Good and Thomas suggested that "a combination of humoral and cellular factors made by the host caused the tissue injury." Nowadays we invoke anaphylatoxins, Toll receptors, signal transduction, apoptosis, caspases, and cytokines to explain the Shwartzman phenomenon. But in the 1950s Good and Thomas had provided a satisfactory explanation and the flow of satisfying, explanatory papers followed Thomas from Minnesota as he moved to NYU in 1954.

Thomas was recruited to NYU by Colin McLeod to become professor and chairman of the Department of Pathology. He was delighted to return to the metropolis with Beryl and his three daughters, Abigail (b. 1941), Judith (b. 1944), and Elizabeth (b. 1948), and to set up his household at Sneden's Landing, a small town up the Hudson from the city. He remained at NYU for 15 years and proceeded to turn it into a world center of immunology, first in pathology (1954-1958), then as professor and chairman of the Department of Medicine at NYU-Bellevue Medical Center; director of III and IV medical divisions, Bellevue Hospital (1958-1966); and finally as dean of the New York University School of Medicine and deputy director of NYU Medical Center (1966-1969). Over those years he attracted and/or trained a legion of scientific stars and superstars at NYU: Frederick Becker, Baruj Benacerraf, John David, Edward Franklin, Emil Gottschlich, Howard Green, H. Sherwood Lawrence, Robert T. McCluskey, Peter Miescher, Victor and Ruth Nussenzweig, Zoltan Ovary, Stuart Schlossman, Chandler Stetson, Jeanette Thorbecke, Jonathan Uhr, and Dorothea Zucker-Franklin. Thomas's international colleagues were frequent visitors: Sir Macfarlane Burnett, Dame Honor Fell, Philip Gell, James Gowans, Sir Peter Medawar, Thomas Sterzl, and Guy Voisin.

Early on in his NYU days Thomas hit a rough patch. Whereas cortisone, the miracle drug, clearly stopped inflammation in the clinic, Thomas was astonished to find that cortisone not only proved ineffective against the Shwartzman phenomenon but also actually provoked it. This puzzle took the wind out of his sails. He was indeed "in irons on his other experiments" and "not being brilliant." Then came the floppy-eared bunnies, as he later explained: "I was able to justify working on so seemingly frivolous a problem by the possibility that one might figure out how cortisone might work. But, I was obliged to confess, despite this, that the work had been done because it was amusing."

After papain, new discoveries proceeded apace. If an exogenous protease caused connective tissue damage, where might endogenous proteases reside? Thomas spent a summer with Dame Honor Fell, director of the Strangeways Research Laboratory in Cambridge. Fell had been studying vitamin A and had found that it produced depletion of cartilage matrix in mouse bone rudiments growing in a dish. Fell and Thomas decided to trade experimental systems. They first added papain to the little bone cultures in the dish and were able to produce vitamin A-like lesions in mouse cartilage. Thomas then returned to NYU to do the reciprocal experiment. With Jack Potter and R. T. McCluskey, Thomas and I stoked rabbits full of vitamin A and sure enough: twenty-four to forty-eight hours later, their ears drooped as if they had been given papain. We were convinced then that Vitamin A in some fashion released an endogenous papain-like enzyme from cartilage cells and that this enzyme proceeded to break down the extracellular matrix. At the time we supposed that the enzyme was present in lysosomes, recently described by Christian de Duve. We suggested that vitamin A had ruptured the walls around these organelles, and that cortisone and its analogues must therefore stabilize the lysosomes.

These days the answer is more complicated. Nowadays we believe that metalloproteinases are released from cells and that synthesis of these proteases is under opposing transcriptional control by vitamin A and cortisone acting via well-defined cytoplasmic and nuclear receptors. Cortisol receptors recognize palindromes of DNA, vitamin A receptors see tandem response elements of DNA, there are at least two types of glucocorticoid receptors, these antagonize fos/jun transcription factors, and so on and so on in abundant detail. It all seemed simpler a generation ago. But these experiments, the last in which Thomas played a hands-on role, pointed the way for Thomas's students and their students to elucidate the roles of anaphylatoxins in neutrophil activation, of oxygen-derived free radicals in tissue injury, of lymphokines (now cytokines) such as MIF and IL-1 in cartilage catabolism, of glucocorticoid action in inflammation via NFkB, and, as a follow-up of the cortisone/lysosome experiments, the description and clinical development of liposomes.

LEWIS THOMAS THE STATESMAN

Thomas had a broad interest in how medical science shapes, and is shaped by, society. Wit, candor, and attention to principle rather than politics made him a valuable spokesman for medical science. While still at NYU, Thomas served as a member of the New York City Board of Health (1957-1969), was instrumental in the construction of the new Bellevue Hospital, and set up the Health Research Council, a sort of local National Institutes of Health. As chairman of the Narcotics Advisory Committee of the New York City Health Research Council, he guided Vincent P. Dole into methadone research and pointed Eric Simon to endorphins (1961-1963). After a stint in New Haven as a professor of pathology and dean (1969-1973) at Yale University School of Medicine, he became president and chief executive officer of the Memorial Sloan-Kettering Cancer Research Center (1973-1980). At MSK he launched a major attack on tumor immunology, recruiting Robert Good as director; Thomas became chancellor of MSK from 1980 to 1983. In retirement, his summer home in the Hamptons made a University Professorship at SUNY-Stony Brook (in 1984) convenient; his Manhattan apartment let him serve as writer in residence at the Cornell University Medical School.

His honors were legion. Lewis Thomas was a member of the National Academy of Sciences, the American Academy and Institute of Arts and Letters, and the American Academy of Arts and Sciences. He served as a Phi Beta Kappa scholar at Harvard, won the Woodrow Wilson Award at Princeton, an award in literature from the American Academy and Institute of Arts and Letters, and the National Book Award in 1973. He was president of the New York Academy of Sciences, received the Kober Medal of the Association of American Physicians, the Britannica Award from the encyclopedia itself, and a Lasker Award for Public Service to Science. His last accolade, before a stoic death of macroglobulinemia, was the first Lewis Thomas Prize from Rockefeller University, "honoring the scientist as poet."

LEWIS THOMAS THE WRITER

Thomas was for several decades the most widely read interlocutor between the older literary culture and the new world of medical science, preceded in this role by such other American physician-writers as Oliver Wendell Holmes, William James, Walter B. Cannon, and Hans Zinsser. Thomas's literary career began modestly enough in 1970 as the result of an after-dinner speech for an inflammation symposium at Brook Lodge. After the usual fawning tribute from the chairman, Lew mounted the podium, murmured, "Thank you, I think," and proceeded to knock the somnolent inflammation boffins out of their seats. Accustomed to the passive voice of dreary fact, they heard instead Thomas making more sense of inflammation in 50 minutes of elegant prose than had prolix lecturers on endotoxin or macrophages in the preceding 16 hours. The talk was printed; someone brought it to the attention of Franz Ingelfinger, editor of the *New England Journal of Medicine*; and Thomas began his career as author of the bimonthly "Notes of a Biology Watcher." Thanks to Elisabeth Sifton, then an editor at Viking Press, those marvelous essays were soon collected into *Lives of a Cell;* the volume became a best-seller and won a National Book Award—and the rest is history. Here, for example, is Thomas's suggestion for signals we might send from Earth to announce ourselves to whatever life there might be in outer space.

