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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

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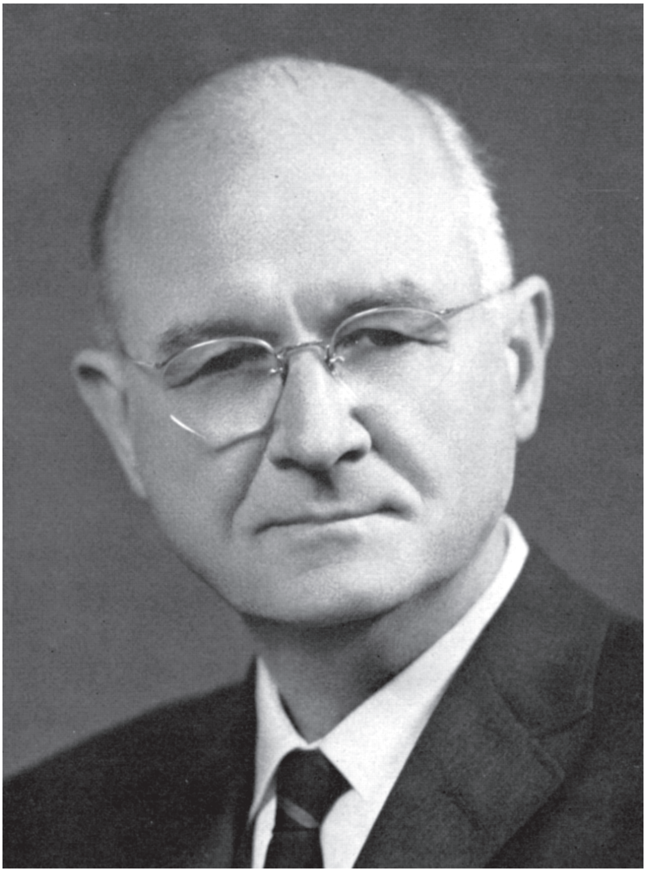

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H A Barker

H. A. BARKER

November 29, 1907–December 24, 2000

BY ROBERT L. SWITZER, EARL R. STADTMAN, AND THRESSA C. STADTMAN

"EXPLORATIONS OF MICROBIAL METABOLISM" was the title chosen by H. A. Barker for his summary of his lifelong scientific accomplishments in 1978. His title was descriptive and accurate, but characteristically for this modest man, it significantly understated the extraordinary breadth and depth of the impact of his discoveries on central concepts in the intermediary metabolism of all organisms, the mechanism of enzymatic catalysis and of coenzyme function, as well as on microbial physiology, taxonomy, and ecology. Barker's contributions are today so much a part of the fundamental fabric of modern biochemistry and microbiology that it is easy for younger scientists to overlook the modest and gentle man who made them. Also important is the exceptional influence Barker had on the many scientists who worked with him, whether as students, postdoctoral researchers, or in one of many collaborations, research visits, and sabbaticals. He taught the conduct of insightful, thoughtful, carefully executed scientific experimentation by quiet example, and he was admired and respected by all who worked with him.

Horace Albert Barker, known as "Nook" by his intimate friends but as H. A. Barker or simply "Dr. Barker" by everyone else, grew up in California and by his own description came slowly to a career in science. Although he was originally more interested in music and literature, his love of the outdoors and the natural world and his undergraduate studies of biology at Stanford University, completed in 1929, led him to choose graduate study first in biology; he then switched to chemistry as his interest in biochemistry developed at Stanford, which awarded him the Ph.D. degree in 1933. During his graduate studies Barker began an association with the Hopkins Marine Station at Pacific Grove that was to profoundly affect the direction of his future scientific research. Two summer research periods were followed by a postdoctoral fellowship from 1933 to 1935 at the Hopkins Marine Station, where Barker began his studies of microbiology under the direction of the distinguished Dutch microbiologist C. B. van Niel. Van Niel's mastery of this discipline, his enthusiasm, and his gifts as a teacher and researcher soon persuaded Barker to choose microbiology for his own career. From van Niel he absorbed the concepts so strongly associated with the "Delft school" of microbiology, namely, that microorganisms can be best understood and classified by the chemical activities they carry out, that understanding these biochemical processes provides the key to the ecological niches microorganisms occupy, and the unity of biochemistry (i.e., that the fundamental nature of the metabolic reactions in bacteria was the same as was being unraveled by other biochemists in the study of yeast and animal tissues).

Barker's commitment to a lifelong study of the chemical activities of microorganisms was consolidated during a year's study (1935-36) with van Niel's mentor A. J. Kluyver in the Delft Microbiology Laboratory, which was supported by a fellowship from the Rockefeller Foundation. In the course of this single year he initiated studies on three topics that he was to pursue throughout his career and that would yield many of his most significant discoveries. These were production of fatty acids by microbial fermentation, the biochemistry of methanogenesis, and the anaerobic degradation of glutamate. Barker adopted from Kluyver and van Niel the practice of characterizing fermentative pathways by quantitative analysis of the amounts of substrates consumed and products formed. From a determination of equations that account for the carbon, nitrogen, and redox balance of a fermentative process much about the nature of the fermentation itself could be deduced. In his later research Barker was to add the use of radiocarbon tracers and identification of individual enzymatic steps in cell-free extracts to this fundamental method. The combination led to the many remarkable discoveries we discuss below.

In 1936 Barker returned to California to take a position as a soil microbiologist in the Agricultural Experiment Station at the University of California, Berkeley. He was to remain on the Berkeley faculty for his entire career, holding appointments first in plant nutrition, then plant and microbial biochemistry, and finally in the reorganized Department of Biochemistry after 1959. His students came from many disciplines, however, and many earned their degrees in an interdepartmental program called comparative biochemistry. Through the years of the Great Depression and World War II to the great flowering of federal support of basic research in the postwar years, Barker pursued his fascination with microbial fermentations. “In lean years we did microbiology,” he told one of us (R.L.S.), “and in good years we did biochemistry.” Because of the proximity of the Berkeley Radiation Laboratory, radioactive carbon isotopes became available to biological researchers. As early as 1939 Barker, Zev Hassid, Sam Ruben, and Martin Kamen began experiments on the fate of $^{11}CO_2$ during methanogenesis.

When [^{14}C] with its much more convenient long half-life became available in 1944, the use of radiotracer technology for the investigation of metabolic pathways began almost immediately to lead to important and often unexpected findings.

It was evident to Barker that to elucidate the individual biochemical steps involved in the fermentations he was studying, it was necessary to isolate and characterize the enzymes that catalyze each step from cell-free systems. This became all the more crucial as his studies led again and again to the involvement of cofactors and the formation of cofactor-bound intermediates—cofactors of universal importance in intermediary metabolism. His interest and skill in working with cell-free systems were stimulated during time spent with Fritz Lipmann in 1941-42 while supported by a Guggenheim Fellowship, by collaborative research with Michael Doudoroff and Zev Hassid on the catalytic mechanism of sucrose phosphorylase, and later during research in 1951 and 1952 with Arthur Kornberg at the National Institutes of Health.

Some of H. A. Barker's most significant scientific contributions will be discussed in individual sections below. Although this modest man did not actively seek public recognition, recognition came to him as the impact of his discoveries became widely appreciated. He was elected a member of the National Academy of Sciences and the American Academy of Arts and Sciences. He received the 1965 Borden Award in Nutrition and the Hopkins Medal from the Biochemical Society (U.K.). He was named California Scientist of the Year in 1966, and President Lyndon Johnson presented the National Medal of Science to him in 1968. The Biochemistry Building on the Berkeley campus was renamed H. A. Barker Hall in 1988, a rare recognition for a living scholar. Professor Barker retired to emeritus status in

1975, but he remained active in the Biochemistry Department's research and intellectual life for many years thereafter. He died peacefully at his home in Berkeley at the age of 93.

During his years at Stanford, Barker married Margaret McDowell, and they were together for 62 years until her death in 1995. They had three children: Barbara Friede, Elizabeth ("Betsy") Mark, and Robert Barker. We have noted that Barker's love of science was interwoven with his love of nature and the outdoors. He and his family carefully reserved a portion of their summers for holidays at their cabin in the Sierras, and Barker's fondness for fishing in remote locations was well known to his many friends.

BIOCHEMISTRY OF METHANE FORMATION

Methane fermentations are now known to serve as important anaerobic processes in which the decomposition of a variety of alcohols, amines, and fatty acids is coupled to the reduction of carbon dioxide to methane. C. B. van Niel, who was trained in the famous Microbiology Department of the Technical University in Delft, Holland, and later became professor of microbiology at Stanford University's Hopkins Marine Station in Pacific Grove, California, became interested in the anaerobic fermentation of specific compounds in nature and the origin of the methane generated in these processes. Barker was exposed to van Niel's theories about the chemistry of the overall methane fermentation when he spent summers at the Hopkins Marine Station while a graduate student in chemistry at Stanford and later as a postdoctoral fellow with van Niel.

During this period van Niel proposed his carbon dioxide reduction theory to explain the origin of methane as a common product in the diverse reactions known at the time. Stimulated by these ideas Barker became interested in methane bacteria. During a later postdoctoral appointment in the laboratory of A. J. Kluyver in Delft he obtained enrichment cultures from canal mud that converted ethanol-bicarbonate mixtures to acetate and methane. The microorganism in these cultures responsible for this reaction was named *Methanobacillus omelianski*. Although it was shown many years later to actually be a consortium of two mutually dependent species, the culture proved to be very useful biological material for detailed studies of carbon dioxide reduction to methane. In particular, this culture was used for Barker's groundbreaking experiments that demonstrated conversion of $^{11}CO_2$ to radioactive methane, even though the half-life of this new radioactive element was only 20 minutes. When the long-lived [^{14}C] isotope became available in 1944, it enabled Barker and his associates to conduct many elegant studies on the chemical details of methane biosynthesis. An important exception to the general theory that methane originates exclusively from carbon dioxide was reported in the literature while one of us (T.C.S.) was a graduate student in Barker's laboratory in Berkeley. The authors reported that when [^{14}C]-labeled carbon dioxide was added to cultures that were fermenting acetate and producing methane, little or no radioactivity was found in the methane evolved. Under Barker's direction these results were confirmed, and furthermore it was established that during acetate fermentation methane was derived exclusively from the methyl group of acetate and the carboxyl group was converted to carbon dioxide. Likewise, during fermentation of [^{14}C]-labeled methanol three equivalents of radioactive methanol were reduced to radioactive methane at the expense of the oxidation of one equivalent of methanol to radioactive carbon dioxide and water. Later Pine and Barker showed that when CD_3OH was used as the substrate, all three deuterium atoms were retained in the methane product. With deuterium-labeled solvent and unlabeled methanol one deuterium atom from solvent was incorporated into the methane product.

The demonstration from Barker's laboratory of methane generation from the methyl groups of acetate and methanol led to a considerably expanded and modified view of the biochemistry of methane fermentation in general. In 1956 Barker proposed a unifying conceptual scheme for methanogenesis from carbon dioxide, acetate, and methanol. A central feature of this scheme was the postulate that carbon dioxide becomes attached to an unspecified carrier prior to stepwise reduction to methane. Furthermore, the methyl groups of acetate and methanol could be transferred to this or additional carriers and be reduced to methane or oxidized to carbon dioxide. This concept was verified by the discoveries by B. Blaylock and T. Stadtman and of D. Grahame of the intermediary role of methyl corrinoids as methyl group carriers and the discovery in the laboratories of R. S. Wolfe, R. K. Thauer, and G. Vogels of several novel cofactors that function as C-1 carriers in methanogenesis. These findings have permitted the detailed biochemistry of methanogenesis to be described.

STUDIES OF FATTY ACID METABOLISM WITH *CLOSTRIDIUM KLUYVERI*

During the year he spent in Kluyver's laboratory Barker embarked upon a study designed to test van Niel's theory that the reduction of CO_2 to methane might be involved in the fermentation of organic compounds by methane bacteria. To this end Barker prepared anaerobic enrichment cultures containing $CaCO_3$ and ethanol and a generous inoculum of mud from the Delft canal outside Kluyver's laboratory. Microscopic examination of a culture producing a mixture of acetic, butyric, and caproic acids and methane disclosed the presence of two different types of bacteria, which Barker separated and purified. One of these catalyzed the conversion of ethanol and CO_2 to methane and acetic acid and was given the name *Methanobacterium omelianski*, as described above. A pure culture of the other microorganism catalyzed the conversion of ethanol and acetic acid to short-chain fatty acids and was given the name *Clostridium kluyveri.* In subsequent studies one of Barker's students B. T. Bornstein established that *C. kluyveri* catalyzes the conversion of one equivalent each of ethanol and acetate to butyrate, and the further reaction of the butyrate with a second equivalent of ethanol to form caproate.

In 1944 a more detailed investigation of the mechanism involved in fatty acid synthesis by *C. kluyveri* became possible with the availability of the long-lived isotope of carbon [^{14}C]. In a collaborative study with Martin D. Kamen, co-discoverer of [^{14}C], Barker and Bornstein demonstrated that the fermentation of [carboxyl-^{14}C]acetate and unlabeled alcohol led to the production of [^{14}C]-butyrate that was almost equally labeled in the carboxyl and beta carbon atoms and to [^{14}C]-caproate that was labeled in the carboxyl, beta, and delta carbon atoms. Furthermore, during fermentation the specific radioactivity of the added acetate was decreased by an amount equivalent to the amount of ethanol used. This established that the formation of butyrate and caproate from ethanol and acetate is a coupled oxidation-reduction process in which the ethanol is oxidized to acetate (or a compound in equilibrium with acetate) and that the consumption of two or three equivalents of acetate leads to formation of the 4- and 6-carbon derivative (*viz.* β-keto acids) that are reduced to butyrate and caproate. In view of Lipmann's calculations showing that the condensation of two moles of acetate to form acetoacetate is strongly endergonic (ΔG^o = +16,000 cal) and Lipmann's demonstration that acetyl-P is formed in the decomposition of pyruvate by *Lactobacillus delbruckii*, Barker proposed that acetyl-P might be the active acetate formed in the oxidation of ethanol by *C. kluyveri*.

It became possible to test this hypothesis with the discovery that dried cell preparations of *C. kluyveri* undergo autolysis in phosphate buffer, yielding cell-free extracts that contained all of the enzymes involved in the conversion of ethanol and acetate to lower fatty acids. These extracts also could utilize molecular oxygen as an electron acceptor for the oxidation of butyrate and caproate to acetate. Studies with these extracts confirmed that ethanol is oxidized to acetyl-P by a mechanism in which acetaldehyde is an intermediate. It was shown further that the cell-free extracts contained an enzyme system that catalyzed the transfer of the phosphoryl group of an unlabeled acetyl-P molecule to [^{14}C]-acetate to form [^{14}C]-acetyl-P, thus accounting for the equilibration of ethanol-derived active acetate with ordinary acetate. Of particular significance was the finding that *C. kluyveri* contained an enzyme (phosphotransacetylase) that catalyzed the reversible transfer of the acetyl moiety of unlabeled acetyl-P to [^{32}P]-labeled inorganic phosphate to form acetyl-^{32}P. Moreover, in the presence of arsenate the enzyme catalyzed rapid hydrolysis of acetyl-P to acetate and inorganic phosphate. These results were reminiscent of the demonstration in 1947 by Doudoroff, Barker, and Hassid that sucrose phosphorylase catalyzes the arsenolysis of glucose-1-phosphate. This led them to postulate that the reaction proceeds by a mechanism in which the glucose moiety of glucose-1-phosphate is transferred to a site on the enzyme with release of phosphate and that substitution of arsenate for phosphate in the reverse reaction leads to the unstable glucose-1-arsenate derivative that undergoes spontaneous hydrolysis to form glucose. By analogy Barker suggested that the arsenolysis of acetyl-P might involve the formation of an acetyl-enzyme intermediate. However, he recognized the possibility that the results could also be explained if phosphotransacetylase catalyzed the reversible transfer of the acetyl group to an undefined cofactor in the cell extracts, possibly coenzyme A (discussed below).

It was subsequently found that molecular hydrogen could serve as the electron donor for the reduction of acetyl-P and acetate to butyrate and that molecular oxygen could serve as an electron acceptor for the oxidation of butyrate to acetate and acetyl-P. Thus, it became evident that the involvement of postulated intermediates in fatty acid synthesis could be determined by manometric measurements of hydrogen or oxygen consumption when the postulated intermediate was incubated with crude cell-free extracts. Of 15 possible intermediates examined, only 2—acetoacetate and vinylacetate—were metabolized by the extracts, however the roles of these two substances as free intermediates in either butyrate synthesis or oxidation were excluded by a number of criteria. Most important was the fact that there was no incorporation of radioactivity in either substance when added to incubation mixtures catalyzing the overall oxygen-dependent oxidation of [^{14}C]-butyrate to [^{14}C]-acetyl-P and [^{14}C]-acetate or during the hydrogen-dependent reduction of the latter labeled compounds to [^{14}C]-butyrate. It was therefore concluded that 4-carbon carboxylic acids at various states of oxidation do not normally occur as free intermediates but are present only as activated derivatives or as enzyme complexes that do not readily equilibrate with the free acids.

While Barker's studies on fatty acid metabolism were in progress, studies in Lipmann's laboratory (1945-49) on the mechanism of sulfonamide acetylation by pigeon liver extracts led to discovery of a new form of active acetate that was produced by a reaction of ATP with acetate and a new coenzyme, which he named coenzyme A (CoA). Further studies in Lipmann's laboratory by G. D. Novelli, M. Soodak, and N. O. Kaplan showed that CoA is composed of adenosine-5'-phosphate pantothenic acid and a sulfhydryl moiety. The biochemical importance of acetyl-CoA became evident from studies in the laboratories of F. Lipmann, S. Ochoa, and D. Nachmansohn showing that acetyl-CoA is implicated in the acetylation of choline, in the synthesis of citrate and acetoacetate, and in pyruvate metabolism. In view of the fact that extracts of *C. kluyveri* were found to contain high concentrations of pantothenic acid, Barker proposed that CoA might be implicated in the fatty acid metabolism of this organism. Subsequently, studies in Lipmann's laboratory by E. Stadtman and G. D. Novelli showed that the arsenolysis of acetyl-P by phosphotransacetylase from *C. kluyveri* is dependent upon the presence of CoA, thus confirming Barker's suggestion that acetyl-CoA might be involved in this reaction. About the same time F. Lynen and E. Reichert (in 1951) succeeded in isolating acetyl-CoA and demonstrated that its synthesis involves acetylation of the free sulfhydryl group of CoA to form a thiolester derivative.

At the first Symposium on Phosphorous Metabolism held in Baltimore, Maryland, in the spring of 1951 Barker reviewed the results of studies with enzyme preparations of *C. kluyveri* as well as complementary studies in the field of CoA metabolism. Based on a most impressive critical analysis of the available information he proposed that the oxidation of butyrate by extracts of *C. kluyveri* occurs by a mechanism in which butyrate is first converted to butyryl-CoA and in which all of the 4-carbon intermediates involved in its oxidation exist as their CoA derivatives. Then the last of these to be formed, acetoacetyl-CoA, is cleaved by reaction with a molecule of free CoA to form two molecules of acetyl-CoA, which in the presence of phosphate is converted by phosphotransetylase to acetyl-P and CoA. Significantly, when Barker first proposed this scheme for fatty acid oxidation, there was only inferential evidence for a role of CoA in the oxidation of butyrate by extracts of *C. kluyveri* and there was no evidence of any kind to implicate CoA in the oxidation of fatty acids by animal enzyme systems. It is therefore a tribute to Barker's imagination and conceptual analysis that within a few years after his report his hypothesis was shown to be correct in every significant detail, not only in *C. kluyveri* but in animals as well.