> Perhaps the safest thing to do at the outset, if technology permits, is to send music. This language may be the best we have for explaining what we are like to others in space, with least ambiguity. I would vote for Bach, all of Bach, streamed out into space, over and over again. We would be bragging, of course, but it is surely excusable for us to put the best possible face on at the beginning of such an acquaintance. We can tell the harder truths later.

That sort of writing was the product of a unique period in American culture. Thomas and his colleagues were educated in colleges at which the liberal arts were still firmly in place, and John Dewey's learning by doing had moved from primary schools into the universities. It was an era when those who did medical science were expected to make only modest claims for their success. "I was lucky," Thomas quipped after he received a gold medal at Bologna in 1978. "Chance favored the prepared grind." He believed that one could do serious work without taking oneself too seriously.

Thomas's chosen means of expression was the informal essay, a literary form that accommodates many topics but always has the mind of its author as the subject. Thomas was as likely in print as on the wards to pair epiphany (à la James Joyce) with entropy (à la the second law of thermodynamics, or $\Delta S > q/T$). In Thomas's prose, epiphany seemed to be having it out with entropy on every page. He would point out that the *Oxford English Dictionary* defines grammar as a body of statements of fact, a science if you will. But a larger portion of grammar spells out the *rules* of practice and therefore ought to be considered an "art." Thomas was convinced that medicine was like grammar, a hybrid of science and art united by syntax.

He was sparing of words when fewer spoke louder. When he received that last award at Rockefeller University, he was confined to a wheelchair. He declined to go to the podium and apologized to the audience for "not rising to the occasion." About the same time, I had reached him on the telephone. "How are you doing?" I asked. He knew what I was asking.

"So," he replied.

"What do you mean by 'so'?"

"Well," said Thomas, "in my family, there were only three ways of answering that question of yours. If things were going along splendidly, you'd answer 'fine.' If there were a bit of trouble around, you'd say 'so-so.' Right now, I'm 'so.'"

When more words were required, they flowed like wine. Thomas understood the very human need to turn the strands of fact into a fabric of belief. Fact marched hand in hand with solace; he assured us that a meningococcus with the bad luck to catch a human was in more trouble than a human who catches a meningococcus. His years in the lab served him well on the page. His sense of trial and error at the bench and in the clinic, of how cells divide, microbes hurt, and creatures die gave an edge to his writing.

> When injected into the bloodstream, endotoxin conveys propaganda, announcing that typhoid bacilli (or other related bacteria) are on the scene and a number of defense mechanisms are automatically switched on, all at once, including fever, malaise, hemorrhage, shock, coma, and death. It is something like an explosion in a munitions factory.

Prose of this rough measure supports the argument that Lewis Thomas has a shot at permanence in the world of letters. A number of his compositions stand up to essays by such other modern masters of the genre as E. B. White,

A. J. Liebling, and John Updike. In a select few Thomas reaches back to touch the mantle of Montaigne.

NOTE

1. B. Barber and R. Fox. The case of the floppy-eared rabbits: An instance of serendipity gained and serendipity lost. *Am. J. Sociol.* 64(1958):128-136.

ALL OTHER quotations by Lewis Thomas and other biographic details are taken from his memoir *The Youngest Science* (1983). They are also based on personal notes, as well as interviews with George Mirick, H. S. Lawrence, Beryl Thomas, and material generously forwarded by Baruj Benacerraf, P. Y. Patterson, Jonathan Uhr, Paul Marks, and others.

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A. E. Whitford

ALBERT EDWARD WHITFORD

October 22, 1905–March 28, 2002

BY DONALD E. OSTERBROCK

ALBERT E. WHITFORD WAS born, raised, and educated in Wisconsin, and then made his mark as an outstanding research astrophysicist there and in California. As a graduate student in physics he developed instrumental improvements that greatly increased the sensitivity of photoelectric measurements of the brightness and color of stars. For the rest of his life he applied these and later even better tools for increasing our knowledge and understanding of stars, interstellar matter, star clusters, galaxies, and clusters of galaxies, from the nearest to the most distant. He became a leader of American astronomy, and his counsel was sought and heeded by the national government.

Albert was born in Milton, Wisconsin, a little village halfway between Madison and Williams Bay, where Yerkes Observatory is located. When Albert was born, his father, Alfred E. Whitford, was the professor of mathematics and physics at tiny Milton College, and *his* father, Albert, for whom our subject was named, had been the professor of mathematics before him. Albert's mother, Mary Whitford, was his father's second cousin from Rhode Island, and he had one sister, Dorothy (later Lerdahl). Albert's Aunt Anna was the professor of German at the college, and his great-uncle, William C. Whitford, had been its first president.

Young Albert grew up in a highly academic, small-college environment.

He entered Milton College in 1922; by then his father had become its president but still taught some classes. The young scion of this family was a star student, and after completing his B.A. in 1926, he went on to graduate work in physics at the University of Wisconsin. The level of the courses there was considerably higher than at Milton. Charles E. Mendenhall was the long-time chairman of the department; he and most of the other professors concentrated largely on experimental physics while theorist Edward H. Van Vleck taught nearly all the more mathematical subjects. Albert's progress slowed down but he never had any trouble with the courses. He was a motivated student who knew how to study, learn the material, and retain and use it. He finished his M.A. in 1928 and his Ph.D. in 1932, six years after his bachelor's degree, considered a long time back then. But, he had done very good work and opened up a new field for himself along the way.

In 1929 at the start of his fourth year at Madison, Whitford learned that Joel Stebbins, the professor of astronomy and director of the university's Washburn Observatory, was looking for a physics graduate student assistant. Stebbins was the U.S. pioneer of electrical (or electronic) astronomical photometry, measuring the magnitudes (or brightnesses) and colors (color indices) of stars. He had begun with selenium photoconductive cells, and then switched to more sensitive photoelectric cells, placed at the focus of the telescope. In 1929 he was still using a sensitive quartz-fiber electrometer to measure the voltage generated by the weak current from the photocell, proportional to the brightness of the star. No doubt on the advice of his colleagues in the physics department, Stebbins had decided to try a more up-to-date technique. By 1929 research physicists were using vacuum tube circuits to measure weak currents, and he wanted to hire a knowledgeable graduate student to design and build a good one for him. Whitford, who had not gotten an assistantship in physics that year, needed a job and he gladly took it in astronomy.

Whitford got the assistantship, tackled the problem, and solved it. It was not easy, but he had the right experimental approach, seeking all the advice he could, reading the latest papers, wiring a trial amplifier setup, and modifying it until he had one that worked. He put the photocell in a vacuum-tight chamber and evacuated the air around it, thus reducing the noise due to electrons produced by cosmic rays striking nitrogen and oxygen molecules. With these improvements Whitford increased the sensitivity of Stebbins's photometer by a factor of nearly four, so that he could measure stars 1.5 magnitudes fainter than before. This made a tremendous increase in the number of accessible objects and correspondingly in the accessible volume of space. Whitford published a paper on these instrumental improvements as part of his Ph.D. thesis in 1932. Back then a physics thesis at Wisconsin required new experimental results to go with a new instrument design. It is not clear whether results from a telescope would have qualified, but in any case there was no time for that. Hence the other part of his thesis was a more traditional paper on laboratory spectroscopy of the Zeeman effect in K II, which he did with the physics department's large Rowland-circle photographic spectrograph.

When Whitford finished his thesis in 1932, the United States was in the midst of the Great Depression. Jobs were hard to find everywhere, especially in universities for fresh Ph.D.s. Stebbins could keep him a year as an assistant on the same job he had held as a graduate student, and was eager to do so. The older man, a highly creative research scientist and an excellent observer, was a tyro in electronics.

He knew that he could not keep the photometer and amplifier in operating condition by himself, but Whitford certainly could and, furthermore, would probably come up with still more improvements. Moreover, Stebbins, a power in U.S. science, promised to recommend Whitford for a National Research Council Postdoctoral Fellowship, to start in 1933. The young Ph.D., with no other options before him, readily agreed and decided to become an astronomer, at least for the time being. He began designing and building better amplifiers and cold boxes for the photometers in the string-and-sealing-wax type shop he had inherited in the basement of the old Washburn Observatory.

In 1932 planning for the 200-inch telescope was well underway in Pasadena, under the leadership of George Ellery Hale, who had secured the funds necessary to build it from the Rockefeller Foundation. He and everyone else connected with the project realized that the surprising velocity-distance relation for galaxies that Edwin Hubble had recently discovered with the 100-inch Mount Wilson reflector would be a key to understanding the nature of the Universe. Its study would be one of the big tasks for the 200-inch when it was completed, and Stebbins's photoelectric photometer would be one of the prime tools to carry it out. Stebbins understood this, too, and used it to gain access to the 60- and 100-inch telescopes, as preliminary work on this and other observing programs that would enable him and Whitford to use the instrument and thus gain experience and insights on how to make it better. With Hale and Stebbins's recommendations, Whitford got the NRC fellowship and spent two years at Caltech and Mount Wilson.

During his three postdoctoral years in Madison and Pasadena, Whitford learned astronomy from the ground up pretty much on his own. There was no graduate program in it at either Wisconsin or Caltech (except one astrophysics course at Caltech taught by Fritz Zwicky), and Whitford learned it by hard study, reading papers, and discussing them with Stebbins and the Mount Wilson astronomers. He kept a bridge open to physics too, doing a research project in laboratory X-ray spectroscopy of high stages of ionization of K (VI through IX) and Ca (VII and VIII). This was a specialty of Robert A. Millikan's physics laboratory at Caltech, where most of the experimental work was done by his former graduate student, currently his young faculty colleague, Ira S. Bowen (later director of Mount Wilson and Palomar observatories). Whitford got to know Bowen, as well as several of the Mount Wilson astronomers, especially Walter Baade and Hubble. The young postdoc lived in the "loggia," a large sleeping room for young single men upstairs in the Athenaeum, the very new Caltech faculty club. The friend Whitford always remembered from there was William A. ("Willy") Fowler, then a boisterous, outgoing physics graduate student, always the center of mischief and pranks designed to disturb the calm quiet that permeated the club but a star experimental student in the lab. Whitford also formed a lifelong friendship with Olin C. Wilson, who was a Caltech graduate student, received his first Ph.D. in astrophysics, and went on the Mount Wilson staff.

Whenever Stebbins could get leave from his teaching duties and come out to observe with the big Mount Wilson telescopes, Whitford would prepare for weeks in advance to be certain that all their apparatus was in working order and that supplies they would need were in place on the mountain. He would work at the telescope with Stebbins all night, troubleshooting and solving any electronic problems that arose. They were taking data with a galvanometer mounted rigidly in the dome, reading the voltage built up across a load resistor in a time signaled by a stopwatch, and recording the numbers by hand throughout the whole night. One of the first objects they measured at Mount Wilson was M 31, the Andromeda galaxy. Moving the telescope across it in right ascension and declination they traced how much larger it is, out to the faintest level, than is easily detectable on photographs. On his return to Madison in 1935 Whitford was promoted to research associate, a full-time position, and in 1938 to assistant professor of astrophysics, a new title at the time.

He continued observing with Stebbins, using the Washburn Observatory's 15.6-inch refractor whenever they could not get to Mount Wilson to use the bigger telescopes there. Chiefly they were measuring the "extinction" (absorption and scattering combined) of starlight by interstellar dust. This had been discovered by Robert J. Trumpler, using photographic photometry, but the photoelectric method Stebbins and Whitford had developed was much more accurate and thus provided better quantitative data on it.

In 1937 Whitford married Eleanor Whitelaw, whom he had first met as the sister of one of his fellow physics graduate students at Wisconsin, Neil Whitelaw. Born in Desoto, Kansas, she was a graduate of nearby Park College (Missouri); she earned a master's degree in education at the University of Chicago, where she worked in admissions before they were married.

In 1940, with war clouds looming over the United States, and France already fallen to Nazi Germany, Whitford was recruited as an experimental physicist to join the new Radiation Laboratory at MIT, the center for development of radar in the United States. He was convinced it was his duty to go. He, his wife, and their infant son, William, named for Albert's great-uncle, the first president of Milton College, moved to Cambridge, where he worked under wartime conditions until the end of 1945. The Whitfords' two daughters, Mary and Martha, were born there.

Albert had no time for astronomical research, though he did participate in writing parts of two papers with Stebbins on their results, all by correspondence. Two friends from the Wisconsin physics department, Ragnar Rollefson and Ray Herb, were at the Radiation Laboratory with him, and the three of them with their families kept in close touch with one another and with what was happening back in Madison. Very occasionally Whitford could get away from the laboratory for a few hours to attend an astronomy colloquium at Harvard, but otherwise he worked long hours six days a week. He made one extended field trip to England to install a new high-frequency radar system in antisubmarine search planes and to train their crews in using and maintaining them.

When the war ended, Whitford was tempted by a job offer from Los Alamos, and then by a faculty position at Purdue University, both in physics, but by then he had become an astronomer, he finally decided. Like F. W. Bessel a century before him, he "chose poverty and the stars." And in fact the University of Wisconsin's central administration, worried by predictions of a postwar recession and a drop in student enrollment, was not very welcoming. The first idea that President E. B. Fred floated was for Whitford to be given a joint appointment in astronomy, physics, and electrical engineering. He refused it, and Stebbins worked successfully to get him back full-time in astronomy. So Albert returned to Madison, still an assistant professor until the fall of 1946, when he was promoted to associate professor of astronomy. No doubt he suffered financially because of his strong attachment to Wisconsin.

He became more and more involved in measuring extinction by interstellar matter. The idea is simple: Light from a distant star shining through dust is absorbed or scattered more effectively at short wavelengths than at long, leaving radiation that has passed through it "reddened" because more blue light than red has been removed from it. We see this with our eyes whenever we look at the Sun close to the horizon, where the light is shining through a long path in the dust-laden air. Thus, comparing stars with the same intrinsic spectrum, the reddened one is the one whose light has passed through more interstellar matter containing dust, and the bluer is the one whose light has passed through less of it. Very nearby stars with no dust between them and us show their intrinsic colors, which are also the intrinsic colors of other stars "just like" them, that is, stars of the same spectral type.