GLUTAMATE FERMENTATION AND DISCOVERY OF THE B_{12} COENZYMES

During his postdoctoral year in Delft in 1936 Barker isolated the glutamate-fermenting bacterium *Clostridium tetanomorphum* and determined that the ratio of products of the fermentation—acetate, butyrate, ammonia, and CO_2—was incompatible with the degradation of glutamate via the reactions associated with the tricarboxylic acid cycle. In the 1950s he and his student J. T. Wachsman returned to the problem of glutamate fermentation and showed by analysis of the products of fermentation of [^{14}C]glutamate that the degradation must involve a previously unknown pathway. Studies with cell-free extracts led to the isolation and identification of mesaconic acid and later β-methyl-L-aspartate as intermediates in the fermentation. Chemical degradation of [^{14}C]mesaconate formed from [4-^{14}C]glutamate led to the surprising conclusion that the methyl group of mesaconate (and β-methylaspartate) originated from C-3 of glutamate and that a novel isomerization of the carbon skeleton of glutamate, namely, migration of the C-1 + C-2 "glycyl" moiety from C-3 to C-4, must occur during its conversion to β-methylaspartate. In 1956 A. Munch-Petersen found that the isomerization was inhibited by charcoal treatment of cell-free extracts and began attempts to isolate and identify the charcoal-absorbable cofactor. After considerable difficulty the identification of the cofactor as a novel form of pseudovitamin B_{12} in 1958 by H. Weissbach and R. D. Smyth was made possible by two advances: development of a rapid spectrophotometric assay for glutamate mutase—the enzyme catalyzing the reversible isomerization of glutamate and β-methylaspartate—and most important of all the discovery that the coenzyme forms of vitamin B_{12} are rapidly destroyed by light. Barker and his coworkers soon isolated several forms of the new coenzyme in pure form and showed that it contained the elements of adenine and a pentose linked to the corrinoid. Lenhert and Hodgkin demonstrated in 1961 by X-ray diffraction analysis that the coenzyme was formed by the direct ligation of C-5 of 5′-deoxyadenosine to the Co atom at the center of the corrinoid ring of vitamin B_{12}. This was the first demonstration of the existence of a biologically stable and functional carbon-metal bond. Barker has described the detailed path of this beautiful series of discoveries; they could serve as a textbook example of the careful, thoughtful, and insightful conduct of research.

The discovery of the coenzyme forms of vitamin B_{12} was quickly followed by the demonstration of their involvement in the methylmalonyl coenzyme A mutase, diol dehydratase, and ethanolamine dehydratase reactions. R. L. Blakley and Barker discovered the involvement of coenzyme B_{12} (now called deoxyadenosylcobalamin) in the ribonucleotide reductase reaction of *Lactobacillus leichmanii* in 1965. The discovery of the involvement of the B_{12} coenzymes in these and other enzymatic reactions led to a period of intensive study of the mechanism of the coenzyme's involvement in catalysis. By a combination of experiments with isotopic tracers and EPR spectroscopy and use of substrate analogues,

R. H. Abeles, J. Stubbe, and others established our current view of these reactions. Transient homolytic cleavage of the 5′-deoxyadenosyl carbon-corrinoid cobalt bond in the enzyme-bound coenzyme leads to formation of a carbon free radical, which in turn abstracts a hydrogen atom from the substrate, leaving a carbon radical form of the substrate, which undergoes rearrangement of the carbon skeleton or dehydration. The substrate product radical then accepts a hydrogen atom from the 5′-carbon of the 5′-deoxyadenosyl coenzyme intermediate, and the coenzyme returns to its original state.

The discovery of the carbon-cobalt bond in deoxyadenosylcobalamin also led to the recognition that C-alkylcorrinoids could play roles in reactions of methionine biosynthesis and methanogenesis involving Co-methyl-B_{12} intermediates and in biogenesis of acetate from CO_2 via enzyme-bound Co-carboxymethyl-B_{12}. The structures of a number of coenzyme B_{12}-dependent enzymes, including glutamate mutase from *C. tetanomorphum*, have been determined at high resolution. While questions of detailed mechanism of catalysis remain, the biochemical roles of corrinoids are well understood today, thanks to Barker's discoveries.

LYSINE FERMENTATION AND THE "RADICAL SAM" ENZYME FAMILY

Toward the end of his research career Barker turned his attention to the pathway of lysine fermentation by anaerobic bacteria. When Olga Rochovansky, a student of Sarah Ratner's, came to his laboratory in the mid-1960s, Rochovansky selected the lysine fermentation for study. Her interest in this fermentation had been stimulated by discussions of this topic with Sarah Ratner and Thressa Stadtman, a visitor in the New York laboratory. In Barker's laboratory a new *Clostridium* species that converted lysine to fatty acids and ammonia was isolated. Cell-free extracts were shown to form acetate from carbons 1 and 2 of lysine, which followed one of the two types of lysine cleavage reactions previously established in 1954. Costilow, Rochovansky, and Barker discovered that a more basic intermediate—3,6-diaminohexanoate, or β-lysine—was initially formed from L-lysine and could be separated by electrophoresis from it. The isolation of β-lysine provided the first lead in the elucidation of a novel pathway of lysine metabolism. The enzyme that catalyzes conversion of L-lysine to β-lysine—lysine 2,3-aminomutase—was oxygen labile and was stimulated by a combination of pyridoxal phosphate, ferrous iron, and S-adenosylmethionine. The full biochemical significance of these initial findings was revealed only in 1990s by an elegant series of investigations by Perry Frey and his collaborators. Remarkably, lysine 2,3-aminomutase resembles the coenzyme B_{12}-dependent enzymes in that it involves enzyme-bound cobalt and the transient formation of an enzyme-bound 5′-deoxyadenosyl free radical. The latter initiates formation of free-radical forms of lysine (bound in Schiff's base form to pyridoxal phosphate). Radical migration accompanies isomerization between lysine and β-lysine. The role of S-adenosylmethionine in radical formation has led to its description as a "poor man's adenosylcobalamin." Now it is recognized that the mechanism of lysine 2,3-aminomutase is also found in a number of other diverse enzymes, which constitute the so-called "radical SAM" enzyme family. Members include the pyruvate formate-lyase activating enzyme, anaerobic ribonucleotide reductase, biotin synthase, and probably lipoate synthase and benzylsuccinate synthase or its activating enzyme. As had occurred so often previously the careful analysis of the biochemistry of a seemingly obscure metabolic pathway in a little studied anaerobic bacterium by Barker and his coworkers led to the discovery of novel and highly unexpected biochemical mechanisms of great general significance.

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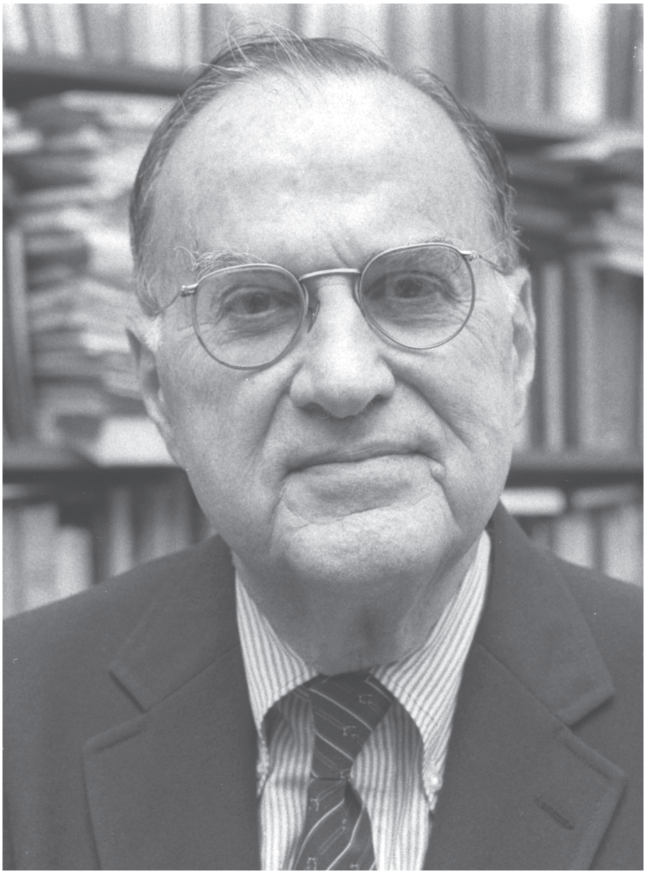

Abram Bergson

ABRAM BERGSON

April 21, 1914–April 23, 2003

BY PAUL A. SAMUELSON

OVER THE LAST TWO-THIRDS of the twentieth century Abram Bergson was a leading American and world economist. He was a creative theorist, both literary and mathematical. Bergson was also a careful statistical empiricist who, from a bully pulpit at Harvard, earned a reputation as the dean of Soviet studies and teacher of that subject's major scholars.

At a young age in 1933 Abram came to the Harvard Graduate School in economics after undergraduate training at Johns Hopkins (where he was a hometown commuter). Adolph Hitler was responsible for new foreign blood arriving in Cambridge to trigger a prewar Harvard renaissance in economics. When Bergson died at age 89, he was the last survivor of Harvard's age of Frank Taussig, and had been a young star in the new age of Joseph Schumpeter, youthful Wassily Leontief, eclectic Gottfried Haberler, and after 1937 Alvin Hansen, the "American Keynesian." As Leontief's second protégé I am proud to have been preceded by Abram Bergson, his first protégé. I would be honored to be known as Bergson's first protégé, for much of my own work in welfare economics owes virtually everything to his classic 1938 *Quarterly Journal of Economics* article that for the first time clarified this subject.

Two of Bergson's most cited papers actually appeared under the authorship of Abram Burk. Burk was indeed the name he had been born with. How A. Burk became A. Bergson is a tale worth telling, both as a reflection of what American academic and ordinary life was like 70 years ago, and for what it tells about his own straight-arrow character.

Abram's older brother Gus (Gustav Burk) studied Harvard graduate physics at the same time that Abe was studying economics. (Reliable family legend tells that Gus's skill in Baltimore poker games won for his junior brother private tutoring in the economics that he would need at Harvard.) Gus Burk particularly felt uncomfortable in having a name that did not correctly identify him as being the son of Russian immigrant Jews. So the two decided legally to change their surname. Abram sought my advice on the tentative substitution of Bergson for Burk. That struck me as an excellent choice: "It makes the point, but does not rub it in." Still Abram dithered: "You don't suspect, Paul, that some will think I'm trying to travel on the prestige of the great French philosopher Henri Bergson?" I put that probability down to near zero. The rest is history. And the old Brahmin *Boston Transcript* wrote a laudatory editorial commending this reverse instance of an opposite common pattern. In the end no significant citation confusion resulted from this early career decision.

Having by 1937 already achieved wide respect as a mathematical economist, serious Abram decided he would add a second string to his bow. Accordingly he learned the Russian language, and made a lengthy research visit to Moscow. Nineteen thirty-seven was the precise year when Stalin was liquidating on a large scale dissidents and innocents as enemies of the revolution. In later reflection Bergson reported how astonishing it had been that none of the many scholars he talked to—most of whom must have known family members and neighbors who were imprisoned or killed—communicated complaints to a naive American visitor.

By 1940 Bergson had written for publication his Harvard Ph.D. thesis. Thereafter at the wartime Office of Strategic Services, at Columbia, at the RAND think tank in Santa Monica, and after 1956 as tenured Harvard professor, Abram Bergson divided his time and energies between pure economic theory and the Soviet economics specialty. After the 1940-42 years at the University of Texas, Austin, Bergson spent most of the World War II years as head of the Russian desk at the Office of Strategic Services. Then at war's end Columbia called him to an economics chair. A decade later at Harvard, after 1956, he taught scores of theorists and Kremlinologists.

Many of the *cognoscenti* at the frontier of modern welfare economics—I being one of them—expected Stockholm to wake up to Bergson's merits. Alone, along with Ian Little or John Harsanyi or John Rawls, a Bergson prize could have added luster to the new post-1968 Alfred Nobel awards in economics. My tentative guess as to why that never did happen goes as follows. Kenneth Arrow's monumental work on the impossibility of *any* constitutional method of voting that would satisfy half-a-dozen plausible desirable axioms, that great theorem somehow got confused in nonspecialists' minds as being a proof against *the possible existence* of the quite different animal of the Bergson Ethical Normative Function. The history of every science contains some history of confusions, and economics is no exception to this.

In connection with ethical value judgments Bergson clarified how they could be distinguishable from testable empirical relations, a problem inadequately grappled with by Lionel Robbins (1932). Bentham, J. S. Mill, Edgeworth, as well as Pareto, Myrdahl, Lerner, Hicks, Kaldor, Scitovsky, Vickery, and Little could be given *coherent* interpretation in light of Burk-Bergson (1938).

Vilfredo Pareto in the years 1892-1913 brought important excellent insights into the post-Bentham utilitarian methodologies of Anglo-Saxon normative economics. But Pareto was an isolated pioneer, self-indulgent in his expositions as is not surprising in an autodidact. Serious Abram pondered important questions such as whether what we have come to call "Pareto optimality," which in even vaguer formulations is already in Mill (1848) if not indeed already in Adam Smith's "invisible hand" (1776), was a singular "*the* optimum" rather than (as in Francis Edgeworth [1881]) an infinity of incommensurable optima. My re-readings with him could not resolve the interpretations. Bergson's insightful happy thought was first to understand that *any* ethical code is, in the language of Arrow (1951), "imposed." The "just" person does not give his second coat to a naked beggar because that happens to tickle his fancy that Monday. It is his credo that requires him to do that.

However, using the useful device of an Individualistic Social Welfare Function—a special case that economists like to contemplate—Bergson could derive Pareto optimality conditions as *necessary* but not sufficient conditions for defining interpersonal normative equity. (Later Leontief and Franklin Fisher elaborated on "weak" mathematical separability and "strong" separability; earlier Irving Fisher had formulated testable conditions for Bentham-like additive hedonism; and as late as 1955 John Harsanyi restored some credence to pre-Bergson cardinality of individual utilities and of Social Cardinal Utility in the light of stochastic choosers sometimes feeling obliged to pay respect to the Independence Axiom in post-von Neumann argumentations about Laplacian Expected Utility. Few National Academy of Sciences readers need to understand this name dropping, inasmuch as out of any one hundred 2003 graduate economic students in the Ivy League and Big Ten, my Bayesian estimate is that almost none of these professionals do comprehend these nuances.)

What needs to be stressed is that Bergson's Social Welfare Function(s) left plenty of room for ethical credos that ordained *duties* and for which separate Pareto-optimality conditions could not even be defined. In the language of Richard Musgrave's magisterial *The Theory of Public Finance* (1958), "merit wants" that are so unpopular on the University of Chicago midway do exist. Some societies might even be unanimous in voting a fair military draft, even though every young voter is unwilling to be a volunteer. (God is in the ad libs. I knew a libertarian economist who was against the tyranny of coercive traffic lights. My spies reported that, nevertheless, commuting to daily work he revealed a preference for the longer route over the lights-free shorter router: His gut knew more about the algebraic pluses and minuses of the calculus of "liberty" than his conscious mind did.)

Abram Bergson was a realist par excellence. He applied generous reasoned discounts to the statistical growth claims of the Stalinist and post-Stalinist statisticians. And yet, after the dozen post-Gorbachev years of communist dissolution the emerging evidence suggests to me—and I think to "Honest Abe" as he was known at Harvard—that the Soviet system was even less productive in most sectors than the international almanacs had estimated. Why? Plain Machiavellian lying? No doubt there was some of that, as all our experts did recognize.

More important, I suggest after much reflection, is the fact that what are called "prices" in a controlled society have little true relationship to relative scarcities and technical trade-off costs. From copious nonmeaningful statistical inputs will have to come quite nonmeaningful statistical estimates. One wonders whether some future transformation of Mao's Chinese economic system will thereafter reveal how hard it is for scholars to gauge correctly how deep China's present-day discount factors ought to be.

Before Schumpeter died in 1950 that learned scholar had to feel some jealousy of John Maynard Keynes, who gained recognition as the twentieth century's greatest economist. Our master therefore missed what he would have certainly relished, namely, his burgeoning posthumous fame. Innovation and long-term trends today command some of the interest and energy that had previously belonged to equilibrium statics and macroeconomic business cycle fluctuations.

Moreover, the fact that widow Elizabeth Boody Schumpeter bequeathed to the Harvard Archives *all* his papers, personal and private, and even those that discuss in an obscure German shorthand the pros and cons of not marrying her, that understandably created a cottage industry in Schumpeter biographies. One of the best and most balanced of those biographies on Joseph Schumpeter, that by the Swedish economic sociologist Richard Swedberg (1991), raised an important question. In my paraphrase the biographer at one point writes, "Now I must ask the following question. Can we judge Joseph Alois Schumpeter to have been a fervent friend to mankind? On the basis of all the known evidence, perhaps no firm answer can be given to this question."

No biographer of Abram Bergson could be in doubt about his personal attitudes and modesty. I have made stronger claims on his behalf than he ever made in print. He was no shrinking violet. Thus when he found some faults in the mathematical writing of Ragnar Frisch (who later was deservedly to share the *first* Bank of Sweden-established Nobel Prize in Economics in 1969), Bergson did stand up to that great and self-confident man. (In Bergson [1936], written when the author was only 24, will be found an earliest formulation of the Constant Elasticity of Substitution Function, which outside of consumer utility analysis, became widely used in production theory; it is also a workhorse in modern finance theory as the one case where optimal portfolio ratios are independent to whether wealth is large or small. This is but one of Bergson's theoretical novelties.)

Those who knew Abram Bergson and knew his informed views on Smith, Marx, Franklin D. Roosevelt, Lenin, and Stalin will judge him to have been a man of the center with a personal preference toward less economic inequality. That majority view among his generation of economists (and mine), perusal of the published literature will confirm, has lost its preponderant majority as the Great Depression and World War II recede farther into history. Libertarianism à la Milton Friedman and Friedreich Hayek has gained in strength. Outside the academy, among voters in general there has been a similar erosion of "altruism." However, with the weakening of "altruism" and the gaining of "my wallet" motivations, I detect no logical or empirical tie-up with libertarianism as such. Also among academics in or outside economics there has taken place little popularity for fundamentalist religions.

Straight-arrow honest people can sometimes seem to many of us naïve—refreshingly naive. Bergson provides such an example. His lack of guile is illustrated by the following anecdote. Abram was a close friend of Harvard's learned Alexander Gerschenkron, who taught economic history and did so as a tough nonelective. Among students and young faculty almost a rebellion was brewing. Therefore a committee was appointed to review requirements. Bergson was asked to be its chairman. If he had asked advice from his Machiavellian MIT friend, Paul Anthony Samuelson, he would not have touched that third-rail topic with a 10-foot insulated pole. Honest Abe was never Machiavellian. He accepted the draft. And, inevitably, by strong majority the committee recommended new and much lighter economic history requirements. Gerschenkron, a strong believer in what he believed in, never quite forgave Honest Abe. A lifetime friendship was strained. Someone else could have been chairperson, as I think Abe came to realize belatedly.

Once in Bergson's rare reminiscing about his Baltimore youth, he mentioned that Gus and he would organize a number of new neighborhood clubs. Their main purpose seemed to be primarily to decide who would be *excluded* from them. Later I learned that being elected to honorific academies was somewhat similar. Energy on research gets you into the Academy; for example, Bergson was elected to the National Academy of Sciences in 1980. After that your time for research becomes compromised by duties to serve on research and nominating membership committees, whose main function is to decide just which worthies will be the ones *not* to be elected.

Back a long time before Bergson's death we had lunch together at the Harvard Faculty Club. A Harvard scholar came by whom I had known a long time, saying hello to me and passing on. At this point, as old friends will gossip to each other, big-mouth Samuelson said: "I wish that guy would not be so sharp with his wife." Abe's response was: "I'm glad to hear you say that." Surprised, I said, "Why should you want an acquaintance to be unkind?" "I don't," Bergson explained, "It's just that he's being so mean to me, and I thought it was something personal."