Stebbins had begun this work with C. Morse Huffer, formerly his graduate student at the University of Illinois, who had come to Madison with him and in 1926 had earned the first Ph.D. in astronomy conferred by Wisconsin. Huffer had remained there on the faculty and observed eclipsing variables with Stebbins. Later they worked on interstellar extinction, but after Whitford's appointment as the junior member of the three-man astronomy faculty, Huffer had concentrated mostly on the variables. Stebbins's and Whitford's early observations were with two color filters, both in the blue spectral region where their photocells were most sensitive. Such measurements provided only a limited baseline for detecting interstellar reddening. As newer cells with extended sensitivity became available in the late 1930s, they started using their "six-color photometry," with bands ranging from the ultraviolet to the (near) infrared. During World War II, U.S. physicists developed highly sensitive infrared detectors for night warfare, and Whitford, aware of these advances, alerted Stebbins to their potentiality for astronomical research. They were hard to obtain and even harder to use effectively, but Whitford's contacts and electronic expertise were just what was needed. He and Stebbins used a PbS cell of this type, working at 2 μ, to penetrate through all the interstellar matter between us and the region around the center of our Galaxy. The early blue measurements of Stebbins, Huffer, and Whitford had proved that interstellar dust lies near the galactic plane, unevenly distributed in patches and "clouds," but that statistically the amount of it along a ray increases with distance. The total extinction along the line to the galactic center is about 25 magnitudes (a factor of 10^{10}) in the visual wavelength region, but is much smaller at 2 μ. Stebbins and Whitford traced the isophotes from their infrared measurements and showed the "bulge," the central part of the halo of our Galaxy, is analogous to the central part of the halo of M 31, seen from our vantage point outside it.

Stebbins retired in 1948 and on his strong recommendation, and the recommendations of several outside senior astronomers, Whitford was named director of Washburn Observatory and was promoted to full professor of astronomy. He analyzed all the photometric data they had on the wavelength dependence of the extinction and showed that it was, to a first approximation, the same in most directions out to all the distances they could measure, at most 2 kpc. This meant there was only one type of dust in those regions, at least in its extinction properties between 3000 Å and 2 μ. This "Whitford interstellar absorption curve" was used by astronomers studying galactic structure and the nature of interstellar dust for decades, until it was extended by new results in the ultraviolet spectral region obtained from orbiting telescopes above the Earth's atmosphere. Whitford was aware of a few anomalous regions in which the extinction was somewhat different, and mentioned them also.

Perhaps the least exciting photoelectric-photometry program that Stebbins and Whitford began, but also one of the most important ones, was measuring accurately the magnitudes of the stars in the North Polar Sequence. These had been adopted years earlier as the standards defining the photographic magnitude system. The idea was that they would always be observable, any time of the night or year, from anywhere in the Northern Hemisphere, and the sequence covered a large range in magnitude, from bright stars to very faint ones. In reality *one* star plus the definition of the magnitude scale are all that is needed, but the sequence was supposed to provide standard stars at all magnitudes that could be observed. It had been set up, mostly at Mount Wilson for the fainter levels photographically, but because of the nonlinearity and other difficulties of that method, it needed checking. Hubble and Baade wanted Stebbins to make this his top-priority observing program at Mount Wilson, and since he used the telescopes there as a guest observer, he listened seriously to what they said. They could use only the 60-inch reflector for this program, because the double-yoke mounting of the 100-inch telescope prevents it from reaching the north polar cap. Even with the 60-inch it is difficult to set onto a star near the North Pole, and time consuming to move from one star to the next. But they did the measurements whenever they could, and their results showed that in fact large errors existed at the faint end of the North Polar Sequence (with respect to the brightest stars in it, which were taken to define the zero point). These errors clearly affected the accuracy of Hubble's velocity-distance relationship, which depended on the measured magnitudes of the galaxies.

Baade favored setting up standard magnitude sequences to faint levels in a few of the more conveniently placed Selected Areas, defined originally by J. C. Kapteyn. Whitford continued this work with Harold L. Johnson, who was briefly at Wisconsin, appointed to the faculty position left open by Stebbins when he retired. They published the results for

Selected Areas 57, 61, and 68, the three considered best located in the sky by Baade in 1950. This program was then continued independently by William A. Baum, who had been added to the Mount Wilson and Palomar observatories staffs to do photoelectric photometry.

Undoubtedly the program Whitford liked best was measuring photoelectrically the magnitudes and colors of galaxies, still called "extragalactic nebulae" by most astronomers in the 1930s though they knew from the then-recent work of Hubble that they were galaxies and that our Milky Way Galaxy was one of them. It was a new, wide-open subject, the study of the whole Universe. Stebbins had tried to measure a few of the brightest galaxies in 1930, but his system, based on an electrometer, evidently was not sensitive enough to touch them, even with the 60-inch Mount Wilson telescope, for he never published any results from that trial. With the new amplifier Whitford had developed they could easily measure fairly bright galaxies. Whitford, who stayed in Pasadena most of the time from 1933 to 1935, measured the nearest, brightest galaxies on his own at Mount Wilson, using its photometer mounted on a 10-inch refractor, which was only infrequently used by other observers and thus usually available to him. He could experiment and tune up the apparatus as he collected data with it. This small telescope, with a correspondingly short focal length, was good for his program, because its small images of these nearby galaxies would fit into the diaphragm and the photocell. Hence he could measure these galaxies in just the same way he and Stebbins measured the apparently smaller, more distant galaxies with the larger 60- and 100-inch reflectors. The nearest of all, M 31, Whitford measured with a still smaller telescope, a 3.5-inch commercial lens with a correspondingly smaller focal length, and hence smaller image of the galaxy. In addition, Stebbins and Whitford traced across

M 31 with the 60-inch, integrating its light over its whole area.

Whitford's paper on the brightest galaxies, published in 1936, was his first solo publication on astronomical measurements. He and Stebbins published their paper on the big-telescope results in 1937; altogether it included 165 galaxies, together with the 11 in Whitford's paper. For many years these were the best magnitudes by far for calibrating the velocity-distance relationship. They had also measured color indices for most of these objects, which showed that as a group, E (elliptical) galaxies all had closely the same color, as did Sa and to a lesser extent Sb ("early-type") spirals. The Sc ("late-type") spirals were more heterogeneous, corresponding to the fact that they contain many more resolved supergiant stars than the other types. Stebbins and Whitford also showed that the few galaxies they could observe near the edges of the obscuring clouds of the Milky Way were reddened, just like the distant stars in our Galaxy they had previously observed, but there was no sign of intergalactic extinction.

After World War II Whitford concentrated more on galaxies and interstellar extinction with their new six-color photometric system, while Stebbins, approaching retirement, concentrated more on nearby stars and Cepheid variables. Together they found that E, Sa, and Sb galaxies tended to be brighter in both the ultraviolet and infrared spectral regions than G dwarf stars with similar colors in the blue and visual regions, and interpreted it as resulting naturally from a mixture of stars with all colors. As they measured galaxies with large redshifts (for that time, though very small in terms of the later-CCD era that Whitford lived into), they found the colors of the distant galaxies did not match those of the nearby ones, redshifted by the expansion, the so-called "K correction." The additional reddening and its interpretation came to be called the "Stebbins-Whitford effect." Theorists and cosmologists puzzled over it, as Whitford and Stebbins did, but it was an observed result, and they published it. Ultimately, in 1956, Whitford himself discovered the cause of the effect. It was not real, though their observational data were correct. The near ultraviolet spectra of galaxies, composites of stellar spectra, are full of discontinuities and groups of strong absorption lines, not smooth continua at all. Measured through a broad ultraviolet filter, they could not really be represented by a single effective wavelength, and it was impossible to calculate the true effect of the redshift on their light as if it were. Whitford and Arthur D. Code, who had replaced Johnson on the UW faculty in 1951, had built a one-channel scanning photoelectric spectrometer for use on the Mount Wilson telescopes, and with it Code obtained a scan of M 32, the nearby bright elliptical galaxy companion of M 31. Numerically redshifting this scan, which showed the discontinuities and absorption features in the near ultraviolet region, Whitford saw that it reproduced the observational data well. There was little if any Stebbins-Whitford effect in the distant galaxies, as Whitford, Code, J. Beverly Oke, and Allan Sandage subsequently confirmed in detail.