In the high-pressure atmosphere of modern university life, true character ultimately reveals itself—for better or worse. Abram Bergson over a long career earned from teachers, pupils, colleagues, and friends much affection and admiration. His wife, Rita Macht Bergson, herself from an academic Baltimore background, played an important role in their family and professional lives. I owe to their three achieving daughters—Judy, Mimi, and Lucy—much informal help in preparing this affectionate memoir.

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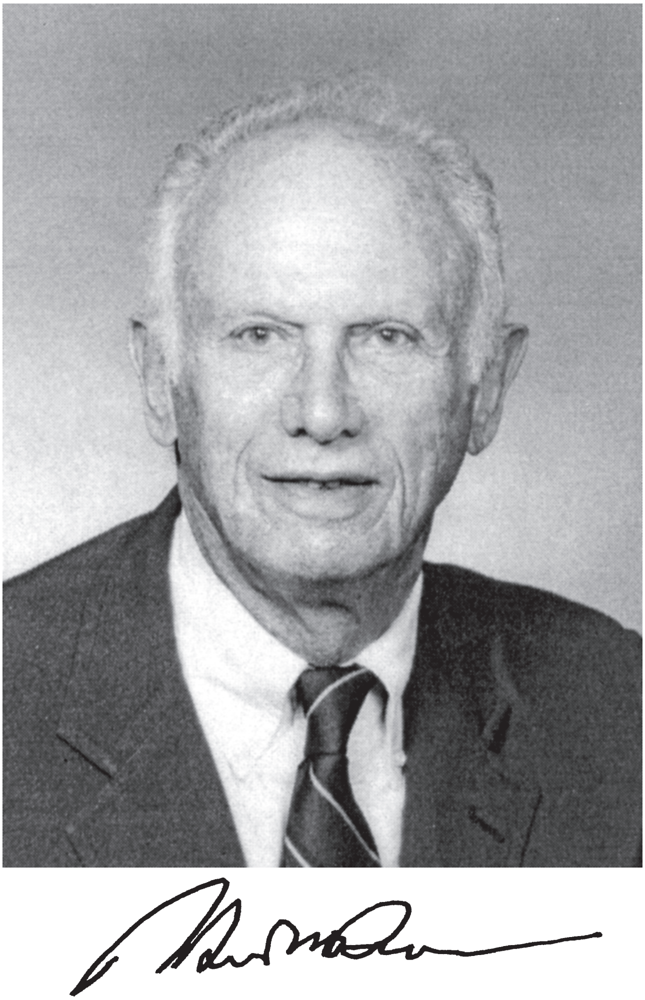

ROBERT M. BERNE

April 22, 1918–October 4, 2001

BY MATTHEW N. LEVY

ROBERT M. ("BOB") BERNE, an acclaimed authority in the field of cardiovascular physiology, died on October 4, 2001, at the age of 83. Bob was born in 1918 in Yonkers, New York, and he grew up in Brooklyn. He attended college at the University of North Carolina and received his bachelor's degree in 1939. He graduated from Harvard Medical School in 1943. He became an instructor in the Physiology Department of Western Reserve University in 1949 and rose to the rank of professor in that department in 1961. Bob moved to the University of Virginia in 1966, at which time he assumed the position of chairman of the Physiology Department. He carried out impressive research in the fields of the coronary circulation and cardiac metabolism. He became professor emeritus at the University of Virginia in 1994.

In the first year of Bob's life he, his father, and his mother all contracted influenza during the terrible epidemic of 1918. Fortunately they all survived. The family moved subsequently to Dobbs Ferry and to the Bronx, and then to Brooklyn, where Bob entered the first grade in public school. Bob described his teacher as a huge, redheaded disciplinarian, who insisted that the students remain quiet for interminably long times. Bob was relieved when his mother became a physical education teacher at the Brooklyn Ethical Culture School, a private school that accepted Bob and his sister as students.

Bob recalled that as he became more and more secure at this school, he became progressively more mischievous. After each of these episodes his mother would receive an angry report from one of his teachers. Finally his mother decided that he should be transferred to a public school. Even though Bob did well academically in this public school, and actually skipped a semester, he was very resentful because his sister lavished in the private school.

During his grammar school years the section of Brooklyn in which Bob lived was very safe. His family gave him the freedom to travel anywhere in the city that he wished to go. During his boyhood years, organized activities, such as Little League baseball, basketball, and soccer, did not interest him. Instead he participated in the popular neighborhood street activities, such as stickball, roller-skate hockey, touch football, and stoopball. He was an aggressive athlete and had a fervent desire to win. He was frequently embroiled in fistfights over trivial disagreements with his playmates, and he often returned home with a black eye or a bloody nose.

During the summers of his grammar school years his activities and surroundings were entirely different. His family rented a small cottage in the countryside near Albany, New York, for the entire summer. The cottage had no electricity, running water, or indoor toilets. Nearby were a small lake and a riding academy. During the summers Bob spent most of his daylight hours at the stable, cleaning the horses and stalls, feeding and watering the horses, and teaching horseback riding.

When Bob was ready to enter high school, he elected to attend Boys High School. This was the school that his father and uncles had attended, and it had an excellent academic reputation. The school encouraged academic competitiveness by posting the 50 highest scholastic averages at the end of each term.

During his high-school years Bob was very short and thin. Even though his mother fed him a high-calorie diet, he failed to gain weight adequately. He developed a persistent cough, and his doctor suspected that he had acquired tuberculosis. Sputum examinations and chest X rays were negative, but his tuberculin test was positive. He was removed from school and was treated with bed rest for six months. Bob felt that this period was the most difficult time in his life, and he became very depressed and inactive. Fortunately his health and mental activities gradually improved. He finally went back to high school, at first on a half-day schedule and then on a full-day schedule. He gradually regained his strength and vitality and ultimately caught up with his class.

Bob was accepted for admission to the University of North Carolina. He found college to be a liberating experience, and he was delighted to find that he had substantial time for extracurricular activities. He quickly discovered girls and alcohol. His first experience with the latter was not too pleasant. He became inebriated, broke a window in a movie theater, and spent the night in jail. He worried that he would be expelled from school, but fortunately there were no repercussions. Bob majored in chemistry and was especially enthusiastic about organic chemistry.

Bob applied to a number of medical schools, and he was delighted to be accepted to Harvard Medical School. His elation faded quickly, however, because he found that the first-semester courses involved the various aspects of anatomy, and the major learning activity involved rote memorization. At the end of the first semester of medical school

Bob was so bored with medical school that he considered discontinuing his medical education.

He found the second-semester courses, physiology and biochemistry, to be somewhat more exciting. At that time biochemistry was still not very advanced, and so the major tutorial activities involved learning the techniques for assessing the contents of various organic compounds in blood and urine. He did find physiology to be somewhat more exciting. The professor, Walter Cannon, presented the material very well, but he terrorized the students periodically by presenting them with very difficult questions.

The second year of medical school included courses in microbiology, pharmacology, and pathology. The professor of microbiology was John Enders, an excellent teacher who ultimately was awarded a Nobel Prize. One of Bob's classmates mishandled cultures of typhoid bacilli and contracted typhoid fever.

The third and fourth years, the clinical years, of medical school were very busy ones. The clinical studies were carried out mainly at the Brigham Hospital. When Bob conducted his studies at that hospital, most of the interns were sick with viral pneumonia, and the medical students assumed the clinical roles of the sick interns. During his clinical activities Bob became acquainted with a clinical teacher, Dr. Weinman, who was conducting research on toxoplasmosis. Bob asked Dr. Weinman whether he could carry out a study of the effects of certain sulfa drugs on mice that were infected with toxoplasmosis. This constituted Bob's first adventure in medical research, and he was thrilled to learn that his research efforts were included subsequently in a paper published in the *Journal of the American Medical Association*. The combination of the clinical and investigative efforts led to Bob's working from about seven each morning until about three the next morning. He was so absorbed with his clinical and investigative efforts that he ignored the chronic fatigue that was caused by his lack of adequate sleep.

After the completion of his second year of medical school, Bob had a one-month vacation from school. During that month he worked as a chauffeur for a medical doctor in New York City. The doctor's wife and two daughters stayed at a summer home about 50 miles from the city. Bob frequently drove the doctor back and forth between the city and the summer home. Bob loved this job, because he could enjoy tennis, swimming, canoeing, and sumptuous meals. During this month away from school, Bob did not know that Beth, the doctor's daughter, who was then only 14 years old, would become his wife just 4 years later. Ultimately Bob and Beth had four lovely children: two girls and two boys. The daughters were Julie and Amie and the sons were Gordon and Michael. As the children matured, Julie became a social worker and has lived mostly in Charlottesville. Amie became a lawyer and has lived mostly in Atlanta. Gordon became an architectural landscaper and has spent most of his life in Charlottesville. Michael became a medical researcher and has lived mostly in Boston.

In April of 1943 Bob began his medical internship at Mt. Sinai Hospital in New York City. The internship, which lasted for nine months, was exhausting. Bob worked all day long every day and all night long on alternate nights. Financial compensation consisted of room, board, and laundry. His assistant residency, which also was nine months long, had a daily work schedule that closely resembled that of his internship. In March of 1944 Bob and Beth became engaged to be married, and a large wedding party was planned for May of that same year. However, Bob was afflicted with hepatitis shortly after their engagement was announced, and one month later he acquired a streptococcal laryngitis. Their plans for a sumptuous wedding were reluctantly discarded and instead a small wedding party was held in his in-laws' home.

Bob's experience in the U.S. Army began in October of 1944 in Carlisle, Pennsylvania, where he received his basic training. After six weeks he was assigned to Fort Jackson in South Carolina as a medical officer, but his medical duties lasted only three months. He was then transported by troop ship to the Pacific island of Luzon, where the troops were training for the invasion of Japan. Twice each day Bob and his associates practiced going over the sides of landing crafts in preparation for storming a beach. The atomic bomb explosions freed Bob and his associates from the requirement of invading Japan.

Shortly after the peace agreement between Japan and the United States was signed, Bob sailed to Japan, arriving in Wakayama late in September. Over the next several months Bob was assigned as a medical officer to several cities on the island of Honshu. Bob's major duties as a medical officer in Japan were to treat venereal diseases that afflicted a substantial portion of U.S. military personnel. Bob finally was permitted to return to the United States in October of 1946.

After his discharge from the Army, Bob returned to the Mount Sinai Hospital in New York to complete his training in internal medicine. Bob was appointed chief resident of the medical service headed by Dr. I. Snapper. At the beginning of this appointment Bob collaborated with Dr. Snapper on a research investigation of the treatment of multiple myeloma. Dr. Snapper was a very demanding mentor who had an encyclopedic knowledge of medicine. During this residency Bob's mother developed metastatic cancer of the lungs, and Bob became involved in her treatment, including diagnostic thoracenteses two or three times per week.

Bob was not able to discuss his mother's true diagnosis, for which failure he felt very guilty thereafter.

Bob Berne and I first met in Cleveland, Ohio, in August of 1948. On this day we began our academic activities as instructors in the Physiology Department of Western Reserve University Medical School (now Case Western Reserve University). I remember the month and year so well, because the Cleveland Indians were then involved in an exciting baseball pennant race. Ordinarily the Cleveland Indians were underdogs, but in late August of 1948 they were leading the New York Yankees in the American League. Bob was an ardent Yankees fan, a graduate of Harvard Medical School, and he had just completed his medical residency at Mt. Sinai Hospital in New York. I was a graduate of Western Reserve University Medical School, and I had just completed a two-year term as a medical officer in the U.S. Army. When Bob and I first met, a warm friendship developed almost immediately, despite the Yankees-Indians rivalry that continued throughout that summer until the World Series ended (with a surprising victory for Cleveland, I must add).

Our salaries were about $2,000 per annum. This amount certainly did not provide for an opulent life style, even though the value of the dollar then was much greater than it is today. The financial standard of living was no more opulent in the Physiology Department than it was at home. Professor Carl Wiggers, the chairman of the Physiology Department and a world-renowned medical scientist and educator, informed us promptly that his department did not have sufficient funds to enable him to hire technicians and other assistants. The members of the Physiology Department at that time were organized into teams of two members each. Wiggers explained that Bob and I would constitute one such team, and that on alternate workdays one of us would be the acting scientist and the partner would be the acting technician. The roles would be reversed on the intervening workdays.

Wiggers suggested that Bob and I would each study a different aspect of a canine model of congestive heart failure. Bob had a greater interest and familiarity with biochemical processes and technics, whereas I used a more bioengineering approach. Bob initiated a study of the effects of reduced cardiac output on renal function, whereas I began a study of the effects of diminished cardiac output on arterial and venous hemodynamics. The two projects required numerous assessments of renal function and blood oxygen content. The animal experiments themselves usually occupied an entire workday. We had the option either of completing our biochemical analyses before we went home late in the evening or of postponing the chemical analyses until the next morning. Bob advocated the first option, because his medical residency experience accustomed him to long periods of sleep deprivation, whereas sleep deprivation made me very uncomfortable. I acceded grudgingly to Bob's preference and we worked together throughout the long nights to complete our laboratory analyses. Fortunately Bob was such a pleasant, enthusiastic, and competent individual that in a very few weeks, I began to enjoy the grueling work schedule! My wife, Ruth, did not object strongly to my nocturnal adventures, although she asserted that I must be mentally deficient to follow such a strenuous routine. Bob's wife, Beth, did not mind the exhausting schedule so much, perhaps because she had long been accustomed to Bob's prolonged workdays during his internal medicine residency.

The laboratory where Bob and I worked was next door to Professor Wiggers's office. Wiggers suffered periodically from gout, and when this disease was especially painful, his mood was not very pleasant. The lighting in our laboratory was inadequate, and we persuaded Dr. Wiggers to have a new fluorescent light installed. A few days later an electrician came into our laboratory to install the new light fixture. The hammering evidently disturbed Dr. Wiggers, because he stormed into our laboratory and ordered the electrician to leave. He then shouted at us and insisted that this disturbance was inexcusable. He added that when he was a young scientist and needed greater illumination, he would activate a kerosene lamp, which was of course silent. Ever since that experience Bob and I referred to our fluorescent light as our kerosene lamp.

After serving one year as instructors in the Physiology Department, Bob and I were promoted to the rank of assistant professor. Our research activities progressed satisfactorily, and we also began to present lectures on various aspects of physiology to the medical students. Professor Wiggers was extremely conscientious about teaching the medical students. Consequently he monitored most of the lectures that we delivered to the medical students. After each lecture he discussed the good and bad features of the lecture and suggested how we might improve the quality of those lectures.

The Medical School of Case Western Reserve University developed a detailed syllabus, which they provided for the education of the medical students. Therefore, they did not recommend established, already published textbooks to their students. Furthermore the preclinical education system at our medical school was not organized on the classical basis of specific biological topics, such as anatomy, biochemistry, physiology, and pathology. Instead the curriculum was organized on an organ system basis, that is, when the cardiovascular system was being presented, faculty members from the various basic and clinical science departments would present the various anatomical, biochemical, physiological, and pathological aspects of the cardiovascular system. The medical school's syllabus reflected this rather unique orientation.

The medical school's curriculum was updated periodically. When it was time to update the cardiovascular subsection in the early 1960s, Bob and I were called upon to update the physiological aspects of that subsection. We collaborated very effectively and enthusiastically. Our major complaint, however, was that the artistic features (such as drawings and graphs) of the curriculum were defective; satisfactory photocopying devices were not yet available. To print the graphic material the curriculum department used mimeograph machines, which were the standard copying devices of that era. Consequently the quality and clarity of the graphs and drawings were unsatisfactory.

Fortunately an official of the Mosby Corporation, one of the major publishers of medical textbooks, invited Bob and me to produce a monograph on cardiovascular physiology. We were both enthusiastic about this prospect. We were confident that the artwork would be far superior to that produced by our curriculum department, and we were also confident that our reading audience would be expanded considerably. The monograph was successful almost immediately, and it eventually led to a full textbook of physiology, which has also been used extensively.

Overall, my academic associations with Bob Berne were very fulfilling. My social associations with Bob were also wonderful. Bob was not only an exceptional teacher and research scientist but he was also a very well-rounded individual. He was an excellent athlete and a superb tennis and squash player. Bob and Beth were devotees of the arts and of the good life, and they introduced my wife and me to many of their social and intellectual activities. Bob and Beth loved classical music, and they encouraged us to attend classical music concerts. Very shortly after they arrived in Cleveland we began to attend Cleveland Orchestra performances with them. The four of us sat together in the very last row of Severance Hall. Bob and Beth also introduced us to exquisite foods. Frequently on Saturday evenings we would patronize Cleveland's best seafood restaurant, where we gorged ourselves on lobster. In those days a complete meal that included a two-pound lobster cost only about one dollar.

Dr. Wiggers retired as chairman of the Physiology Department in the early 1950s, and he moved to an office in the nearby Cleveland Clinic. In that office he became the first editor of the prestigious journal *Circulation Research*. Shortly thereafter George Sayers became the new chairman of the Physiology Department of Case Western Reserve University. The major emphasis of the department shifted to endocrine physiology, and Bob Berne's principal interest focused on cardiac metabolism and the coronary circulation. Bob's early studies indicated that a labile vasodilator was released from the myocardial cells when the oxygen supply to those cells was inadequate. His experiments suggested strongly that adenosine was an important mediator of the coronary vascular dilatation.

Bob's studies on the adenosine hypothesis were postponed temporarily when he took a sabbatical with Professor E. C. Slater at the University of Amsterdam in 1959 and 1960. There he studied the "relaxing factor" in skeletal muscle. His two daughters attended the local school in Holland, and they quickly learned to speak Dutch fluently.

In 1964 Bob accepted the attractive offer to become the chairman of the Physiology Department of the University of Virginia. Bob held this prestigious position for the next 22 years. Shortly after he had moved to Charlottesville he accepted a sabbatical with Gustav Born at the Royal College of Surgeons in London. Bob and Professor Born shared a mutual interest in the effects of adenosine on the microcirculation. This collaboration led to an important series of studies on the effects of adenosine on the coronary circulation and on cardiac function. These studies were carried out in collaboration with Professor Brian Duling, who completed a postdoctoral fellowship under Berne's tutelage. Thereafter, Professor Duling became a valuable member of the Physiology Department and subsequently became the director of the Cardiovascular Research Center at the University of Virginia.

In collaboration with Professor Ted Rall, a member of the pharmacology department at the University of Virginia, Bob tested the hypothesis that adenosine receptors on the surface of the coronary vasculature could induce coronary vasodilation. Furthermore, the studies of Rall and Schrader revealed that adenosine could counteract the ability of β-adrenergic stimulation to elevate intracellular cyclic AMP and to increase the conductivity of the Ca^{++} channels in the myocardial cells. Luis Belardinelli, a postdoctoral fellow in Berne's laboratory, studied the electrophysiological effects of adenosine in the heart. In collaboration with Bob Berne, Luis determined the effects of adenosine on certain cardiac arrhythmias in human subjects. Preliminary studies in human subjects were very promising.

Bob Berne was an outstanding scientist, author, and teacher, and he was consequently elected to the National Academy of Science. Bob was a delightful person who enjoyed life fully. He was unpretentious, and he was a pleasure to communicate with. His family, his many friends, his collaborators, and his many admirers around the world miss him profoundly!