Whitford's other favorite, closely related observational program was the structure of our own Galaxy. His work was also linked to his studies of interstellar extinction, which hides distant objects near the galactic plane, as he knew better than anyone else. He had realized, even before World War II, that the spectral types assigned to distant OB stars by W. W. Morgan, observing at nearby Yerkes Observatory, were much more accurate than the older Harvard and Mount Wilson types. Whitford knew this because his color indices, reddening, and extinction determinations based on the Yerkes types showed less observational scatter than those based on the older types. From then on, Whitford and Stebbins used Morgan's types (which he furnished them before publication) and he used their extinction determinations. Morgan's first identification of spiral arms near the Sun in our Milky Way depended heavily on the Wisconsin photoelectric measurements, and Morgan, Whitford, and Code coauthored the definitive papers on this subject.

Likewise when Baade was able to locate large numbers of RR Lyrae variables close to the galactic center (itself hidden by large amounts of extinction right in the galactic plane), Whitford supplied the color measurements that made it possible to estimate the extinction along the line of sight to this concentration of Population II objects close to the nucleus. There were no normal OB stars observable there, but using the 100-inch telescope with his most advanced photoelectric systems, Whitford was able to measure the color of a globular cluster just beyond them. With the resulting extinction value Baade gave the distance to the galactic center as 8.3 kpc, a value that has held up for many years.

At the end of May 1948, as Stebbins approached retirement, President Fred summoned Whitford to his office for two long interviews. Albert stated his goals for the university's astronomy program as continuing as the leader in photoelectric photometry in America, frequent observing trips to use the big telescopes in California, and a new reflecting telescope about 36 inches in diameter at a dark-sky site off the campus but near Madison. It would replace the antiquated 15.6-inch refractor on Observatory Hill, which he had regarded as a relic ever since he first saw it as a graduate student in 1929. At this conference Whitford also agreed (and in fact strongly favored) incorporating Washburn Observatory into the College of Letters and Science. Under Stebbins and the earlier directors it had the status of a separate research organization directly under the president, though the director had regular teaching duties in the college. Whitford's appointment as director dragged on all summer (most of which he spent at Mount Wilson). Even after his return the observatory remained rudderless, probably because of its transfer to Letters and Science. Finally in October 1948 the Board of Regents promoted him to full professor (though without any salary increase) and named him observatory director. Stebbins had resisted integrating it into the college, preferring to keep it as his own independent research base, but Whitford realized that teaching more undergraduates and training graduate students in a regular program were important parts of a university and would lead to increased support.

Whitford, given the go-ahead by Fred, assigned Ted Houck, then a graduate student, soon to become Wisconsin's third Ph.D. in astronomy, to search for a dark site in the still largely rural area west of Madison. Using a portable photoelectric photometer Houck measured the sky brightness at numerous locations, and eventually Whitford chose Pine Bluff, on a hill some 15 miles from the campus. In 1950, when an opportunity arose to have a 36-inch primary mirror made for a low price at the Yerkes Observatory optical shop, Whitford got Fred's permission not only to order it but also to put it on the university's priority list for $275,000 from the Wisconsin Alumni Research Foundation, Wisconsin's own "little NSF," to start building the observatory. By 1952 the proposal had worked its way to the action part of the list, and Whitford with help from Code and Houck prepared a conceptual plan that was approved, and the foundation was granted the money. Whitford planned and oversaw the construction of the 36-inch telescope optimized for photoelectric photometry and spectrophotometry, spending huge amounts of his time and effort on the project. The National Science Foundation provided additional funds, and it was completed in June 1958 and was dedicated at a meeting of the American Astronomical Society in Madison, just as Whitford himself was leaving for a new post as director of Lick Observatory, of the University of California. Pine Bluff Observatory was his legacy to Wisconsin, and Code, Houck, and other new arrivals, including myself, used it effectively with graduate students to build up the University of Wisconsin as a real power in astronomy.

Whitford and his wife loved Wisconsin and hated to leave Madison, but he believed that taking either the Lick directorship or the directorship of the new National Radio Astronomy Observatory, which he had been offered at about the same time, was his duty. He knew optical astronomy very well indeed by then and felt he could make a bigger contribution at Lick. It was a prestigious old research institution, still using telescopes built in the previous century. Its astronomers were waiting impatiently for a new 120-inch reflector to be finished, which when completed would be the second-largest telescope in the world. Probably the reasons Whitford was chosen to succeed C. Donald Shane, who was stepping down after 12 years as director, were his drive and mechanical knowledge, demonstrated in getting the Wisconsin 36-inch reflector and its auxiliary instruments completed on schedule. Two members of the Lick faculty, Olin J. Eggen (UW's second Ph.D. in astronomy) and Gerald E. Kron (who had earned a master's degree in astronomy there), knew him especially well, and his research qualifications were extremely high (he had been elected to the National Academy of Sciences in 1954). In addition, Stebbins was spending his postretirement years in California near Lick and visited it frequently. He had recommended Whitford strongly for the directorship. At Lick Whitford took hold immediately, plunged into the details of the telescope construction and optics, and got rid of one highly placed engineer. The new director had the telescope completed in 1959 and taking data on a regular basis in 1960. Before long, astronomers not only from Lick but from other University of California campuses, too, were using it for frontier research on stars, clusters, galaxies, and quasars.

Whitford was completely dedicated to astronomy, knew it very well, and worked hard at it. He was highly intelligent and had unmatched integrity. Astronomers trusted his judgment. In the 1950s, as one of the few Midwestern astronomers with a proven big-telescope research record, he played a leading role in the conferences and discussions that resulted in the founding of Kitt Peak National Observatory, later to become the nucleus of the National Optical Astronomical Observatories. Whitford served as a highly active, strongly research-oriented member of its Board of Directors as long as he remained at UW. For all these reasons he was chosen to head the first organized survey of the needs of U.S. astronomy, sponsored by the National Academy of Sciences. The panel he chaired included many of the leaders of all fields of astronomy. Under his leadership they held hearings and sought the views of research workers throughout the country. Whitford pushed for open democratic discussions and sought the views of all. The resulting "Whitford Report," published in 1964, became the overall guide to National Science Foundation priorities for a decade. It was highly successful, and as a result similar panels were set up in other fields of science; in astronomy a new survey was made each decade, successively headed by Jesse L. Greenstein, George B. Field, and John N. Bahcall.

Within a few years of his arrival at Lick Observatory, Whitford had a new, unexpected problem to deal with. The University of California administration, headed by President Clark Kerr, who took office in Berkeley in 1958, simultaneously with Whitford on Mount Hamilton, decided it did not want to continue Lick as a freestanding research institution with no students, reporting directly to the president. It and the Scripps Institution of Oceanography in La Jolla, the only other large unit of this type, were to become parts of the University of California campuses. Scripps became the nucleus of the new, science-oriented University of California, San Diego, and Lick was to be part of a completely new campus in the San Jose area. The site selected, Santa Cruz, was about 70 miles from Mount Hamilton, and all the faculty members who lived in the little astronomy village there would teach and have their offices on the campus. Several of the astronomers had lived on the mountain for years and liked it and the freedom they had to spend all their time on research. There was strong resistance to the idea of the move from some of them. Whitford, however, knew it was his duty to lead them to the campus and he did so in his businesslike way, assigning duties in planning the move to each of the senior staff members. A few of them left Lick Observatory and the University of California at that time, a few others grumbled but gave in, and the rest looked forward to being on a campus with professors in other fields and undergraduate students to teach. Whitford's leadership strengthened this group and did much to make the move a real success.

In the fall of 1966 the Lick faculty and staff moved down to the new campus in the redwoods. Chancellor Dean E. McHenry, supported by President Kerr, was innovative, putting into practice many "progressive" ideas for the 1960s, such as not having "old-fashioned" departments nor many required courses, and doing away with letter grades. Whitford, who had grown up at Milton College and spent 30 years at the University of Wisconsin, was used to being on a campus and knew what would be required. More than a year before the move Whitford had asked Peter Bodenheimer, then close to finishing the Ph.D. in theoretical astrophysics at Berkeley, whether he would be interested in a faculty job at Lick. Bodenheimer was, and after two years as a postdoctoral fellow at Princeton University he was back at Santa Cruz as the first theoretician on the Lick faculty. Graduate courses in astronomy and astrophysics started in 1967, with a first contingent of nine new students. There was no department, these were passé at Santa Cruz, but there was a "Board of Studies in Astronomy and Astrophysics" that looked like a department, acted like a department, and worked like a department, and included all the Lick faculty members. It was a separate unit, organizationally independent of Lick, but with almost complete overlap of membership with it. Whitford supported the new board strongly, did his share of the teaching in it, and provided sound advice and guidance to the "conveners" (as chairs were then called) beginning with George W. Preston. Whitford strongly approved the proposal for a large National Science Foundation "departmental improvement grant," which provided startup funds and the impetus to add several more theoreticians to the board faculty over the next several years. He and Kenneth Thimann, another National Academy of Sciences member and a plant biologist from Harvard, gave Santa Cruz a start and a big push toward frontier research in physical and biological sciences, which has continued and strengthened over the years.

At Wisconsin Whitford had taught hundreds of undergraduate students in elementary astronomy survey courses, but only a very few graduate students, in one-on-one "reading" or "research" courses: Eggen (who was largely Stebbins's student), Houck, John Bahng, Kenneth Hallam, and John Neff. At Santa Cruz he enjoyed teaching a regular graduate course in galaxies, in which he gave the students a thorough grounding in research techniques.

The 1960s were turbulent years in U.S. universities, nowhere more than at the University of California. The new president, Clark Kerr, who had taken office in 1958, the year Whitford arrived, was making it over into a "multiversity," with several coequal campuses. There were hard clashes between students, faculty, and administrators. The Lick astronomers were isolated from almost all of these on Mount Hamilton; at Santa Cruz Whitford did his best to shield them from it. He was their leader who negotiated for them with the campus and university-wide administrators, and was gradually ground down in these struggles.

Whitford's overall passion was research in astronomy, and he pushed it hard. His own specialty was photoelectric photometry, but he strongly supported the other Lick astronomers who were developing other auxiliary instruments for the new 120-inch telescope. He took a keen interest in what they were doing, questioned them closely, and made frequent suggestions. Some of them resented his approach, but it worked, and Lick Observatory was producing a lot of good research results with the coudé spectrograph, image-tube dissector scanner, and fast nebular spectrograph they developed.

In 1968, after 10 years as Lick director, Whitford stepped down but remained an active faculty member as a professor of astronomy and astrophysics until he reached the mandatory retirement age in 1973. He had spent all his time and effort on the directorship for a decade, but now he plunged back into research, taking up where he had left off with six-color photometry and galaxy research. After his retirement he could do no more observing at the big Lick telescope, but he made several trips to Chile to use the Cerro Tololo Inter-American Observatory, the southern branch of the U.S. National Astronomical Observatories he had done so much to get started. Later he worked on interpreting published data or new observational results obtained by younger collaborators, especially on stellar populations in the central bulge of our Galaxy, harking back to his early papers on M 31, the Andromeda galaxy, Now the data came from much more advanced detectors than the first near-infrared photocells and PbS cells he had used earlier, and were recorded and reduced digitally. He had come a long way in astronomical photometry.

Whitford's main interest remained astronomy all his life. He came to the office daily; attended all the colloquia, research seminars, and public lectures; read the journals; and was an unofficial adviser and font of wisdom to several graduate students, including David Burstein, R. Michael Rich, and Donald Terndrup. Albert loved to see his children and grandchildren; he could remember as a boy studying Latin at Milton Academy and going to his grandfather's house and reading to him from Virgil. The old man, though he could no longer see the words on the page, would correct young Albert from memory if he skipped or mispronounced a word. Albert himself loved the outdoors, especially hiking in the Sierra, which he had first encountered in his postdoctoral years in California. Later in his life he would lead family camping trips into them well into his eighties.

Albert's wife, Eleanor, died in 1986, ending a long, highly compatible marriage. He lived on in Santa Cruz, still coming to the campus daily and working on stellar populations until 1996. That year a symposium on stellar populations was held in his honor at Santa Cruz, under the redwoods outside Kerr Hall, where he and all the other astronomers' offices were then located. Soon after that he returned to Madison, where his son, Bill, was a professor in the law school and one of his grandsons a graduate student at the University of Wisconsin. In Madison Albert lived in a senior citizens' complex only a few blocks from the Capitol, between it and the campus. In the astronomy department he was especially welcome as a visiting emeritus professor and had a desk in an office, to which he came frequently to discuss astronomy with students and faculty alike, to keep up to date on the latest research in the current journals, and to revisit his old haunts. Until the last few months before his death he continued to go there frequently, and he died after only a short illness.

Whitford was a man of tremendous integrity. He found it difficult to speak anything but the whole truth and nothing but the truth, not necessarily the best tactic in all situations for the director of a large research institution. He was interested in astronomy and research to the last, always wanting to know more about the evidence for any new discovery. Never domineering or aggressive, he was always willing to discuss astronomy and pass on his accumulated wisdom, based on hard study and experience. And he was a keen judge of human beings, scientists especially. He had learned to understand and predict what they could and could not do. Over the years he made many important contributions to our knowledge of the Universe, and his peers acknowledged them by making him the Henry Norris Russell lecturer of the American Astronomical Society in 1986 and awarding him the Catherine Wolfe Bruce medal of the Astronomical Society of the Pacific in 1996. The Whitford interstellar extinction "law" and the Whitford report are his best-known legacies to us.

THIS BIOGRAPHICAL memoir is based mainly on Whitford's published scientific papers, and on many letters to, from, or about him in the University of Wisconsin Archives, Madison, and in the Mary Lea Shane Archives of the Lick Observatory, University Library, University of California, Santa Cruz. My own conversations and discussions with him over the years from approximately 1950 to 2001 were also insightful. Some of the latter, after his retirement in 1973, were

interviews on which I kept notes. Albert's own published autobiographical article, "A Half Century of Astronomy" (*Annual Review of Astronomy and Astrophysics* 24[1986]:1-13) is informative but typically modest. His son, William C. Whitford, and daughters, Mary W. Graves and Martha W. Barss, provided additional biographical details at my request, and several faculty colleagues who had known him at Madison, Santa Cruz, or Mount Wilson made helpful suggestions or comments on earlier drafts of this memoir.