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Gerhard L. Closs

GERHARD LUDWIG CLOSS

May 1, 1928–May 24, 1992

BY HEINZ D. ROTH

When Gerhard Ludwig Closs succumbed to a massive heart attack on May 24, 1992, the work of one of the great organic chemists came to an untimely end. Professor Closs made significant contributions in four areas. He was an early leader in the field of carbene chemistry; he elaborated various significant aspects of the photosynthetic pigments; he pioneered important applications of magnetic resonance to characterize reaction intermediates; and he elucidated intricate facets of electron transfer chemistry. This biographical memoir provides a welcome opportunity to pay tribute to one of the outstanding chemists of the post-World War II era—and to a friend.

Gerhard Closs was born on May 1, 1928, in Wuppertal-Elberfeld, Germany, a small, bustling city known for its suspended tram (Schwebebahn) and for the pharmaceutical branch of Bayer, one of Germany's major chemical manufacturers. Before he could complete his high-school education he was pressed into military service as a 16-year-old in 1944. He barely survived the ordeal of war: He was seriously wounded on the eastern front. After the war he completed high school and enrolled in Universität Tübingen. Having received a Ph.D. degree in 1955 for work with Georg Wittig, he joined R. B. Woodward's group at Harvard for two years.

In 1957 he accepted a position as assistant professor at the University of Chicago as a natural products chemist.

Gerhard's early independent studies were assisted by Lieselotte E. Closs (née Pohmer), his wife and most productive coworker; their collaboration produced 15 publications between 1959 and 1969. Lieselotte also received a Ph.D. degree with Wittig for her classic work on the transient existence of dehydrobenzene.[1,2] She did postdoctoral work at MIT. Lieselotte and Gerhard were married on August 17, 1956, in Cambridge, Massachusetts.

Employment opportunities for women scientists were very limited in the 1950s and 1960s. University regulations against nepotism prohibited wives from holding paid positions in the same department as their husbands; therefore, Lieselotte could only work as an unpaid volunteer. The availability of a skilled coworker proved especially fortuitous when Gerhard entered the chemically induced dynamic nuclear polarization field (see below). Lieselotte re-entered the lab, carried out a few simple but elegant experiments to probe key aspects, and soon had results sufficient for two "Communications to the Editor."

Gerhard Closs was granted tenure in 1961 and was promoted to full professor just two years later. Almost 20 years later he accepted the position of section head in the Chemistry Division at Argonne National Laboratory, while remaining on the Chicago faculty. Although he kept this position for only three years, it significantly influenced the direction of his research in the final decade of his life.

The work of Gerhard Closs has been recognized at the University of Chicago and in the scientific community at large. He was appointed the Michelson Distinguished Service Professor, and his colleagues honored him along with N. C. Yang with a symposium on the occasion of their sixtieth birthdays. He was awarded the Jean Servas Stas Medal by the Belgian Chemical Society in 1971, the James Flack Norris and A. C. Cope awards by the American Chemical Society in 1974 and 1991, respectively, and the Photochemistry Prize by the Inter-American Photochemical Association in 1992. He was elected a member of the National Academy of Sciences in 1974 and the American Academy of Arts and Sciences the following year. The Inter-American Photochemical Association honors his memory with the G. L. Closs Memorial Award, which allows a student to present a research paper at one of its meetings.

In 1981 he also was honored as chairman of the Gordon Research Conference on free radical reactions and in 1990 the Gordon Research Conference on radical ions, and by many distinguished lectureships in the United States, Canada, Japan, and Europe. Among these were the Bayer Lectureship at Universität Köln, Germany (where the author first met him), a visiting professorship at Yale (where the acquaintance was renewed), regular visits to Bell Laboratories, and the Merck Distinguished Lectureship at Rutgers University (where the author was privileged to be his host).

Closs was a featured speaker at many national and international congresses and symposia, and his participation at meetings was a highly important contribution to science. He brought to these meetings a keen analytical mind and the command of an unequalled breadth of chemical topics: from subtle details of organic synthesis, to a deep understanding of mechanistic details, to the intricacies of chemical physics, and a keen chemical intuition. This combination allowed him to probe proposed theories or mechanisms as they were being presented. Few of his peers made more pertinent comments than Gerhard did, or in a more impertinent fashion when he felt it necessary. Even accomplished and experienced lecturers must have felt a tinge of apprehension when he raised his hand and, on being recognized, uttered his familiar, "I would like to take issue with . . .".

Gerhard Closs relaxed by sailing, and sometimes racing, his sailboat on Lake Michigan for hours, days, or weeks. He relished his fine collection of graphic art, he was stimulated by theater performances, including modern and avant-garde plays, and he enjoyed classical music. No matter how hard the author tried, however, Gerhard could not be persuaded to attend an opera performance. (He had sworn off opera as a teen in the early 1940s, following a performance of Da Ponte and Mozart's *Cosi Fan Tutti* in Wuppertal.)

During his 35 years at the University of Chicago, Gerhard developed a deep appreciation, even love, for his adopted country. He only bought American cars and "took issue" with many Americans and foreigners alike who dared to criticize the United States in his presence. One afternoon, while attending a Gordon Conference in New Hampshire, Gerhard was interviewed by a local reporter. Asked what he thought of consumers who bought foreign-made articles, he quickly voiced his disapproval and then added, having spotted the reporter's Japanese-made camera, "and that applies also to you."

With the death of Gerhard Closs the chemical sciences lost a most formidable champion, a practitioner of the highest intellectual standards, a keen mind, and a skilled experimenter who was always probing accepted theories and was never afraid to break new ground. The scientific community has lost a teacher, mentor, collaborator, and kin spirit, and a few who were privileged have lost a friend.

The earliest publications of Gerhard Closs stem from his thesis work with Wittig and describe ylid rearrangements with ring enlargement or contraction,[3] and from his postdoctoral training with R. B. Woodward (total synthesis of chlorophyll).[4] His first independent publications, for example, a paper on the active constituents of *Panaeolus venenosus*,[5] reflect his being hired as a natural products chemist. As part of the *venenosus* project the physiological effects of the mushroom were to be tested, and the young assistant professor volunteered for the study. The highly amusing conversation ensuing between Gerhard Closs and his physician and collaborator is part of the *Mycologia* publication.[5] It was often cited at Closs group festivities and never failed to amuse; coworkers fortunate to have obtained a reprint of this paper count it among their prized possessions.

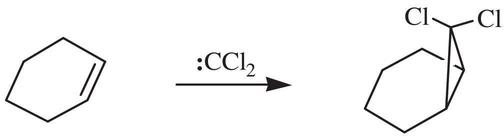
Gerhard Closs never lost interest in natural products and photosynthetic pigments. Seventeen of his 132 lifetime publications dealt with the chlorophylls; he contributed significantly to such important topics as linked chlorophyll dimers, photosynthetic reaction centers, and porphyrin metal complexes. Still, his most significant contributions came in three other fields of chemistry, one area for each decade of his professional career.

Gerhard Closs's first major contributions came in the emerging field of carbene chemistry; interestingly, a distant predecessor at the University of Chicago, John U. Nef, was an early champion of divalent carbon chemistry. Alas, Nef's interpretations of his results are at variance with the accepted definitions and the prevailing understanding in the field since the mid-twentieth century so that his work no longer qualifies as carbene chemistry.[6]

The actual roots of the carbene field lie in the base-catalyzed hydrolysis of trichloromethane by Geuther in 1862.[7] Hine repeated this experiment in 1949 and recognized the reaction as an α-elimination, the consecutive removal of H^+ and Cl^- from the same carbon, generating dichlorocarbene.[8] In 1954 Doering and Hoffman trapped the postulated species by addition to cyclohexene, demonstrating its intermolecular reactivity.[9]

$$CHCl_3 + OH^- \longrightarrow CCl_3^- + HOH \qquad \text{(eq. 1)}$$

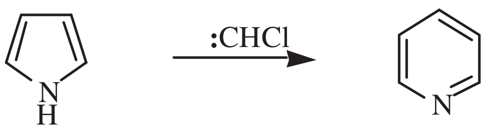
$$CCl_3^- \longrightarrow :CCl_2 + Cl^- \qquad \text{(eq. 2)}$$



(eq. 3)

The development of divalent carbon chemistry involved various facets: substituted carbenes; the notion of spin multiplicity; chemical studies probing carbene reactivity and the stereochemistry of their reactions; and the application of new physical methods (e.g., electron spin resonance, electron nuclear double resonance, chemically induced dynamic nuclear polarization, and optical spectroscopy). Gerhard Closs played a significant role in introducing these new techniques to the study of carbenes.

Closs generated chlorocarbene from methylene chloride; addition of the new carbene to alkenes, benzene, or phenol gave rise to chlorocyclopropanes,[10] tropylium chloride,[11] or tropone,[12] respectively. Five-membered heterocycles (e.g., pyrrole and indole) reacted with chlorocarbene by ring expansion.[13] It is tempting to see in these ring enlargements echoes of his doctoral thesis.



Additional carbenes arose by reaction of alkyl and benzal halides with organolithium compounds; here the term "carbenoid" was introduced to denote carbenes that appeared to be complexed (i. e., associated) with lithium halide.[14] The base-induced α-elimination of chloroalkenes formed cyclopropenes by intramolecular addition of alkenyl-carbenes.[15]

By the time of his promotion to associate professor he began to ask further-reaching questions; he decided to characterize carbenes more thoroughly and, if possible, observe them directly. Spectroscopic techniques available at this time included optical spectroscopy and electron spin resonance. Optical spectroscopy had received a recent boost by the advent of flash photolysis in 1949-50.[16] Herzberg observed the emission spectra of the parent methylene, CH_2, and its isotopomers CHD and CD_2, in 1961.[17]

Electron spin resonance (ESR) spectroscopy was a later development,[18] but by 1953 organic free radicals or radical ions had been studied. Gerhard Closs was fortunate to have Clyde Hutchison, an expert ESR spectroscopist, as a colleague. They generated diphenylmethylene at cryogenic temperatures in benzophenone crystals and observed the first ESR spectrum of a ground state triplet carbene in a single crystal.[19] About two weeks before the Chicago group, Edel Wasserman and coworkers at Bell Laboratories generated diphenylmethylene in a glassy matrix.[20] The single crystal approach of the Chicago collaborators lent itself to a more detailed analysis and interpretation. Ultimately, electron nuclear double resonance (ENDOR) revealed the detailed structure of this intermediate (see Figure 1).[21] Among Closs's additional ESR studies cyclopentanediyl and trimethylcyclopropenyl deserve special mention.

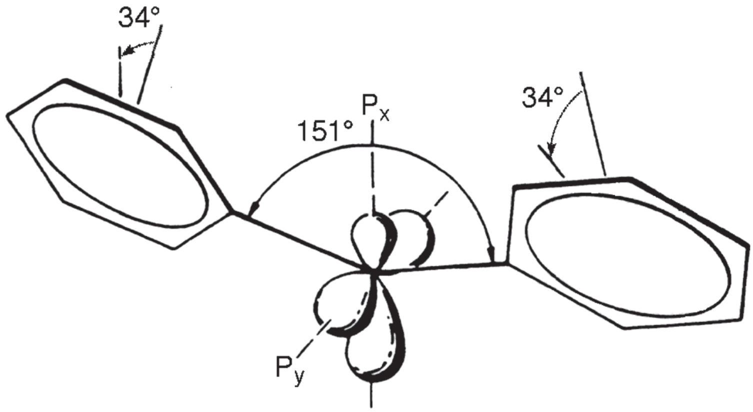
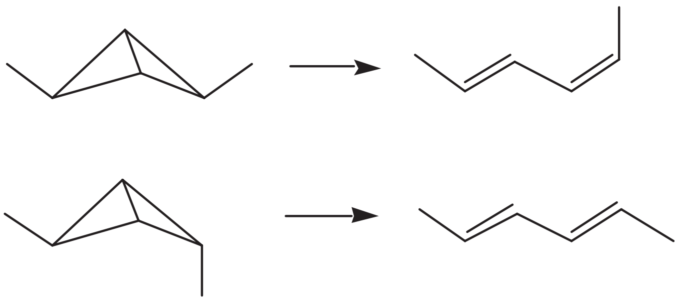


FIGURE 1 Structure of diphenylmethylene as derived from ENDOR experiments.[21]

Following the work on the ESR spectroscopy of triplet states Gerhard Closs studied triplet carbenes by optical spectroscopy,[22] including the pioneering time-resolved laser spectroscopy study of diphenylmethylene.[25] Two other groups probed optical spectroscopy of diphenylmethylene independently,[23,24] and only one study had dealt with the application of time-resolved laser spectroscopy (TRLS) to carbene chemistry[26] when Closs and Rabinow's study of diphenylmethylene addition to alkenes[25] opened the field to studies in other laboratories. Although the limited (μs) time resolution of these early studies appears almost primitive compared to today's sophisticated TRLS experiments, the available time resolution was exactly right for the somewhat "sluggish" diphenyl-methylene.

Related studies involved cyclopropenes and bicyclobutanes, newly accessible with his new carbenes, notably the isomerization of 2,4-dimethylbicyclo[1.1.0]butane to butadiene. The conservation of orbital symmetry, a concept developed by Woodward and Hoffmann in the early 1960s,[27] predicts that this reaction will proceed as a concerted [$_{\sigma}2_{s}+_{\sigma}2_{a}$] process with predictable stereochemistry for the migrating carbon centers. Closs and Pfeffer probed the rearrangement of two 2,4-dimethylbicyclobutanes to two hexadienes and elucidated the steric course of this reaction.[28]

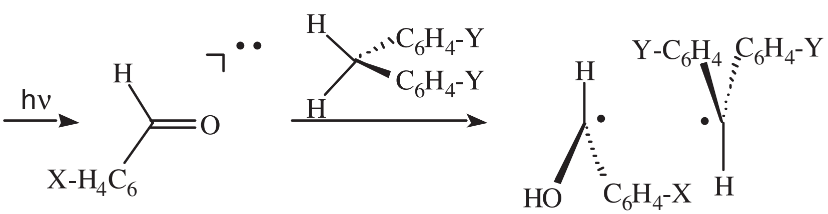


By the mid-1960s Gerhard Closs, an acknowledged expert in carbene chemistry, made his final major contribution to this field, the first application of the chemically induced dynamic nuclear polarization method. In 1967 enhanced nuclear magnetic resonance (NMR) emission was observed in some chemical reactions,[29,30] yet another facet in the rich palette of NMR applications. Because some reactions giving rise to NMR emission were known free-radical reactions, these effects were explained as electron-nuclear cross relaxation, hence the designation "chemically induced dynamic nuclear polarization" (CIDNP) for the new phenomenon. However, this mechanism was soon found wanting, as an increasing number of effects were incompatible with the cross relaxation mechanism.

Gerhard Closs immediately recognized the value of this technique. With his thorough understanding of organic reaction mechanisms and his expertise in the physical principles underlying magnetic resonance, he was in a unique position to elucidate the physical and chemical principles underlying CIDNP. He entered the field with all his vigor. He persuaded Lieselotte to return to the bench for a few well-designed experiments.[31,32] In 1969 four back-to-back communications appeared in the *Journal of the American Chemical Society*, followed quickly by six more, for a total of ten communications in only 20 months. After two CIDNP studies in photoreactions of diphenyldiazomethane[31] and benzophenone[32] Closs began to probe the actual origin of the spin polarization effects.

Recognizing that all CIDNP effects required the involvement of radical pairs,[33] he developed a theory that could explain the observed polarization and designed elegant experiments to probe key features of the theory. Because the polarization changed with the spin multiplicity (μ) of the precursor from which the pair was generated, he suggested CIDNP "as a tool for determination of spin multiplicities of radical pair precursors."[34]

He also recognized that the polarization pattern of CIDNP effects depends critically on the relative *g* factors *(Δg)* of the paired radicals and illustrated the effect of systematical changes in the g factor difference (Δg) between the interacting radicals on the CIDNP spectra (see Figure 2).[35]



In this context I will share an anecdote about the 1970 American Chemical Society meeting in Houston, Texas, where

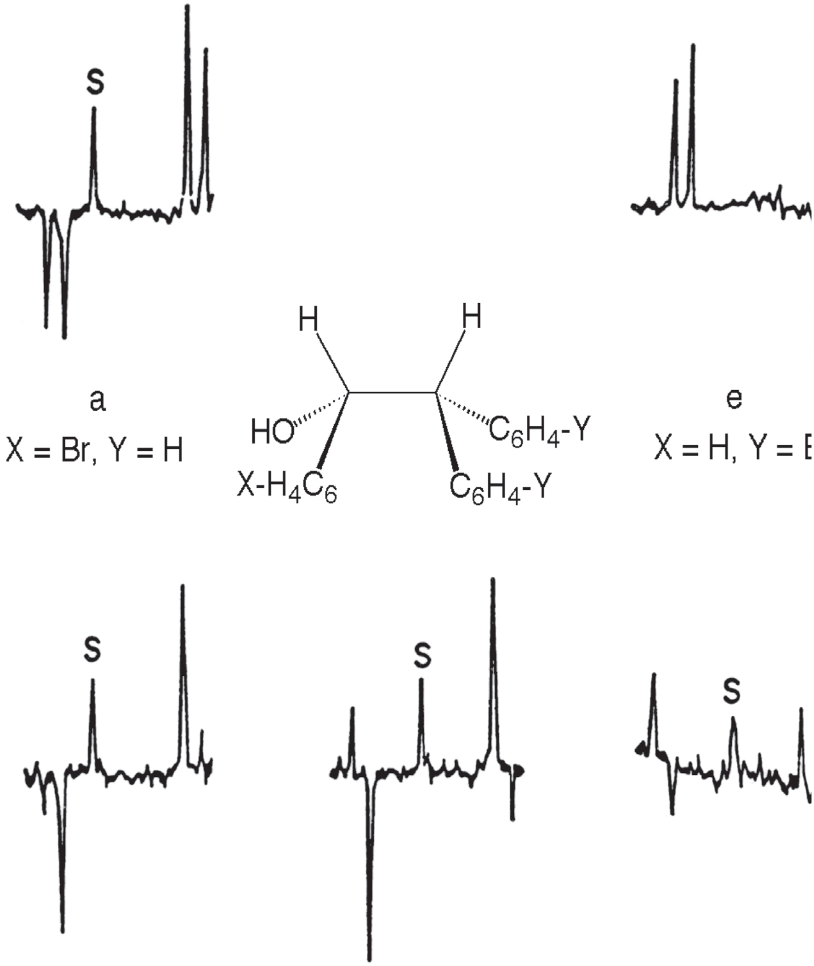


FIGURE 2 CIDNP spectra (benzylic protons) of coupling products generated during the photoreactions of benzaldehyde (X = H) and derivatives (X = Cl, Br) with diarylmethanes (Y = H, Cl, Br).[33]

Gerhard first disclosed major features of the proposed theory. The ACS Organic Division sponsored a symposium about the new phenomenon; most of the early contributors were present, except the Japanese and Russians. Gerhard was scheduled to give the opening lecture of the afternoon session. After brief introductory remarks he asked for the first slide, studied it briefly, asked quickly for the second and third slides, then turning to the session chairman, said, "Mr. Chairman, I know this is highly irregular; I thought this could only happen to Michael Dewar [Professor Michael Dewar, for four years, 1959-63, Closs's colleague at the University of Chicago; University of Texas, Austin 1963–]. I took the wrong set of slides." The next speaker was asked to present his talk out of turn, and Gerhard retrieved the correct set of slides from his hotel, which fortunately was just across the street. When he returned, he showed a series of astonishing slides that were well worth the (brief) wait (as well as the violation of the ACS guidelines for the presentation of papers).

Coincidental with the Chicago group, L. Oosterhoff and R. Kaptein at Universiteit Leiden (Netherlands) worked on the mechanism of CIDNP and demonstrated additional elements of the theory. They noted that in-cage products and cage-escape (free-radical) products showed polarization of opposite sign,[36] and that protons with hyperfine coupling constants of opposite sign show CIDNP effects of opposite sign.[36] The Radical Pair Theory emerging from the work of the Chicago and Leiden groups is now generally accepted and can explain the vast majority of all nuclear spin polarization effects.

In more than 20 additional publications Closs and collaborators elucidated additional facets of the theory of CIDNP or dealt with applications to new problems. They made major contributions to understanding biradicals: The magnetic field dependence of CIDNP effects of biradicals yields their average singlet-triplet splitting,[37] as well as their lifetimes (see Figure 3).[38] Closs and colleagues also established relaxation[39] and cross relaxation phenomena.[40] Cross relaxation transfers nuclear spin polarization to nuclei, which are not coupled to an unpaired electron spin. The existence of cross relaxation complicates the interpretation of CIDNP results, particularly in experiments designed to derive electronic structures of free radicals or radical ions from their polarization pattern.

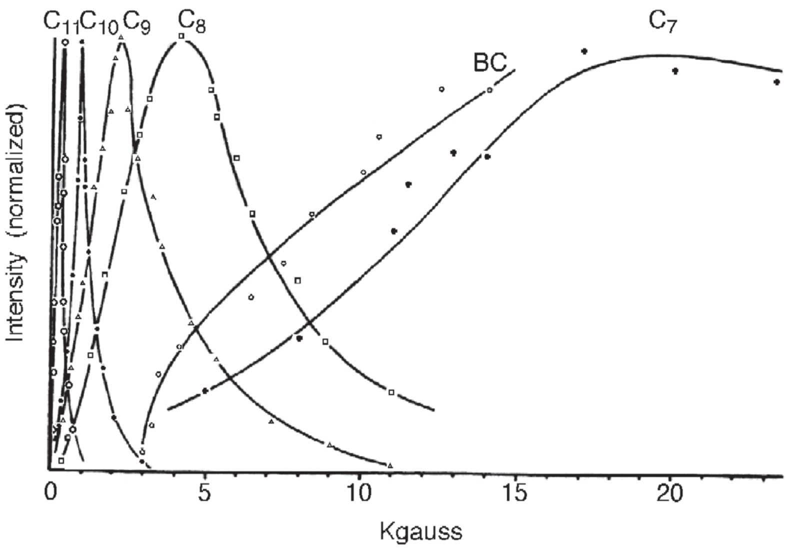
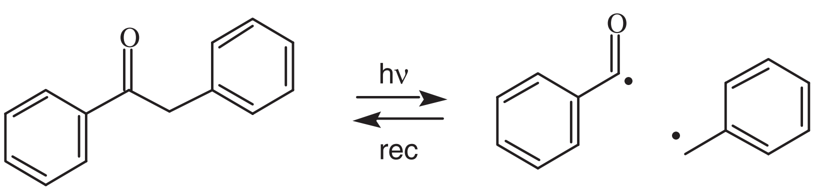


FIGURE 3 Normalized intensity versus magnetic field of aldehyde proton signals obtained by photolysis of cycloalkanones and a bicyclic ketone (BC). All signals are in emission.[37,38]

Closs also pioneered flash photolysis with CIDNP detection, combining the advantages of TRLS with the structural information content unique to CIDNP.[41-43] "The basis for the success of the time-resolved method is the fact that geminate processes are complete in a fraction of a microsecond, while combination of free ions and/or exchange . . . may take tens or hundreds of microseconds depending on concentration."[42] This method was soon adopted in other laboratories for various applications. The Closs group applied this technique to determine the spin density distribution in radical ions of chlorophyll and derivatives. The CIDNP intensities 1 μs after a photoinduced electron transfer reaction revealed the signs and relative magnitudes of hyperfine coupling constants[43] in good agreement with ENDOR spectroscopy results. Another application probed the kinetics of triplet states and biradicals; photolysis of deoxybenzoin cleaves a C–C bond next to the carbonyl group. The resulting radical pair can regenerate the reactant by geminate recombination; free radicals escaping from the geminate cage may have secondary encounters, forming deoxybenzoin or bibenzyl. At the shortest delay then attainable (1 μs) the reactant showed the expected geminate polarization; as expected, when the delay time was increased to 100 μs, this signal grew, due to contributions from free radical combination (see Figure 4).[41]



Without any doubt Gerhard Closs's contributions have made the CIDNP method a well-defined, sophisticated, powerful, and reliable technique with a wide spectrum of important applications. When he first came to Chicago, supervision of the departmental NMR instrument was one of the tasks assigned to him. Surely Gerhard Closs took this responsibility seriously and made the most of the opportunities this position offered.

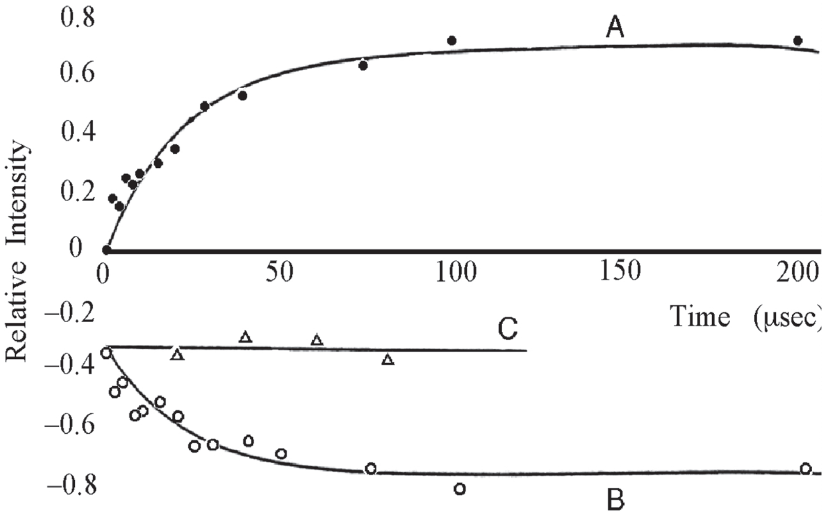


FIGURE 4 Normalized line intensities of bibenzyl (A) and deoxybenzoin (B), observed during the photolysis of deoxybenzoin, as a function of delay time T. The deoxybenzoin polarization in the presence of a (thiol) free radical scavenger is designated C.[41]

In the final decade of his life Gerhard Closs became interested in several aspects of chemically induced dynamic electron polarization (CIDEP), a phenomenon related to CIDNP and discovered several years earlier.[44] Essentially all CIDEP effects observed in the 20 years following the discovery could be explained by the radical pair or the triplet mechanism or by a combination of both. In the mid-1980s several time-resolved CIDEP spectra with highly unusual features were observed upon photolysis of ketones in micelles,[45] which could not be explained by the existing theories. As a natural extension of his earlier work on biradicals and in the context of his interest in the superexchange mechanism of intramolecular electron transfer (see below), Gerhard Closs investigated related systems in collaboration with M. Forbes and J. Norris. They recognized the unusual effects as manifestations of electron spin-spin interactions; the resulting effects are rapidly lost in solution but they "remain observable because of limited diffusion in micelles."[46] McLauchlan and coworkers derived a similar explanation independently.[47] The concept of spin-correlated radical pairs is now generally accepted.

Closs's time-resolved electron spin resonance studies culminated in several elegant studies of electron spin polarized polymethylene biradicals in solution, in which through-bond interactions were illuminated.[48] Simulating the shape and time dependence of CIDEP spectra from acyl-alkyl and alkyl-alkyl polymethylene biradicals by perturbation theory yielded the electron spin-spin interaction, J, and the end-to-end contact rate, which is inversely related to the lifetime. It is remarkable that Gerhard Closs was still breaking new ground at the onset of his seventh decade.

In 1979 Gerhard Closs accepted the position of section head in the Chemistry Division at Argonne National Laboratory. He kept this position for only three years, but this appointment significantly influenced the direction of his research in the final decade of his life. He took an interest in the electron transfer work of John Miller, who had evidence for the "inverted Marcus region." More than three decades earlier Marcus formulated the rate of an electron transfer reaction as a function of two parameters, its driving force (i.e., the free energy), ΔG^0, of the reaction and a "solvent reorganization energy," λ_s, required to accommodate the changing charge distribution.[49]

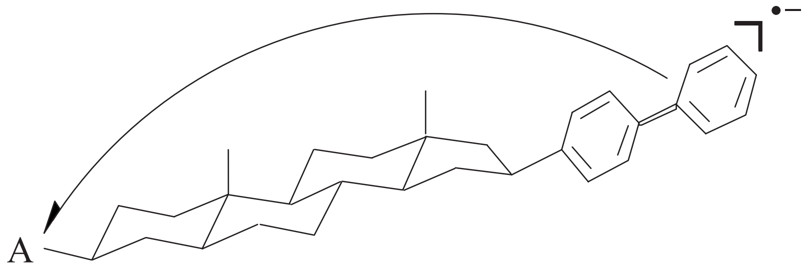
$$k_{ET} = A' \exp-[(\Delta G^0 + \lambda_s)^2 (4\lambda_s k_B T)^{-1}]$$

The most striking result emerging from this work was the prediction that electron transfer rates increase with increasing driving force to a maximum at $\lambda_s = \Delta G^0$, but then unexpectedly (and perhaps counterintuitively) increasing the driving force further would decrease the reaction rate. The essential predictions of this theory were reproduced by numerous theoretical approaches descending from the original idea of Marcus. It took almost 30 years before this prediction was confirmed by experiment.

Electron transfer reactions fall into three classes: charge separation, the photoinduced generation of radical ion pairs from a donor and an acceptor molecule; charge recombination, the reverse process; and electron exchange between a charged and a neutral entity. Rehm and Weller studied the charge separation in more than 60 organic donor-acceptor pairs. The rate constants of fluorescence quenching indicated a maximum rate of electron transfer, essentially diffusion limited, without evidence for an inverted region. Although at variance with the existing theories, the results were rationalized based on an ad hoc theory.[50] Similar results were observed in additional systems; all attempts to verify the predicted inverted free energy dependence of electron transfer rates met with failure.

Miller and coworkers had studied the (charge neutral) electron transfer from radiolytically generated radical anions to aromatic hydrocarbons in frozen solutions with free energy changes ranging from $0.01 < -\Delta G^0 < 2.75$ eV. The rates of electron transfer decreased at high exothermicities,[51] the first report of a reduction of electron transfer rates with increasing driving force. However, because the entities were randomly distributed in rigid glasses, the results were difficult to interpret and to understand.

In the collaboration that ensued and continued to Gerhard Closs's death donor and acceptor were linked by a rigid steroid spacer in molecules of the type A-Sp-biphenyl. The electron transfer rates observed for the radiolytically generated monoanions of these systems showed a striking deviation from the classical Brønsted relationship (see Figure 5),[52] confirming the predictions of Marcus. Less than five months after Gerhard Closs's death, R. A. Marcus was awarded the 1992 Nobel Prize in chemistry. In the announcement the Swedish Academy disclosed that the Nobel committee had long recognized the significance of Marcus's theory but had waited for experimental verification before awarding the prize.



These findings stimulated major research efforts around the world. Within a year the Marcus inverted region for charge recombination was reported and elaborated; a charge separation reaction was also found to exhibit the full range of Marcus behavior. The Chicago-Argonne team continued to produce a host of significant results. They established the nonlinear dependence of electron transfer rates upon solvent polarity, probed the distance dependence using decalin and cyclohexane spacers, and established the temperature independence of electron transfer rates in several systems. Having studied both "hole" and electron transfer rates, Closs and Miller reasoned that triplet energy transfer might be considered as the sum of the two processes. This consideration spawned an illuminating, unprecedented comparison of the rates of hole, electron, and triplet energy transfer.[53] Further collaboration was cut short by his untimely death.

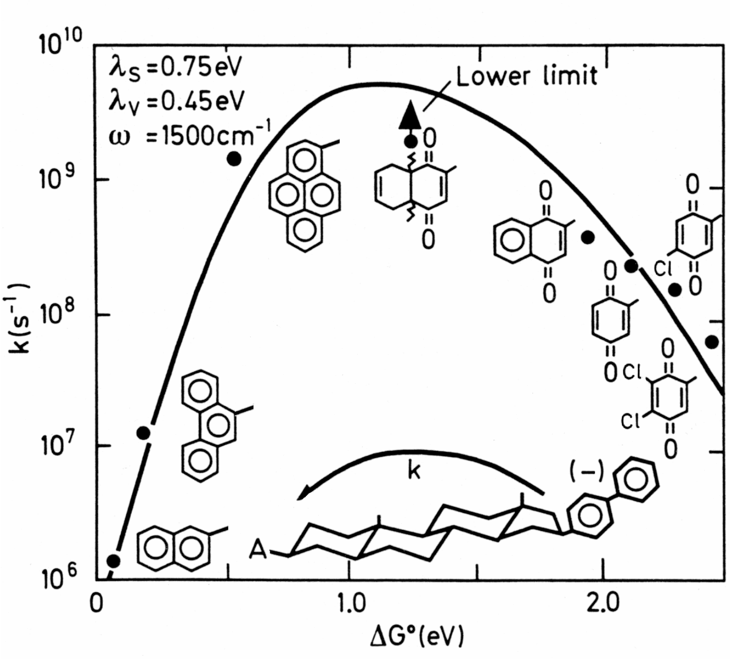


FIGURE 5 Intramolecular rate constants as a function of free energy change in 2-methyloxacyclopentane solution at 296 K. The electron transfer occurs from biphenyl anions to the eight different acceptor moieties (shown adjacent to the data points), in eight bifunctional molecules of the general structure shown in the center.[52]

One significant aspect of the electron transfer work suggested that donor and acceptor are coupled by the interactions with the orbitals of the intervening molecular fragments, a mechanism referred to as superexchange. This was one of the reasons that caused Gerhard Closs to reconsider the interaction of two unpaired electron spins in biradicals through the intervening molecular fragment, leading to the concept of spin-correlated radical pairs (see above).

Gerhard Closs made significant contributions in three fields of chemistry. He was an early leader in the field of carbene chemistry, he pioneered applications of magnetic resonance to characterize reaction intermediates, and he elucidated intricate facets of electron transfer chemistry. He will be remembered for the depth and breadth of his understanding and for his rigorous, all-encompassing approach to research. Once he became interested in a problem he would first master the theoretical background, or develop it himself, as was the case with CIDNP. Then he would identify key features that needed to be verified and design ingenious and decisive experiments to probe the theory. Finally, he would synthesize the selected target molecules and conduct the physical experiments.

IN WRITING THIS MEMOIR I have benefited from conversations with several of Gerhard's students and colleagues, particularly in the difficult task of choosing the "Selected Bibliography" from his many superb publications: Robert A. Moss, who was a student in the carbene years (cf., 1964); John Miller, who inspired Gerhard's interest in electron transfer and collaborated with him for his final 10 years (cf., 1983, 1984, 1986, 1989, 1990); Jim Norris, an associate and friend at the University of Chicago, who made available a tribute to Gerhard, written for the *University of Chicago Chronicle*;[54] and Piotr Piotrowiak, one of his last students, who bridged the electron transfer and biradical projects (cf., 1989, 1992).

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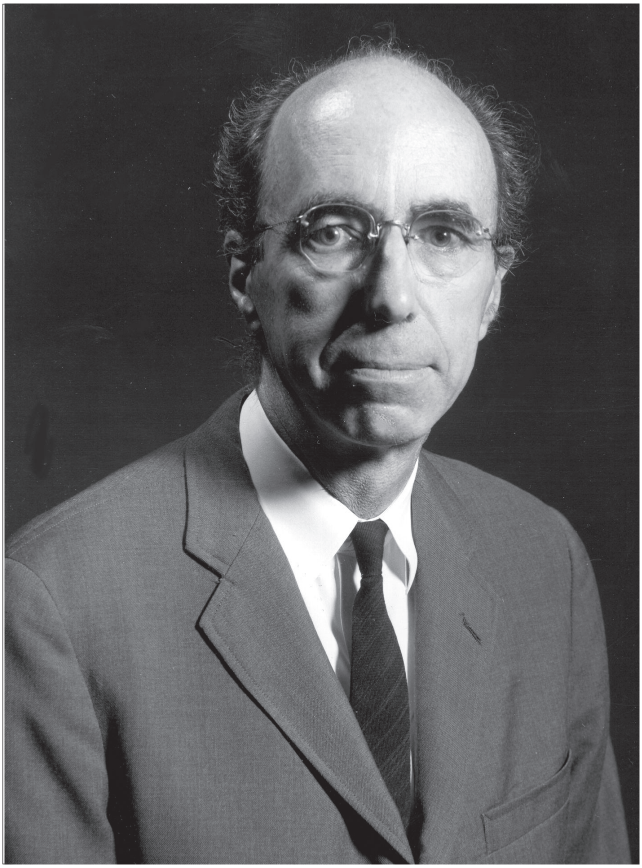

Sidney Darlington

SIDNEY DARLINGTON

July 13, 1906–October 31, 1997

BY IRWIN W. SANDBERG AND ERNEST S. KUH

SIDNEY DARLINGTON, ONE of world's most creative and influential circuit theorists, died at his home in Exeter, New Hampshire, on October 31, 1997, at the age of 91. He was a man of uncommon depth and breath whose first love was circuit theory. He made important, widely known contributions in several areas, including network synthesis, radar systems, rocket guidance, and transistor networks.

Sid was born in Pittsburgh, Pennsylvania. According to Sid, both his parents came from families in southeastern Pennsylvania. His maternal grandfather moved his family from the East to a quarter section of virgin prairie in western Minnesota when his mother was about 12 years old. She saw covered wagons taking settlers to what was then the Dakota Territory. At age 17 she taught in a one-room prairie school, but eventually was able to return to the East and attended Bryn Mawr College. She was determined that her children should have the best possible education. Sid's father was a mechanical engineer. Sid's brother, P. Jackson Darlington, Jr., became a biologist, and was elected to membership in the National Academy of Sciences in 1964. Both Sid and his brother attended Phillips Exeter Academy. There Sid won both of the school's math prizes, as well as the physics prize.

Sid received a B.S. in physics (magna cum laude) from Harvard College in 1928, a B.S. in electrical communication from the Massachusetts Institute of Technology in 1929, and a Ph.D. in physics from Columbia University in 1940. He was strongly influenced by George Washington Pierce at Harvard and by Ernst A. Guillemin at MIT. According to Sid, it was Guillemin who inspired his interest in circuit theory. At Columbia Sid took courses he described as "inspiring" with Isadore Rabi, and sometimes wondered if he might have been even happier as a theoretical physicist.

In 1929 Sid became a member of technical staff at Bell Laboratories, and in 1934 he was transferred to the laboratories' Mathematics Research Center, where his first supervisor was Hendrik W. Bode. Sid remained at Bell Laboratories until he retired as head of the Circuits and Control Department at the then mandatory retirement age of 65. One of us (I.W.S.) was a member of Sid's department for about seven years, starting in 1960. Sid was a member of both the National Academy of Engineering and the National Academy of Sciences. In 1945 he was awarded the Presidential Medal of Freedom, the United States' highest civilian honor for his contributions during World War II. President Truman established the award in that year to reward notable service during the war. He received the Edison Medal of the Institute of Electrical and Electronic Engineers (IEEE) in 1975 and the IEEE Medal of Honor in 1981.

In Darlington's early days at Bell Laboratories there was much interest in electrical filter theory, mainly in connection with the exacting needs of systems using frequency-division multiplexing. Filter theory was very different then from what it is today in that it was marked by ad hoc techniques in which complex filters were designed by cascading less complex filter sections whose attenuation characteristics were specified in graphical form. This was often unsatisfactory for several reasons. For example, the theory available did not adequately take into account the loading of the various sections on their predecessors. Sid's brilliant contribution was to recast the filter design problem as two problems: approximation and network synthesis, and to give a solution to each problem. The approximation problem he addressed is to suitably approximate the desired typically idealized filter characteristic using a real rational function of a complex variable, and here Darlington made significant pioneering contributions involving the use of Tchebyscheff polynomials. His main contribution, which concerned the exact synthesis of a two-port network that realized (i.e., implemented) the rational function, was the introduction of his well-known insertion-loss synthesis method. This work by Darlington led to his beautiful structural result that no more than one resistor is needed to synthesize any RLC impedance.