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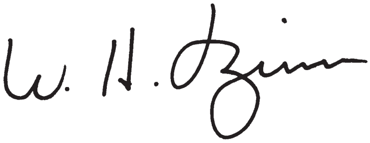

WALTER HENRY ZINN

December 10, 1906–February 14, 2000

BY ALVIN M. WEINBERG

WALTER ("WALLY") HENRY ZINN was Enrico Fermi's close associate during the Manhattan Project. After World War II he became the leading U.S. figure in the earliest development of nuclear energy. So pervasive was his stamp on nuclear development that a proper obituary to Walter Zinn must be nothing short of an account of the origins of nuclear energy and how Zinn profoundly affected its development.

Fission was discovered in 1938. By then Zinn had already received his Ph.D. in physics from Columbia (in 1934) and had been on the faculty of City College of New York. He also had a laboratory at Columbia, where he collaborated with Leo Szilard and Enrico Fermi in elucidating the nature of fission. In those exciting days nuclear physicists were asking how many neutrons were emitted by a uranium nucleus undergoing fission induced by a neutron. If the answer were greater than one, a nuclear chain reaction was possible; if less than one, a divergent chain reaction was impossible. Zinn and Szilard found that about two neutrons were emitted by a fissioning uranium nucleus; in this they confirmed the results of Fermi, Anderson, and Hahnstein. Thus was born experimental verification of the Manhattan Project's purpose: to make an atomic bomb.

After the number of neutrons released from fission was shown to be around two, Fermi lost no time in demonstrating the chain reaction. Zinn joined Fermi's experimental team, and he soon became Fermi's "executive officer." In this capacity Zinn organized the heavy experimental work that was needed to carry out Fermi's plan to build a divergent chain reaction.

The aim was to demonstrate that a lattice consisting of uranium and graphite would chain-react if it were large enough. Since the critical size of such a lattice was larger than the amount of uranium and graphite then available, Fermi devised the so-called exponential experiments (i.e., subcritical arrays in which the neutrons were distributed exponentially). Zinn participated in the first of these experiments, which was carried out at Columbia. The multiplication constant was found to be a disappointing $k = 0.87$, but Fermi was confident that purer uranium and graphite and a better lattice dimension would achieve the magical $k = 1.0$.

By late 1941 the plutonium branch of the uranium project was consolidated under Arthur Compton at the University of Chicago Metallurgical Laboratory, and Zinn accompanied Fermi to the Met Lab. At Chicago some 30-odd exponential experiments were conducted to measure the multiplication constant in different lattices of uranium and graphite. Each experiment involved a pile of graphite and uranium about 11 feet high and 8 feet on the side. Changing from one configuration to another required a team of strong University of Chicago athletes bossed by Zinn, who was in daily contact with Fermi.

The first experiment that showed $k > 1$ was performed in May 1942. Arthur Compton's announcement of $k > 1$ was for me the most exciting event of the uranium project. I remember discussing the actual demonstration of the divergent chain reaction with Fermi and Wigner when Wigner bravely said he was so sure the pile would chain-react that he doubted he would attend the historic event (he actually did attend).

We had a bowling club that met weekly, and on the night of December 1, 1942, I walked with Zinn to our club. By this time Zinn and Herbert Anderson's team had erected a pile that would be critical the next day.

I was too junior to attend the first criticality experiment at about 3:20 p.m. on December 2, 1942. Fermi was in overall charge, but Zinn saw to it that Fermi's directions were carried out. At the instant of criticality Zinn was responsible for the so-called "zip" rod, a simple bar of cadmium held by a spring and tied outside the pile by a 100-pound counterweight. Zinn held an axe with which he was ready to cut the rope that held the zip rod if the chain reaction were to get out of hand. Fortunately the "landing in the new world" (words used by Arthur Compton in a phone call to James Conant) was uneventful. Zinn did not have to cut the rope that kept the zip rod from entering the pile.

By 1946 the Atomic Energy Act had been passed by Congress and the Manhattan Project was transformed into the U.S. Atomic Energy Commission (AEC). Most of the senior members of the project, such as Bethe, Compton, Fermi, Oppenheimer, Teller, and Wigner, returned to their universities; their replacements included Norris Bradbury at Los Alamos and Zinn at Argonne.

Zinn had emerged during the wartime Hanford development as a natural leader. He was intelligent, very close to Fermi, and he was tough. According to his son Professor Robert Zinn of Yale, Zinn spoke at home only of pleasant happenings; yet he would explode when he was too involved with people he regarded as foolish.

In those earliest days the AEC had hardly decided on how to develop nuclear energy. In 1948 the AEC designated Argonne as the National Laboratory responsible for all work on reactors. As director of Argonne, Zinn found himself the nominal scientific boss of several different reactor projects, particularly the High Flux (a water-moderated, highly enriched reactor being designed in Oak Ridge under Wigner's supervision); the NaK-cooled fast breeder prototype (EBR-1) being developed directly under Zinn's supervision at Argonne; and the newly established Submarine Thermal Reactor (STR) for naval propulsion that was essentially a pressurized version of the High Flux water-moderated reactor. The EBR-1 project, having been Zinn's baby from the first, smoothly merged with the rest of Argonne, and EBR-1 became the first reactor to generate electricity.

The High Flux, renamed the Materials Testing Reactor (MTR), had already received extensive preliminary design at Oak Ridge. The project was divided between the Oak Ridge group, which was responsible for the interior of the MTR, and the Argonne group, which was responsible for the external facilities required to manage the 30,000 kW generated in the MTR. Zinn, as director of Argonne, became chairman of a five-member steering committee that oversaw the entire project. MTR was the first successful demonstration of a very-high-power-density, water-moderated, and water-cooled reactor.

The submarine reactor, STR, was a different story. Both Zinn and Captain H. G. Rickover were tough; although the AEC had assigned a naval reactor role to Argonne, Zinn's relations with Rickover were never friendly. The general layout of the STR had been the brainchild of Harold Etherington of Argonne, yet Rickover insisted that Argonne's STR group take its orders from Rickover and not from Zinn. The upshot of the matter was that Zinn actually threw Rickover out of Argonne, because as Wally told me, he could not tolerate two bosses for the same reactor! After the blowup with Rickover, Argonne's role in developing STR became secondary to that of the Westinghouse Bettis Laboratory.

Relieved of prime responsibility for STR, Zinn at the suggestion of Sam Untermeyer experimented with boiling-water reactors. Thus was born the BWR (Boiling-Water Reactor) that now accounts for about 20 percent of the world's fleet of approximately 440 nuclear power plants.

Although EBR-1, MTR, and the BWR were the main efforts at Argonne, the laboratory designed or built several other reactors: the first medium power (300 kW) heavy-water reactor; the huge D_2O tritium producers built and operated at Savannah River, South Carolina, by the Du Pont Company; and power reactors cooled by various coolants. Zinn was an important player in most of these developments.

Zinn's role as leader of the postwar development of reactors was symbolized at the First Geneva Conference on Peaceful Uses of Atomic Energy in Geneva, Switzerland, in August 1955. This U.N.-sponsored conference involved over a thousand nuclear energy experts from both sides of the Iron Curtain. The opening session was like a thirteenth-century jousting tournament with the Soviet Union and United States each putting forward its champion. D. I. Blokhinsev described the Obninsk 5000-kW graphite-moderated, water-cooled pilot plant. He was followed by Zinn, who gave the first public account of successful experiments with the boiling-water reactor. The Russian pilot plant was the forerunner of their plutonium-producing reactors. Zinn's boiling-water experiments led to the 90 large commercial boiling-water reactors (BWR) now operating.

Zinn left Argonne in 1956 after serving for eight years as its first director. The general campus-like layout of the laboratory reflects Zinn's sensitive practicality. Zinn could be stubborn both in his relations with the contracting entity, the University of Chicago, and the AEC, which funded Argonne.

As a former National Laboratory director, I can say that Zinn was a model of what a director of the then-emerging national laboratories should be: sensitive to the aspirations of both contractor and fund provider, but confident enough to prevail when this was necessary. This is illustrated by the following anecdote: Zinn, Harrell (University of Chicago contracting officer), and Tammaro (area manager for the AEC) were discussing whether Argonne's security fence should include the entire site or simply separate buildings. Zinn and Tammaro couldn't agree. Tammaro finally said that Harrell (a University of Chicago vice-president who had signed the contract with the AEC) should decide. At this point Zinn took over. "He can't decide a single thing," which illustrates Zinn's commitment to experts, not to bureaucrats.

Zinn, as director of Argonne, was in no position to design and build large power reactors. He therefore left Argonne to establish the General Nuclear Engineering Company (GNEC) with headquarters in Dunedin, Florida. The company flourished and was much involved in large-scale pressurized-water reactors. Eventually GNEC was acquired by Combustion Engineering Company, and Zinn became head of its fast-growing Nuclear Division. He retired from Combustion in the early 1970s but remained on the company's board of directors until the early 1980s.

By this time Zinn had become sort of a gray eminence of nuclear development. He also received the highest honors: the Ford Family's Atoms for Peace Award, the Enrico Fermi Award, and membership in both the National Academy of Sciences (in 1956) and the National Academy of Engineering (in 1975). He also served on the President's Science Advisory Committee during the 1960s.

Many of the most important decisions of the American nuclear effort during the post-1940s were attributable to Zinn. Among those were:

• The establishment of the Reactor Test Station in Arco, Idaho, where the prototypes of the first naval reactor as well as MTR, EBR-1, and EBR-2 were built and operated.

• The founding of the American Nuclear Society (ANS), which was strongly influenced by Zinn, its first president. Today ANS has about 10,000 members and is the main technical society in the field of nuclear science and engineering.

By the end of the war we realized that counting the different moderators and coolants the number of power reactor concepts could be counted in the dozens. Not all could be pursued simultaneously. Choices had to be made, and Zinn's view greatly influenced the earliest decision as to which paths to follow. Two basically different paths were suggested by Zinn: variants of the naval reactor STR, which led to the commercial PWRs and BWRs, and the fast breeder, which led to the EBR-1 and its successor, EBR-2, and breeders in Russia, Japan, the United Kingdom, France, and India. It was Zinn's persistent advocacy of the NaK-cooled EBR-1 that thrust the U.S. reactor program on this dual path: light-water burners and liquid-metal-cooled fast breeders. This path has been followed by most nuclear developers, Canada being a notable exception.

Zinn's espousal of the first breeder was based on the earliest estimates of how much uranium could be extracted. In those early days before much exploration for uranium had taken place we thought uranium was scarce. The breeder would have to be developed if nuclear energy was to be a long-term source of energy. This was a deeply held conviction of Fermi, Wigner, and Szilard; and it was the guiding principle for Zinn (who was influenced by Fermi). Thus a primary goal of the earliest reactor development plan was the fast breeder. This remained an important element of Zinn's approach to reactor development. But now some

60 years after December 2, 1942, we realize that the earth's cache of uranium is many times larger than our earliest estimates and therefore the *quick* development of the fast breeder may be unnecessary. That is not to say that Zinn's EBR-2, a 20,000-kW sodium-cooled reactor was not a major technical success. Although it was shut down after 40 years of corrosion-free operation and thus vindicated Zinn's judgment that the Liquid Metal Fast Breeder Reactor (LMFBR) was a practical device, the shutdown of EBR-2 was a major error in the U.S. reactor plan.

WALTER ZINN AS VIEWED BY HIS SON, PROFESSOR ROBERT ZINN

Walter Henry Zinn was born December 10, 1906, in Kitchener, Ontario, Canada, and died on February 14, 2000, in Clearwater, Florida. The son of John Zinn and Maria Anna Stoskopf, he had an older brother, Albert, who was 10 years his senior. Of his working class family Walter was the only one of his immediate family to attend college. His father worked for much of his life in a tire factory, and Albert was also a factory worker. As a boy, Walter also worked in one or more factories.

Walter skipped a few grades during elementary school and entered Queen's University. He graduated from Queen's in 1927 with a B.A. degree in mathematics and remained there until he earned an M.A. degree in 1930. In 1957 Queen's awarded him an honorary D.Sc. degree. In 1930 Walter became a graduate student in physics at Columbia University. He married Jennie A. Smith in 1933, whom he had met when they were both students at Queen's. Jennie, who was always called Jean, died in 1964. Walter became a naturalized U.S. citizen in 1938.

Walter held teaching positions at Queen's during 1927-1928 and at Columbia during 1931-1932 and was on the faculty of City College of New York from 1932 to 1941. He also had a research laboratory at Columbia, where in 1939 he participated in the famous early work on the fission of uranium and the possibility of a chain reaction.

In 1966 Walter married Mary Teresa Pratt, who survives him, as do his sons, John Eric and Robert James Zinn, and his stepson, Warren Johnson. Another stepson, Robert Johnson, died in 1991. Walter had nine grandchildren.

Walter frequently recalled events that surrounded the beginning of the Manhattan Project and the first chain reaction. His descriptions corresponded with those that have been published by historians, but captured much more of the excitement of the moment and also the great concern that scientists had for Germany's development of the atomic bomb and the possibility that the Germans were ahead of the United States in the race to produce the first bomb. Walter had a tremendous admiration for Enrico Fermi, and he was awed by Fermi's genius both as an experimental physicist and as a theorist. Walter's comments on Fermi were often to the effect that at one moment Fermi could invent a novel way to make a difficult measurement and in the next could argue a subtle point in theory with the very best theorists (e.g., Eugene Wigner). Walter also enjoyed telling how he served twice as a real estate agent for Fermi by finding places for the Fermi family to live near New York City and later Chicago, how they traveled to work together, and how it was to have Fermi as a friend and colleague.

Walter also had high regard for Leo Szilard, his collaborator on one of the first experiments on fission. While Walter acknowledged that Szilard was of little real help with design or operation of the experimental apparatus, he said that Szilard was an "idea man" with few peers and that he motivated others to conduct the "right" experiments.

The McCarthy era occurred during Walter's tenure as director of Argonne National Laboratory, and he had a few stories to tell about the hysteria that enveloped that period, including imagined security breaches at Argonne. One particularly frustrating episode for Walter involved a small bottle of slightly enriched uranium that some media people and politicians were convinced had been taken by Russian spies. Walter was in some hot water over this issue until the missing bottle was discovered in the garbage in Argonne's landfill. Experiences like that caused Walter to hold most politicians in low esteem, because they seemed less interested in truth than in advancing their own careers.

Walter was proud of his work on the development of nuclear energy for the production of electricity. He worked on many of the designs that later became the standard ones for the nuclear power industry. He worked hard on reactor safety, and he would recall experiments at the Idaho test site, where a reactor was purposely destroyed to better understand various safety issues. He believed that properly designed and operated reactors were very safe. He firmly believed in a bright future for nuclear energy. When in 1948 the AEC assigned responsibility for reactor development to the Argonne National Laboratory, Walter emerged as a natural leader of the U.S. effort to develop nuclear power.

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