It is interesting that his results were not widely used until many years after they were obtained. This occurred partially because more exacting computations were required than for the earlier "image-parameter" filter designs. Also, due to its novelty, it was not easy for filter designers at the time to fully appreciate Darlington's contributions. This is easier to understand in the context of the history of the development of lumped-constant filter theory, which originally was an extension of the theory of transmission lines, and in which originally the concepts of a propagation constant, characteristic impedance, reflection factor, etc., played a prominent role.

Sid's work also profoundly influenced electrical engineering education. After World War II the Darlington synthesis of reactance two-ports was taught to a generation of graduate students who learned that linear circuit design could be formulated precisely in terms of specifications and tolerances, and that the problems formulated could be solved systematically. With concurrent advances in communication and control theory, electrical engineers began to appreciate that higher mathematics was a powerful tool for advanced study and research. This helped pave the way for the introduction of system theory and system analysis, and thus further broadened the scope of electrical engineering education.

During World War II, Sid was heavily involved in several studies of military systems. These studies concerned mainly the development of computers for antiaircraft gun control and bombsights. For a seven-month period beginning in 1944 he took a leave of absence to join the U.S. Office of Field Service. He was assigned to the 14th Antiaircraft Command in the southwest Pacific area, where he served as a consultant and technical observer. It was this work that led to his receipt of the Medal of Freedom.

In addition to never losing interest in circuit theory, Sid retained an interest in military systems—and related systems—throughout his tenure at Bell Laboratories. One of his most important contributions was the invention of what is called "chirp radar." The chirp idea is a way to form a pulsed radar's transmitted signal so that relatively high peak power is not needed to achieve long-range and high resolution. This involves transmitting long frequency-modulated pulses. The corresponding reflected and received (chirped) pulses are "collapsed" into relatively short pulses using a network that introduces a time delay that is frequency dependent. The idea has been widely used, and there has been much interest in the design of the needed delay networks—not only at Bell Laboratories, but also at many other companies and at universities. Darlington's IEEE Medal of Honor citation reads: "For fundamental contributions to filtering and signal processing leading to chirp radar."

Sid also did very influential work concerning rocket guidance. In 1954 he ingeniously combined radar-tracking techniques with principles of inertial guidance to develop the highly effective Bell Laboratories Command Guidance System that has launched many of the U.S. space vehicles, including NASA's Thor Delta booster and the Air Force's Titan I missile. The system has proved to be remarkably reliable and has played a central role in placing into orbit many satellites, including the Echo I communications satellite, Syncom, and Intelsat.

Darlington is best known for an idea that he probably developed very quickly: the Darlington transistor, a simple circuit comprised of two or more transistors that behave as a much improved single transistor. As is well known to the circuits and systems community, this idea is widely used and has had a great impact on the design of integrated circuits.

Sid was a visiting professor for periods of time of from one to six weeks at the University of California, Berkeley, from 1960 to 1972, and in 1978 he was a visiting professor at the University of California, Los Angeles, for a month. He gave many lectures and very much enjoyed these visits. Colleagues and students often remarked among themselves about how impressed they were with his keen physical insights, sophisticated mathematical talent, and pursuit of definitive results. After Sid retired from Bell Laboratories he became an adjunct professor at the University of New Hampshire, where he received an honorary doctorate in 1982. He was a consultant to Bell Laboratories from 1971 to 1974. Darlington held more than 40 patents, and was active in professional society activities. During 1959-60 he was the chairman of the IEEE Professional Group on Circuit Theory, and in 1986 he received the Circuits and Systems Society's first Society Award.

Sid was married twice. He had no children with his first wife. He married for the second time in 1965; the marriage produced two daughters. Sid seemed proud that he was over 60 years old when they were born. He said that he "would like to live at least until age 86, so as to see both girls graduate from college." Happily he did. Sid was a man of great personal and professional integrity. He was an intense but gentle man who was surprisingly modest. He was also a gregarious person who knew a lot about many things and had much to say. A colleague once commented that "asking Sid Darlington a question was like trying to take a drink from a fire hose."

In his personal life, Sid enjoyed hiking, snowshoeing, and mountain climbing. He is survived by his wife, Joan, of Exeter, New Hampshire; two daughters, Ellen and Rebecca; and his sister, Celia.

CAREER CHRONOLOGY

1929-71	Bell Telephone Laboratories, Inc., member of the technical staff, department head at the time of retirement.
1944-45	On leave from Bell Telephone Laboratories for seven months, employed by the Office of Field Services and assigned to the 14th Antiaircraft Command in the southwest Pacific area.
1960-72	Visiting lecturer at the University of California, Berkeley, for periods of one to six weeks.
1971	Retired from Bell Telephone Laboratories at the mandatory retirement age.
1971-74	Part-time consultant to Bell Telephone Laboratories.
1971-97	Adjunct professor at the University of New Hampshire and occasional consultant.
1978	Visiting lecturer at the University of California, Los Angeles, for one month.
1980-97	Member of the newly established New Hampshire State Legislative Academy of Science and Technology.

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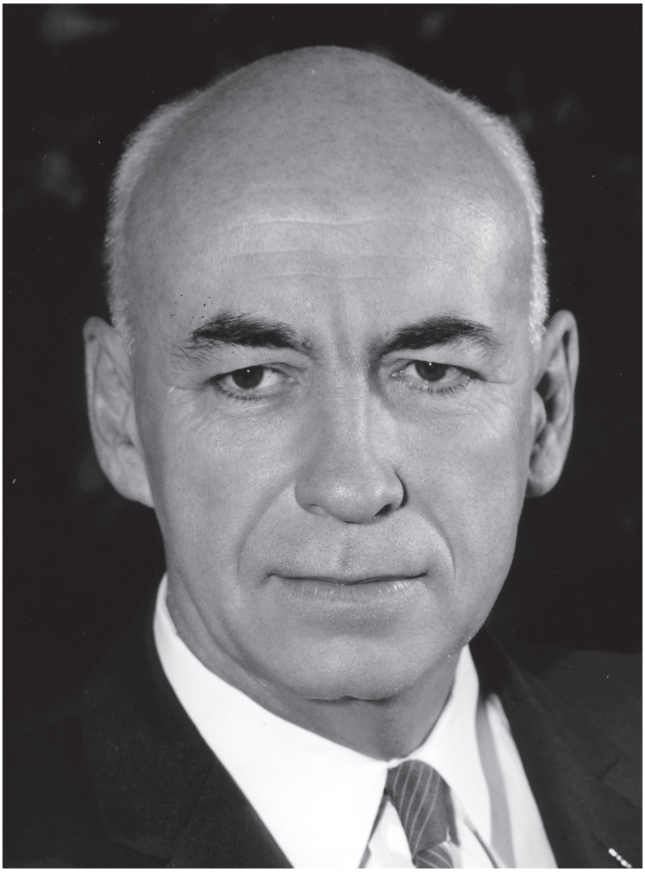

ROBERT R. GILRUTH

October 18, 1913–August 17, 2000

BY CHRISTOPHER C. KRAFT, JR.

ROBERT R. GILRUTH, a father of human space flight, never sought public attention, and his leadership and technical contributions were often overlooked. Of the many heroes in the early days of the U.S. space program Gilruth was among the most respected. He led the United States in the Mercury, Gemini, and Apollo efforts and directed the greatest engineering achievement in history: the safe voyages of humans to the Moon.

I worked for Bob as Director of flight operations and succeeded him as Director of the Johnson Space Center. He was one of the greatest men I have ever known. He launched his career at the Langley Memorial Aeronautical Laboratory in Hampton, Virginia, concentrating on the handling qualities of airplanes. In 1945 he organized and directed free-flight experiments with rocket-powered models at Wallops Island, investigating flight dynamics at transonic and supersonic speeds. By 1952 Gilruth was Assistant Director of the Langley laboratory, responsible for research into hypersonic aerodynamics, high-temperature structures, and dynamic loads. In 1958 he became Director of the Space Task Group and then managed the design, development, and flight operations of Project Mercury, which put Americans into space.

In 1961, after President Kennedy committed the nation to land a human on the Moon, Gilruth became Director of NASA's Manned Spacecraft Center in Houston, Texas. He actively directed and oversaw the design and construction of spacecraft, the selection and training of astronauts, and the planning and operation of space flights. In 1973 Bob Gilruth retired from NASA. In later life he suffered from Alzheimer's disease. He died on August 17, 2000, at the age of 86.

Robert Rowe Gilruth was born October 18, 1913, in Nashwauk, Minnesota. He graduated from high school in Duluth, Minnesota, after attending public schools in several communities in that region. He studied aeronautical engineering at the University of Minnesota, where he received his bachelor's degree, and then a master's degree in 1936. Bob Gilruth's first engineering experiences came from watching his grandfather carve little boats to sail on the Minnesota lakes. Gilruth's parents were both teachers. His mother had an inclination toward math, while his father was "a born teacher, but not an engineer," who loved to read the classics to Gilruth and his older sister. Gilruth did not want to follow in his parents' footsteps as an educator. "I was going to build something," he remembered later. "I wasn't sure what."[1] Aeronautical engineering grabbed his imagination, although he would continue to invent and build boats for the rest of his life.

When Gilruth was about 11, his father lost his job, and the family moved to Duluth to find work. There the young Gilruth designed rubber-band-powered airplanes, inventing a feathering propeller to reduce drag during glide. Modestly, Gilruth later asserted that he "wasn't a very good student,[2] and said his parents did not see much future in aviation, but the young Gilruth scoured magazines for articles about airplanes. He read *American Boy* and *Popular Mechanics*, as many boys did. When Bob learned about the National Advisory Committee for Aeronautics (NACA) from the pages of *The Saturday Evening Post*, he sent away for NACA reports on airfoils. He used the information to improve his rubber-band gliders and successfully competed in local model-airplane contests.

To save his family money Gilruth attended junior college before entering the University of Minnesota to study aeronautical engineering. He studied structure and loading, and basically "how to design an airplane,"[3] although he felt the department at that time was better at "teaching you . . . the routine things you did in an airplane company."[4]

When Gilruth completed his undergraduate degree in 1935, the United States was so deep in the Great Depression that none of the 17 graduates received job offers in aviation. Some of Bob's classmates joined the Naval Air Corps, but Gilruth never considered seriously becoming a pilot. "I didn't think that I had time to learn to fly," he said. "And I didn't really think that it would do that much for me to be a pilot.[5] I wanted to go to NACA. That's what I wanted to do."[6]

The University of Minnesota gave Gilruth a graduate research fellowship. Earning 50 dollars a month, he worked toward his master's degree on several projects. For example, he reluctantly helped on a department chief's project to build a hot-air military "barrage balloon" that depended on a ground-based generator transmitting electricity up a tether to heat the balloon's air. The project failed, and an embarrassing demonstration for the press provided Gilruth some useful early experience.

About this time the famed French balloonist Jean Piccard joined Minnesota's faculty. Piccard, a pioneer in upper atmosphere research, asked Gilruth to develop a valve to keep constant air pressure inside an airplane's cockpit. According to Gilruth, Piccard was very interested in helping airplanes to fly high: "He said they'd be out of the thunderstorm belt, the air would be thinner, and you'd be able to go faster."[7] And he was right. This experience primed Gilruth for his future studies of high flight.

Piccard's mentoring helped Gilruth in other ways, too. For example, Gilruth said Piccard "used blasting caps on everything. It was great for me because it wasn't too long before I was using igniters on all kinds of spacecraft,"[8] beginning with rockets at NACA's Wallops Island site. Gilruth learned much from Piccard, especially his "ways of looking at problems . . . of simplifying things." Piccard's ideas about high-altitude balloon gondolas would help Gilruth later when the Mercury capsule was being designed.

During his fellowship at Minnesota, Bob Gilruth met and married Jean Barnhill, a fellow engineering student and an aviatrix who flew in cross-country races. The new Mrs. Gilruth likewise worked with Piccard and she helped construct an unmanned balloon that sent back telemetry on cosmic radiation. Piccard himself was married to an American, Jeannette Piccard, an engineer and a balloonist who Gilruth considered to be "at least half the brains of the family."[9] Later, during Mercury, Gilruth would hire Mrs. Piccard to serve as a consultant.

Also at this time, for a wage of 40 cents an hour, Gilruth helped to design the *Laird Watt*, a racing plane flown by the famed pilot Roscoe Turner. According to Gilruth, "I was trying to design an airplane that was going to win the Thompson Trophy Race. . . . I made good use of that experience when I went to work for NACA. . . . It was equivalent to a couple of years' experience, even though it was done while I was at school."[10]

In his graduate project at the University of Minnesota Gilruth investigated the possibilities of placing an airplane's propellers at the ends of its wings to take advantage of the tip vortices that are naturally produced there. However, the added effects were not large enough to follow up on his findings. In December 1936 just before he received his graduate degree Gilruth was offered a job as a junior engineer at NACA.

Gilruth regarded NACA to be a better place to learn than graduate school and found Langley to be "an absolutely fantastic place to work."[11] When he left Minnesota, the temperature was 20 degrees below zero with 2 feet of snow on the ground. He arrived by train in Hampton, Virginia, where it was about 45 degrees and overcast. The grass was green and the magnolia leaves were on the trees. "I got out in that air, and . . . my goodness!" He looked around and said, "Gee, this is really neat."[12] Jean subsequently joined Bob in Hampton, where they set up housekeeping in a small apartment. This is where they would design and build their first boat, and later design their home and await the birth of their daughter, Barbara.

When Gilruth joined NACA in early 1937, he felt the United States had not made much progress in aviation since World War I. Charles Lindbergh's 1927 solo flight across the Atlantic had been "a great shot in the arm for this country,"[13] much as Alan Shepard's *Mercury* flight would be in 1961. However, Gilruth said, "the Army flew the airmail for a while and they lost a lot of airplanes. . . . We had not made our mark in aviation."[14] But Langley had an engine research lab, advanced wind tunnels, and a towing basin for work on seaplanes. "Best of all, they had a staff of skilled people, dedicated in how you made the airplane better."[15]

Oddly, Gilruth was given no assignment at first. He was just assigned to a desk. As a new, young engineer he was "kind of worried, yet I hadn't done anything wrong."[16] So Gilruth started reading. He studied all of NACA's technical reports. One day another engineer, Hartley Soulé, noticed

Gilruth and said, "Here, you're not doing anything. How about working these up for me?"[17] He handed Gilruth films that he had taken during a recent research flight. Six months later Gilruth had replaced Soulé as the engineer who flew with the test pilots. The purpose of the project was to determine quantitative criteria for the flying and handling qualities of airplanes. When Soulé was soon promoted, Gilruth became the flying quality expert at Langley.

As a result of the project Gilruth wrote a report titled "Requirements for Satisfactory Flying Qualities of Airplanes,"[18] which abstained from pilot jargon and put numbers to the qualities that made an airplane's characteristics good or bad. For the first time Gilruth used his concept of "stick force per g," which compares the pilot's actions to the airplane's reactions. This report helped to make Gilruth's reputation. Later when World War II was raging, the British were so enamored with Gilruth's findings that they sent a team of people to consult with him in 1943.

During the war Bob Gilruth and many other aeronautical engineers were inducted into the military, put on enlisted reserve, and then sent back to their design work. When I joined Langley in 1945, Gilruth was trying to break the sound barrier. He had invented a technique he called "wing flow." This placed small models above the wings of flying airplanes and used the accelerated flow there to study mach conditions. "This was like making a wind tunnel along the top of a wing of an airplane,"[19] Gilruth explained. He was able to show that a thinner wing like that of a P-51 flew better around mach speed than a thicker wing like that of a P-47. The results were so important that they were promptly classified top secret, but they helped shape the wing of the Bell X-1, which would break the sound barrier in 1947.

At about this time Gilruth and others at Langley were also dropping streamlined bodies from high altitudes. They used telemetry to measure airfoil drag as the bodies went through the sound barrier. In 1945 Gilruth was placed in charge of developing a guided missile research station on Wallops Island. His team used Doppler radar to measure missile speeds and calculate the drag of airfoils and the behavior of ailerons as they passed through the speed of sound. Gilruth's organization became known as PARD, the Pilotless Aircraft Research Division. Others joining Gilruth at Langley after the war were people like Max Faget and Caldwell Johnson; both would be major contributors in the race to the Moon.

Promoted to assistant director of Langley in 1952, Gilruth worked on several of the ballistic missiles that were being developed then. He was deeply troubled by the advent of the Atomic Age of warfare. He said, "I felt that things had really gotten out of hand."[20] He was also aware of the discussion of orbiting an artificial satellite, but he wasn't much intrigued by that possibility. On the other hand, he said, "When you think about putting a man up there, that's a different thing. That's a lot more exciting. There are a lot of things you can do with men up in orbit."[21]

Like most Americans Robert Gilruth realized the world changed when, in 1957, the Soviet Union orbited first a *Sputnik* satellite and then a second satellite, which carried a dog named Laika. According to Gilruth, "When I saw the dog go up, I said, 'My God, we better get going because it's going to be a legitimate program to put man in space.' I didn't need somebody to hit me on the head and tell me that."[22]

After the dog flew in space Gilruth and his colleagues considered manned space flight. "We started scheming about what you could do."[23] To Gilruth "the problems of putting a man in space [and] the physical problems of the vehicle were pretty well solved before we ever really started the

Mercury program. . . . We could do it without exceeding the gravity forces" that a man could endure. "We had experiments with couches where a man could safely stand 20 g's . . . and that's a lot more than you need for re-entry."[24]

At Wallops Island Gilruth's teams had already studied the heat generated by high-velocity re-entry. The U.S. Air Force was considering using winged re-entry vehicles, but they would be too heavy. On August 1, 1958, Gilruth went before Congress to present a manned space program based on the blunt body shape, which Mercury would later use. Still, there were skeptics. Even Gilruth's supportive boss Hugh Dryden called the blunt body approach the same as "shooting a lady out of a cannon."[25] Using a blunt capsule seemed a stunt to many at the time, but the new idea of using a preceding shock wave was the best way for a spacecraft to re-enter the atmosphere.

Gilruth was made the leader of the Space Task Group: "I was pried out of Langley. . . . I was expected to put man in space and bring him back in good shape—and do it before the Soviets, which we didn't do."[26] I got to work with Bob as his assistant.

Congress created the National Aeronautics and Space Administration (NASA) on October 1, 1958, and incorporated all of NACA and its 8,000 employees. Before long NASA absorbed the space science group of the Naval Research Laboratory, the Jet Propulsion Laboratory, and the Army Ballistic Missile Agency in Huntsville, Alabama, where Wernher von Braun's engineers were already designing large rockets.[27]

At this point Gilruth's career took a turn not uncommon to first-rate engineers. He went from being leader of design and testing teams to being manager of a huge program. As his first task he hired the best engineers and managers he could find, even bringing in many from Canada. NACA's

Langley largely had been an in-house operation, but NASA would work differently. In some ways, said Gilruth, "All we were was a contracting agency," letting contracts to companies large and small.

Project Mercury commenced. Astronauts were selected and trained, capsules designed and constructed, rockets tested, and the Cape Canaveral launch site readied. But the Soviet Union beat the United States into space with the one-orbit flight of Yuri Gagarin on April 12, 1961. This event stirred the world and frightened the United States. To many the Soviets obviously led the Americans in important areas of technology. They were certainly ahead in propaganda. In Gilruth's words, "Poor President Kennedy was fit to be tied."[28]

When Alan Shepard flew his sub-orbital flight on May 5, 1961, Kennedy and the American public were delighted. But now Project Mercury wasn't enough. By itself it was just "a dead-end program,"[29] which had already ceded space primacy to the Soviets. Mercury needed to be part of a bigger competition that the United States could expect to win. As Gilruth tells it, "And that's where Kennedy came along and said, 'Look, I want to be first. How do we do something?'"

Gilruth advised the President, "Well, you've got to pick a job that's difficult—that's new—that they'll have to start from scratch. They just can't take their old rocket and put another gimmick on it and do something we can't do. It's got to be something that requires a great big rocket—like going to the moon. Going to the moon will take new rocket technology, and if you want to do that, I think our country could probably win because we'd both have to start from scratch."[30]

In Gilruth's later recollection, "Kennedy bought that. He was a young man. He didn't have all the wisdom he would have had. If he'd been older, he probably never would have done it."[31] Interestingly this decision was made in the same time frame as the failed U.S.-supported military operation at the Bay of Pigs in Cuba in April 1961.

And so on May 25, 1961, President Kennedy challenged the U.S. Congress: "[T]his nation should commit itself to achieving the goal, before this decade is out, of landing a man on the moon and returning him safely to the earth . . . [for] . . . one purpose which this nation will never overlook: the survival of the man who first makes this daring flight. But in a very real sense, it will not be one man going to the moon—if we make this judgment affirmatively, it will be an entire nation. For all of us must work to put him there."[32]

Even though Gilruth was "up to his neck"[33] in Kennedy's decision he was still stunned when he heard the speech. He was flying on a DC-3 with NASA Administrator James Webb. They heard it on the radio. Gilruth knew well what Kennedy was going to say, "but I still was aghast that he was saying it, and that we were going to try to do it."[34] The enormity of the challenge was overwhelming. Still, Gilruth was glad Kennedy had set a lunar landing date of "before this decade is out." Otherwise, with budgets and politics the Moon landing might never happen.

So the Apollo program was born: the most audacious engineering challenge in history. Bob Gilruth was to lead it from the new Manned Spacecraft Center to be located a few miles south of Houston, Texas. Not only did he have to manage Apollo but he also had to build a great space center in a place that was then no more than a salt-grass cow pasture. Of Houston Gilruth thought what many have thought: The climate is bad, the air conditioning is good, and the people are wonderful.[35] It was also near water, where Gilruth could build his boats.

In an amazingly brief period the Manned Spacecraft Center was constructed, the Gemini flights were flown, and the Apollo spacecraft were built, all as Gilruth coordinated these activities and other efforts with the other NASA installation directors, von Braun at Marshall in Huntsville, Alabama, and Kurt Debus at Cape Canaveral, Florida. But even with the pressures of deadline and the competition with the Soviets, Gilruth demanded that things be done right. He insisted on the inclusion of the Gemini flights that would develop technology and techniques for orbital rendezvous, docking, and space walks.

Then, on January 27, 1967, a fire during a ground simulation in an Apollo spacecraft test killed the Apollo 1 prime crew: Gus Grissom, Ed White, and Roger Chaffee. Bob Gilruth was in Washington, D.C., meeting with contractors. "I got a call from [the prime contractor] North American, saying 'We just lost our crew on the Cape.' I said, 'We lost them? Nobody's flying.' They said, 'But this was on the ground.'" Gilruth couldn't believe it. None of us could. We learned it was due to a lot of bad luck and some bad work.[36] As would happen with NASA's later space tragedies, Apollo 1 triggered rethinking and reworking.

It also brought about a recommitment to courage. In 1968 NASA decided to fly the Apollo 8 around-the-Moon mission much ahead of schedule. At that point, "James Webb retired because he felt that he could not face another potential tragedy after the Apollo fire of January 1967," Gilruth said. "I hated to see him leave but I understood how deeply he felt and all he had endured since the fire."[37]

The Apollo program was now beginning to move rather rapidly. With Apollo 8, for the first time in history humans had left their home planet. Everyone now realized a Moon landing was imminent. On July 20, 1969, much of the world watched as the Apollo 11 crew set footprints on the Moon.

Later flights introduced lunar rovers to the Moon's surface and with the final mission, Apollo 17, included a geologist, Harrison Schmidt.

But by now Apollo had lost much of its political support and the public's interest in space flight had waned. In fact NASA had to pay the television networks to broadcast Apollo 17. Interestingly this mission was the only Apollo launch that Gilruth watched in person. He preferred to be with us in Mission Control in Houston.

Ironically Bob's interest in Apollo was waning, too. I don't mean his interest in the challenge, but his interest in risking lives to repeat what had already been done. As he put it, "We'd already flown to the moon many times. I put up my back and said, 'We must stop. There are so many chances for us losing a crew. We just know that we're going to do that if we keep going.'"[38] Bob Gilruth regarded the astronauts almost as his own family. The decision to halt Apollo was made with his tacit approval.

Gilruth thought the U.S. space program should look in other directions. At one time before Apollo he was more interested in building a space station than in going to the Moon. He was also interested in opening up cooperation with the Soviets, and he made trips to Russia to prepare for what was to become the Apollo-Soyuz mission of July 1975.

Gilruth's wife, Jean, died in 1972 after the last Moon landing and during the period of his trips to Russia. He had left the Manned Spacecraft Center to work in Washington, D.C., as NASA's head of personnel development. He subsequently retired from NASA in 1973 and worked as a consultant for a short time thereafter; but soon he moved back to Houston with his new wife, Jo. Later that year they launched a 52-foot multi-hull sailboat, "*The Outrigger,*" designed and built by Gilruth in his spare time during the previous 10 years. Gilruth died in Virginia in 2000 after a long illness. In addition to his wife, Jo Gilruth, he is survived by his daughter, Barbara Jean Wyatt.

Robert Gilruth's achievements and life history are simple enough to trace, however his effects on people are deep and continuing. He was such an interesting personality, a beautiful man, a true leader, and a mentor. When I succeeded Bob as Director of the Johnson Space Center, I was fully ready. No one could have prepared me better. To give a better sense of his personality and influence I would like to share some quotes from some of Bob's peers and employees.

Aleck Bond: "After being interviewed by that man—he was such a likeable person and a wonderful human being—I said, 'This is the man I want to work for.'"

Caldwell Johnson: "He was indeed the boss! There was no question about who was the boss. I remember once. . . . I had grown a beard. Don't ask me why, but I had. . . . And we were in the cafeteria one day, and he said, 'Why don't you—Why don't you shave off that beard?' It was clear he didn't like it. And I said, 'Well, I would if I had a good reason to do it.' And he said, 'I'll give you a good reason. You go home and shave that thing off!' I went home and shaved it off."

Alan Shepard: "I obviously appreciate his decision to let me make the first flight, but he never told me why he made that decision the way he did. I asked him several times over the years, and he always said, 'Well, you were just the right man at the right time.'"

Charles Bingman: "He became the kind of Center manager that did not rely on his really tough-minded, hard-nosed management skills, but he was an almost inspirational leader. He was a very, very, very truthful, honest, straightforward man. . . . He kept saying, 'With me, what you see is what you get. I have no hidden agenda.' . . . That set the tone for the people in the Center."

Duane Catterson: "An inspirational leader, a gentleman, somebody who just didn't have a mean bone in his body, and yet commanded instant respect because he had such an all-encompassing knowledge of his field and what he was doing, and a great capacity to inspire confidence in people around him."

Kenneth Kleinknecht: "I never remember Bob Gilruth telling anybody what they should do or how to do anything. He just talked with them long enough that they thought his idea was their idea and they went and did it the way he wanted it."

Dorothy Lee: "I'm in my little corner. He stops at the door, and I can see him. The group is discussing something, trying to solve a problem, and he listens. Then he asks a question which turned their thinking around and headed them down the right path. And he turned around with a smile on his face and he walked out. I thought: there goes a big man. He didn't tell them how to do it; he just asked a question. Well, that became my modus operandi when I became a manager."

Such legacies attest to Robert Gilruth's leadership skills and his technical contributions to this country's human spaceflight achievements so poignantly symbolized by the American flag on the surface of the Moon.

SELECTED HONORS AND DISTINCTIONS

1950 Sylvanus Albert Reed Award from the Institute of Aerospace Sciences
1954 Outstanding Achievement Award from the University of Minnesota
1960 Governor of the National Rocket Club
1961 Fellow of the American Astronautical Society
1962 Honorary doctor of science from the University of Minnesota, Indiana Institute of Technology, and George Washington University
Louis W. Hill Space Transportation Award
NASA Distinguished Service Medal
Goddard Memorial Trophy of the National Rocket Club
Great Living American Award from the U.S. Chamber of Commerce

President's Award for Distinguished Federal Civilian Service
1963 Honorary doctor of engineering from the Michigan Technological University
1965 Spirit of St. Louis Medal by the American Society of Mechanical Engineers
1966 Honorary Member of the Aerospace Medical Association
Daniel and Florence Guggenheim International Astronautics Award of the International Academy of Astronautics
1967 1966 Space Flight Award by the American Astronautical Society
1969 NASA Distinguished Service Medals (after Apollo 8 and Apollo 11)
One of the first 10 persons installed in the National Space Hall of Fame
Rockefeller Public Service "At Large" Award
1970 Honorary doctor of laws from the New Mexico State University
ASME Medal from the American Society of Mechanical Engineers
1971 James Watt International Medal from the Institution of Mechanical Engineers
National Aviation Club Award for Achievement
1972 Robert R. Collier Trophy of the National Aeronautic Association and the National Aviation Club
1974 Member of the National Academy of Sciences
1975 One of the 35 space pioneers inducted into the International Space Hall of Fame

RECOGNITION FROM OTHER ORGANIZATIONS

Member, National Academy of Engineering
Honorary fellow, American Institute of Aeronautics and Astronautics
Honorary fellow, Royal Aeronautical Society
Member, International Academy of Astronautics

NOTES

The quotes from Bob Gilruth's colleagues are from the NASA Johnson Space Center Oral History Project. They, along with Gilruth's own interviews conducted for the National Air and Space Museum, have been the major resources in compiling this tribute.

1. First oral history interview of Dr. Robert Gilruth. Interviewers: David DeVorkin and Martin Collins, National Air and Space Museum, Washington, D.C., March 21, 1986, p 8.
2. First Gilruth oral history interview, p 17.
3. First Gilruth oral history interview, p 24.
4. First Gilruth oral history interview, p 24.
5. First Gilruth oral history interview, p 26.
6. First Gilruth oral history interview, p 27.
7. First Gilruth oral history interview, p 48.
8. Second oral history interview of Dr. Robert Gilruth. Interviewers: David DeVorkin, Linda Ezell, and Martin Collins, May 14, 1986, p 3.
9. Second Gilruth oral history interview, p 4.
10. Second Gilruth oral history interview, p 12.
11. Second Gilruth oral history interview, p 13.
12. Second Gilruth oral history interview, p 38.
13. Second Gilruth oral history interview, p 14.
14. Second Gilruth oral history interview, p 14.
15. Second Gilruth oral history interview, p 14.
16. Second Gilruth oral history interview, p 16.
17. Second Gilruth oral history interview, p 16.
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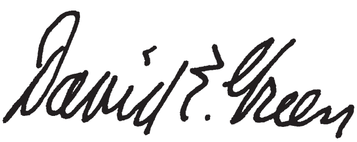

DAVID EZRA GREEN

August 5, 1910–July 8, 1983

BY HELMUT BEINERT, PAUL K. STUMPF,
AND SALIH J. WAKIL

At the time of David Green's death in 1983, Frank Huennekens, one of Green's postdoctoral fellows, wrote in his personal recollections:

> David Green was a remarkable person. Endowed with a keen intellect, an insatiable curiosity about Nature, a vivid imagination and boundless energy, he pursued a career devoted entirely to research. Over a period of four decades he and his colleagues published nearly 700 journal articles and reviews covering a broad spectrum of enzymology and bioenergetics. And, he was the author, co-author or editor of eight books. A legion of postdoctorals and visiting investigators received training in his laboratory. History will surely record that he was one of the giants of 20th-century biochemistry.

Green's professional career had four distinct periods, during which he explored, developed, and refined the expanding concepts of enzymology. They were his educational experiences at New York University and at Cambridge; his return to the United States to begin his American career for one year at Harvard; his first academic appointment at Columbia College of Physicians and Surgeons in New York City; and finally his selection as codirector of the Institute for Enzyme Research at the University of Wisconsin at Madison, where he remained until his untimely death in 1983. As we will see, Green played a pivotal role in the expanding frontier of enzymology, not only in the United States but also throughout the world.

David Ezra Green was born in Brooklyn, New York, on August 5, 1910. He attended the public school system there and apparently was indifferent to his studies in both his grade-school and high-school days. About his early education Green explained, "As I look back, school per se exerted little influence on me. My friends and my family were the principal catalysts in my development. There was not a single teacher in high school that fired or inspired me, though I respected them all as competent individuals. Curiously enough, courses in science did not particularly interest me. I hardly know why I avoided them in high school." Interestingly the *Book of Knowledge*, an encyclopedia popular during that period, became Green's bible. Its 20 volumes served as sources of information on subjects ranging from the arts to the sciences. His father loved learning, and it was from him that Green acquired an interest in books, ideas, and self-development.

In 1928 Green enrolled in New York University at the Washington Square campus and initially intended to study medicine. After taking the premedical-school curriculum for two years, however, he realized that the field of medicine did not interest him. Fortunately he was offered a student assistantship in the Department of Biology, and there he completed his undergraduate studies in 1931. A very important event was his summer experience at Woods Hole, where he associated with Professor Robert Chambers, the famed cell physiologist, and later with Professor Leonor Michaelis. Apparently the close association with Michaelis inspired Green and aroused his desire to explore more fully the mysteries of biological oxidations.

Green received a master's degree in 1932 at New York

University and left for Cambridge University in England, where his potential talents were nurtured in the fertile soil of the Biochemistry Department led by the famous Sir Frederick Gowland Hopkins. The department was home to some of the greatest biochemists of that period—including David Keilin, Malcolm Dixon, Robin Hill, Joseph and Dorothy Needham, Judah Quastel, Marjorie Stephenson, Ernest Gale, and Norman Pirie—and was ranked as one of the leading centers of innovative research in the new field of enzymology. Green soon ensconced himself among the department's many graduate and postgraduate students as the brash young American he was, a character that he did not lose even when made a Beit fellow.

Green conducted his graduate studies and research under the supervision of Malcolm Dixon, who said of Green's graduate work, "David threw himself into his research with great enthusiasm, energy, and enterprise. He was full of ideas, which he expressed freely; and although not everybody agreed with all of them, they were always interesting and characterized by freshness and vitality." In his initial year at Cambridge, Green completed all the research required for his Ph.D. thesis, "The Application of Oxidation-Reduction Potentials to Biological Systems." Although he received his Ph.D. degree on June 8, 1934, the results of his thesis research had been published in Biochemical Journal in 1933 under the title "The Reduction Potentials of Cysteine, Glutathione and Glycylcysteine."

During his eight years of research at Cambridge University and in collaboration with his colleagues, Green published an astounding 32 publications in peer-reviewed journals. His scientific genius was best expressed, however, in an eloquent essay titled "Reconstruction of the Chemical Events in Living Cells," in which he wrote at the age of 27:

The mastering of a particular machine requires not only a knowledge of the component parts, but also the practical ability to take the machine to pieces and reconstruct the original. . . . One may ask with good reason what is the point of imitating the cell with mixtures of the components in test tubes. Is it egotism and vanity on the part of the biochemist or a flair of chemical engineering? The study of mechanism, perforce, must be extremely limited in dealing with intact tissues. The variation of conditions, which is essential to studies of mechanism, must lie within the confines of those tolerated by living material. The biochemist has therefore to resort to the disorganization of the cell in order to puzzle out the mechanisms of reaction. The major discoveries of the mechanisms which cells utilize for their reactions have practically all been made by the analyses of the behavior of cell extracts and of enzyme systems.

It was also obvious that the talented Green had other thoughts besides his research, in that he became acquainted with Doris Cribb, at the time the director of the design department at the Cambridge School of Art, which ultimately led to their marriage on April 16, 1936.

In 1940 after the defeat of the British at Dunkirk, the U.S. government recalled all U.S. citizens who were living in Europe. Green, Doris, and their young daughter, Rowena, returned to the United States, where he became a research fellow in the Department of Biochemistry at the Harvard University Medical School. Having refined his skills as an enzymologist at Cambridge, as well as having acquired a magnificent English accent tainted slightly by his Brooklyn years, he began his American career under rather humbling circumstances.

Green most likely was astounded at the facilities assigned to him at Harvard, when he compared them to those he had enjoyed at Cambridge. There were no cold rooms nearby and no centrifuge in the laboratory. An old Dubosque colorimeter was available for colorimetric measurements, and strangely the cupboards in the laboratory were stuffed with an abundance of filter paper in all shapes and sizes. Most prominently lacking was a key piece of equipment widely used in the 1940s: a Warburg constant volume respirometer system. All the cofactors Green needed to conduct his experiments had to be isolated from yeast and from animal tissues. And because his research funds were derived solely from a grant awarded to him by the Ella Sachs Ploetz Foundation, he had a rudimentary research team: E. Knox, a bright medical student, and Paul K. Stumpf, a senior at Harvard College. Stumpf reminisced,

During my senior year at Harvard (1940-1941), I was required to prepare a research thesis to fulfill the honors requirement in biochemistry. Since I had become interested in enzymes, and since, in 1940, no enzymologist was on the faculty in Cambridge, Massachusetts, I made an appointment to see Professor A. Baird Hastings, at that time the chair of the Department of Biological Chemistry at the Harvard Medical School in Brookline. At the appointed hour, I was ushered into the august and wood-paneled chambers of Hastings and after a brief series of questions, Hastings informed me that he was no longer active in this field but that a "young chap" just back from Cambridge University was downstairs and it would be a worthwhile experience for me to at least meet him. Hastings then took me down to the first floor and we entered a high ceiling, dark laboratory with an enormous stone basin, and a central wooden bench. He introduced me to David Green. After Hastings left, Green asked me a few questions and then in his English accent instructed me to roll up my sleeves and go to work. In this way I began my six-year relation with Green.

Limited as Green's equipment and personnel resources were, he isolated a yeast flavoprotein, purified potato starch phosphorylase, and published his results in the *Journal of Biological Chemistry.* In 1940 he authored an important book titled *Mechanisms of Biological Oxidation,* which was published by Cambridge University Press. This 178-page book with its nine chapters had a profound effect on the fledgling field of enzymology. With great clarity Green described what was then known about the enzymatic systems involved in oxidation-reduction processes. Equally important was his essay, "Enzymes and Trace Substances," published in 1941 in Volume I of the new series titled *Advances in Enzymology*. Green wrote in that essay,

> The thesis which we shall develop in this article is that any substance which occurs in traces in the cell and which is necessary in traces in the diet or medium must be an essential part of some enzyme. We shall define a trace concentration as one where the uppermost limit is less than 5 micrograms per gram dry weight of the cell. . . . The fundamental assumption of the trace substance-enzyme thesis is that there is no rational explanation available of how traces of some substance can exert profound biological activity except in enzymic phenomena.

As with his book this essay had a profound effect on the development of the logical explanations of a large number of cofactors that were then already known or were to be discovered during the next decade. With the obvious limit of knowledge in the field in the 1940s Green had trouble explaining the functions of inhibitors and pharmacologically active drugs, as well as of plant and animal hormones. Nevertheless this thesis influenced the directions many biochemists took in their researches in the late 1940s and throughout the 1950s.

Late in 1941 Green was appointed assistant professor of biochemistry in the Department of Medicine at the Columbia College of Physicians and Surgeons in New York City. The department had a distinguished record of research in a broad range of the medical sciences and an excellent group of scientists and clinicians. In addition the building that housed the Department of Medicine also housed an equally distinguished Department of Biochemistry. Green was assigned a small but modern facility that had all the accessory rooms so sorely missing at Harvard. Soon he accumulated sufficient funds to hire a technician and a dishwasher and was able to employ his first and only graduate student, Paul K. Stumpf, who had already been associated with Green at Harvard.

Green was in his element at Columbia, and his research thrived. By 1943 he was able to double his laboratory space by remodeling an adjoining room. Sarah Ratner joined his group at that time and hired as her technician Marian Blanchard. Stumpf occupied the remaining space to continue his collaboration with Green, as well as to carry out his Ph.D. research project on the pyruvic oxidase of *Proteus vulgaris*. In the later years of his Columbia period Luis Leloir and W. Farnsworth Loomis joined Green's small research group. Throughout this period Green kept his desk in his small laboratory, where he administrated the two-room laboratory complex, ordered equipment, carried out all his own experiments, wrote his papers, and met an endless number of visiting scientists.

The Columbia period proved to be an exciting time for Green and his colleagues. With World War II fully underway and with supplies and equipment at a premium, Green was able to procure ample funding from private sources, such as the Williams-Waterman Fund of the Research Corporation, the Rockefeller Foundation, and the Winthrop Chemical Company. His research team published 20 papers on the enzymatic oxidation of amino acids, transamination, and the mechanism of pyruvic acid oxidation. In addition, he supported the construction of an ultrasonic device that was used to disintegrate bacteria, purchased one of the first battery-driven Beckman DU spectrophotometers, and was one of the first biochemists to use the new Waring blender to extract enzymes from tissues.

The efficiency and productivity of Green's laboratory were demonstrated in other ways as well. One time when he needed a supply of milk xanthine oxidase, Green managed to obtain 10 liters of raw heavy cream and in the process of isolating the enzyme produced a large amount of butter as a byproduct. Needless to say, because of shortages of butter during those war years, the byproduct was rapidly divided and consumed by his collaborators.

An excellent scientist, Green had an intuitive sense of designing relevant experiments and a knack for isolating unstable enzyme systems. Enthusiastic, impetuous, and always available for advice and encouragement, he was a rich source of information on all aspects of enzymology. David Nachmanson, Konrad Bloch, David Rittenberg, and David Shemin from the Department of Biochemistry at Columbia frequently sought his advice. Other frequent visitors included Severo Ochoa, Efraim Racker, Herman Kalckar, Fritz Lipmann, Otto Meyerhof, Boris Chain, M. Heidelberger, Karl Meyer, I. C. Gunsalus, W. W. Umbreit, Birgit Vennesland, and many of his former colleagues from Cambridge University. Green was largely responsible for the formation of the Enzyme Club, which met monthly at the downtown Columbia University Faculty Club and brought together investigators from the greater New York City area to discuss common interests. This idea caught on throughout the United States and for many years enzyme clubs were established at many urban academic centers.

In his last few years at Columbia Green was so successful in isolating and purifying soluble enzymes that he became bored with his successes and expanded his interests into the far more complicated and challenging field of oxidative phosphorylation and into multi-enzyme systems, such as those involved in the complete oxidation of pyruvic acid. For these studies Green used insoluble preparations obtained each day from rabbit kidneys and named this complex mixture of enzyme-bound systems the “cyclophorase system.” Many rabbits were needed to keep a supply of fresh kidneys for this work. The remaining parts of the rabbits were eagerly sought after by a long line of students and staff who would wait patiently outside Green's laboratory each morning for their share of fresh meat, presumably to be consumed in the evening as rabbit stew.

All the research conducted in Green's laboratory throughout this period had the imprint of his talent. If he played a key role in the selection of and was an active participant in a research project, he was a coauthor. If on the other hand he merely advised and encouraged the progress of a research project carried out by a member of his team, he did not claim coauthorship on the research paper. This policy was to become an established procedure when he became codirector of the Institute for Enzyme Research at the University of Wisconsin in Madison. Consequently many very important papers on fatty acid oxidation and fatty acid synthesis that were published in the 1950s by researchers at the institute did not carry his name.

When the University of Wisconsin decided to organize an enzyme institute, Green, an obvious choice, was selected to be its codirector. He moved from New York to Madison in 1948. Sarah Ratner remained at Columbia. Stumpf joined the School of Public Health at the University of Michigan to investigate virus biochemistry and a year and a half later was invited to join the famous Department of Plant Nutrition at the University of California, Berkeley, and there began his career as a plant biochemist.

From his arrival in Madison in 1948 until his death in 1983 Green and his colleagues engaged in six areas of research: fatty acid oxidation; metallo-flavoproteins; fatty acid synthesis; mitochondria, coenzyme Q, and the respiratory chain complexes; mitochondrial anatomy; and electron transport and oxidative phosphorylation. These areas represent unique chapters in Green's work at the Institute for Enzyme Research.

The Institute for Enzyme Research building was not ready when Green arrived in Madison, and he and his growing research team were housed in an old abandoned building on the engineering campus about two blocks from their final destination. This time is remembered among the team members—though with little nostalgia—as the "barn days." They continued the cyclophorase work with the aim of improving the solubility of some of the fractions obtained so that separation of the individual components of the energy-producing enzyme systems (e.g., pyruvate or fatty acid oxidation) could be achieved and their properties documented. They tried various tissue sources and fractionation schemes, using changes of pH and salt concentrations in combination with different centrifugation conditions, but progress was slow and insufficient soluble material for further isolation work was produced.

During his last months at Columbia and the "barn days" at Wisconsin, Green was able to attract a considerable amount of funds from the National Institutes of Health and particularly from the Rockefeller Foundation. The post-World War II period, under the spell of Vannevar Bush's famous motto "Science, the Endless Frontier," was a time of generous financial support for scientific research. It was also a time when federal agencies themselves were seeking worthwhile projects to support. No doubt Green's skill as a persuasive writer and his flair for picturing the broader implications of his experiments served him well. He was able to equip his laboratory in the new Institute for Enzyme Research building in a grand way, which was unique for those days, and support 10 postdoctoral fellows.

Green's reputation attracted many eager young scientists to the Institute for Enzyme Research, including two of us (H.B. and S.J.W.). When the building was first occupied in 1949, there were at least 30 employees, including academic, technical, and auxiliary staff. Among the first fellows who became better known later in their careers were Frank Huennekens, Henry Mahler, Jesse Rabinowitz, Harold Edelhoch, Richard Schweet, Venkataraman Jaganathan, and Rao Sanadi. They were followed by Salih Wakil, Fred Crane, David M. Gibson, Joe Hatefi, Dan Ziegler, Anthony Linnane, Giorgio Lenaz, David Wharton, Gerald Brierley, Alan Senior, Alex Tzagoloff, David McLennan, Robert Goldberger, and Roderick Capaldi. These senior fellows would eventually leave the Institute for Enzyme Research for academic appointments at other institutions.

Green also provided a temporary haven to several senior scientists who had for different reasons an interruption in their careers. Among these were Tom Singer, Edna Kearney, and John Gergely. Often there were other visiting senior scientists in the laboratory. Some visited briefly, others completed a sabbatical. Among these were Osamu Hayaishi from Japan, Vernon Cheldelin from Oregon State, Walter Nelson from Cornell, Elizabeth Steyn-Parvé from Utrecht, and even Robert Alberty from the Chemistry Department at the University of Wisconsin in Madison. The continual presence of such scientists and the ideas and expertise they brought to the Institute for Enzyme Research made it an interesting and stimulating place to be.

Green once said during those days, “If we can lick fatty acid oxidation, I will be the happiest of men.” This meant that those who worked most closely with him were involved in this project. Despite their concerted efforts none of the enzyme preparations they produced had sufficient activity to be further purified. The solution to this impasse would come from Henry Lardy and his group, who had moved from the biochemistry department to the second floor of the Institute for Enzyme Research building. One of his students, George Drysdale, was investigating fatty acid oxidation in rat liver. He had discovered that he could increase fatty acid oxidation activity many-fold in rat-liver extracts when he began the process with an acetone powder of the liver tissues. Drysdale's discovery, which was presented at an institute seminar and later published, brought the breakthrough that Green and his group needed, namely, replacing tissue homogenates or fractions thereof with extracts of acetone powders.

Though rat liver was an unsuitable source for the further fractionation he intended to pursue, Green organized the production of acetone powders from slaughterhouse byproducts, such as pig and beef liver, kidney, and heart tissues. Through this ambitious undertaking Green had the opportunity to fully display his talent for strategy and planning on a grand scale. The Oscar Mayer Company in Madison donated the tissues, and a messenger from the Institute for Enzyme Research picked them up daily in large buckets of ice. A crew always awaited the arrival of the messenger in the prep room at the institute, armed with knives, cutting boards, blenders, chilled buffers, and acetone.

The extraction techniques that Green and his staff had used previously were adapted to the large-scale production of the acetone powders. Some additional hurdles had to be overcome, however, before the individual activities could be separated. Relying on his experience with enzymes and his knowledge of how to link them in ways that would not interfere with the reaction that was to be measured, Green devised a quick and practical assay for the overall fatty acid oxidation activity. The assay, which was a typical brainchild of Green's, assayed the overall effect of the formation of the end product of the five reaction steps, namely, formation of the activated fatty acid (i.e., acyl-CoA), generation of a double bond in the fatty acid chain, hydration of the double bond, oxidation of hydroxy-acyl-CoA to form ketoacyl-CoA, and then the separation of keto-acyl-CoA into two acyl-CoA derivatives. The final acceptor was triphenyl-tetrazolium. To be able to turn over fatty acid oxidation the assay also had to contain malate dehydrogenase, oxaloacetate-condensing enzyme, diaphorase, CoA, ATP, NAD, and a linking dye, such as pyocyanin.

All of the ingredients needed for Green's assay could be prepared or purchased, except for CoA, which was available only in minute amounts and had to be obtained from microbiologists who had extracted it in a crude form from bacteria. Although Frank Strong and his group in the Biochemistry Department at the University of Wisconsin were trying to produce CoA from yeast extracts, they had not been able to get rid of the large amount of nucleic acids in these extracts without great difficulty. Richard Von Korff, who at the time was one of the institute's postdoctoral fellows, had worked out a practical assay for CoA that was based on the reduction of NAD by the alpha-ketoglutrate oxidation system and was the only assay for CoA quick enough to be used for monitoring column effluents. He suggested that Green and his group might make use of the sulfhydryl nature of CoA and try to precipitate it with a metal. Green recalled that F. G. Hopkins and his group had successfully precipitated glutathione (GSH) with mercuric ions. This was tried with yeast extract but intractable gum resulted. Beinert dug up from the literature an alternative procedure using cuprous oxide in acid solution. While it gave a clean precipitate with GSH, the results with yeast extract were again disappointing. When an excess of GSH was added to the yeast extract, however, a clean precipitate was obtained with a good yield of CoA with an about 20-fold enrichment in CoA and enough material for further purification. Having the ability to produce from 100 mg to 200 mg of CoA in a single run made it possible to prepare the real substrates, acyl-CoAs, for the enzymes by exploiting the corresponding activating enzymes.

Green quickly constituted a CoA production crew under Helmut Beinert's supervision. Frank Strong's group in the Biochemistry Department (Harvey Higgins, Bob Handschuhmacher, and Don Buyske) kindly supplied the prepurified yeast extract and worked out the method for the separation of CoA and GSH. The production of CoA in quantity was a cornerstone of all the work on fatty acid oxidation and biosynthesis. Green applied for a patent through the Wisconsin Alumni Research Foundation. The patent was never enforced, but Green, in the capacity of consultant, persuaded the management at the newly founded Pabst Laboratories in Milwaukee, who were experts on yeast products, to take over the production of CoA. Pabst then supplied Green with CoA free of charge.

These were truly exciting days. Through the combined effort of at least half of the people in the Institute for Enzyme Research—in producing the CoA derivatives, in purifying and characterizing the enzymes, and in making assays—the entire work on the enzymes involved in fatty acid oxidation was completed in less than a year. The results of this scientific *blitzkrieg* were ready for presentation at the 1953 spring meeting of the American Society for Biological Chemistry in Chicago, thus marking one of the high points in Green's career. That Green personally contributed to almost every phase of the project—not only by providing funds and the organizational infrastructure but also by continually giving advice and encouragement and by cross-coordinating the activities and cross-correlating the results—should not be overlooked. As is typical of any scientific endeavor, however, he was not alone in aspiring to the worthwhile goal of solving the riddle of fatty acid oxidation.

While Green's *blitzkrieg* was proceeding in Madison, both

Lipmann's and Ochoa's groups on the East Coast were also deciphering some of the key aspects of fatty acid oxidation. The most vocal and intense competition, however, came from Feodor Lynen and his group in Munich, who had provided the cornerstone of all these activities by isolating and characterizing active acetate (i.e., acetyl-CoA). So it happened that Lynen was also invited to the 1953 spring meeting of the American Society for Biological Chemistry in Chicago to present a plenary lecture on fatty acid oxidation. Lynen presented his lecture immediately before Henry Mahler took the podium to tell the Institute for Enzyme Research's story. Lynen was clearly upset by the timing of the presentations and it was obvious from his remarks that he felt Green had unfairly intruded upon what he considered to be his territory.

The details of the work on the individual enzymes of fatty acid oxidation are too extensive to describe here, but some outcomes deserve to be mentioned because they contributed to new lines of investigation that were developing in Green's laboratory at that time. These include the elucidation of the properties of flavoproteins, of flavoproteins in series, and of bound transition metals in flavoproteins and eventually in other proteins.

Butyryl-CoA dehydrogenase had a vibrant green color and contained some copper. It was first thought to be a copper-flavoprotein and to require copper to be active. While this belief did not stand the test of time, it nevertheless alerted the group to the possibility that heavy metals might play a role in catalysis in some enzymes that did not contain heme. This possibility was reinforced by the finding of Singer and his group that succinate dehydrogenase contains tightly bound flavin and iron. Tightly bound iron was soon also found in NADH dehydrogenase and molybdenum was found in xanthine oxidase. Similar findings were reported by other investigators, thus ushering in the fields of metal-flavoproteins and of non-heme iron proteins.

Another line of work that was also initiated at that time was directly concerned with the properties of flavoproteins, such as the formation of free radicals or of charge-transfer complexes with substrates. In addition, the first compulsory flavoprotein-flavoprotein interaction, as between acyl-CoA dehydrogenase and the electron-transferring protein ETF, was demonstrated. Beinert pursued this line of investigation with the postdoctoral fellows who worked with him after he started an independent group at the institute. It eventually led to the discovery of iron-sulfur proteins and the characterization of these proteins by electron paramagnetic resonance spectroscopy (EPR). There was collaboration with members of Green's group on some aspects of this work.

Soon after the enzymes of fatty acid oxidation were characterized Green and many of the Institute for Enzyme Research fellows moved on to study the more challenging problem of electron transport and oxidative phosphorylation. Green gave Gibson and Wakil the task of "mopping up the field of fatty acid oxidation, and showing that fatty acid synthesis is the reversal of β-oxidation." The idea that the processes of fatty acid synthesis and the beta-oxidation of fatty acids were interrelated was not new, having been articulated at the beginning of the twentieth century by F. Knoop and H. S. Raper. As early as 1907 Raper had recognized that naturally occurring fatty acids are composed of an even number of carbon atoms and had suggested that these acids are produced by the condensation of a highly reactive substance that contains two carbon atoms. Subsequent work by R. Sonderhoff and H. Thomas and later by D. Rittenberg and K. Bloch reaffirmed this view and showed that successive condensation of acetate leads to the formation of fatty acids. The identification of acetyl-CoA as the "active acetate," the fact that fatty acid oxidation leads to the formation of acetyl-CoA, and the fact that isotopic acetate was incorporated into fatty acids were sufficient evidence to claim an interrelationship between the oxidation and synthesis of fatty acids. This view was given further credence when the sequential cascade of enzymes involved in the β-oxidation of fatty acids was shown to be reversible. Even Lynen accepted the idea of reversibility. In 1953 he began his Harvey lectures by saying, "Let us consider the fatty acid synthesis" and concluded that the "beta-oxidation of fatty acids proposed by Knoop is nothing else than the reversal of this cyclic process."

Before Gibson and Wakil began the work on solving the synthesis-oxidation problem, Beinert and Stansly had attempted to convert acetyl-CoA into longer chained fatty acyl-CoAs in the presence of the purified enzymes of the β-oxidation cycle, NADH, and a reduced dye. Surprisingly they were not able to synthesize a fatty acyl-CoA longer in chain length than butyryl-CoA. This outcome suggested that the process of fatty acid synthesis was more complex than the simple reversal of fatty acid oxidation and that another approach to the problem was needed. The Gibson-Wakil team decided to use the pigeon liver system of Gurin and his collaborators, who had shown that soluble pigeon liver extracts were able to convert [^{14}C] acetate into long-chain fatty acids.

Ammonium sulfate fractionation of the soluble pigeon liver extracts yielded three separate protein fractions that collectively converted [^{14}C] acetate into long-chain fatty acids in the presence of seven essential cofactors. Further purification of these fractions and the replacement of acetate by acetyl-CoA reduced the requirement for long-chain fatty acid synthesis with only ATP, isocitrate, NADPH, and

Mn. The requirement for ATP remained absolute throughout the purification process even though acetyl-CoA was used as a substrate, and no hydrolysis or resynthesis of acetyl-CoA was noted during the incubation of the reaction mixture. The assumption the group made at the time was that ATP was required in yet unknown steps in the conversion of acetyl-CoA into fatty acids.

Other significant observations were also made. For instance, if the incubation of the reaction mixture was carried out in a conical test tube rather than in a round-bottom test tube, there was a significant increase in the incorporation of [^{14}C] acetyl-CoA into fatty acids, a phenomenon that was called the "test tube factor." If the incubation mixture was placed in a shaking bath at 37°C, there was a relative decrease in fatty acid synthesis. Moreover, attempts to identify heat-stable or -unstable "factor(s)" led to increases in the incorporation of [^{14}C] acetyl-CoA into palmitate, regardless of the source of the boiled extracts. This mystery was finally solved by accident when a phosphate buffer was mistakenly used instead of the usual bicarbonate-phosphate buffer to re-dissolve the enzyme fractions and no enzyme activity was detected. Adding bicarbonate to the preparation, however, caused very significant increases in fatty acid synthesis. Soon it was realized that HCO_3^- had to be present in the reaction mixture for [^{14}C] acetyl-CoA to be converted to fatty acids and that [^{14}C]HCO_3 was not incorporated into the fatty acid products, suggesting that its role may be to form an intermediate metabolite in the synthesis of fatty acids. When Wakil incubated [^{14}C] HCO_3^- with one of the soluble protein fractions (R_1g) in the presence of acetyl-CoA, ATP, and Mn^{++}, an intermediate metabolite was formed that could be converted into fatty acids in the presence of the second protein fraction (R_2g). This intermediate was isolated and identified as malonyl-CoA.

The first of the two soluble protein factors (R_1g) was later named acetyl-CoA carboxylase.

As the requirement for acetyl-CoA carboxylase was uncovered the question arose about whether any vitamin components were present in the enzyme fractions. The decision was made to screen the enzyme fractions at the nearby Wisconsin Alumni Research Foundation for the presence of various vitamins. When the answer came, it was a delightful surprise: the R_1g fraction contained a high concentration of biotin, which was covalently bound to the protein and remained concentrated in the protein throughout its fractionation. The Institute for Enzyme Research team also demonstrated, for the first time, that the protein-bound biotin participates in the reaction, since adding avidin (the egg-white protein that specifically and tightly binds biotin) inhibited fatty acid synthesis. Treating avidin with free biotin before allowing it to interact with the acetyl-CoA carboxylase did not inhibit the enzyme, and its product malonyl-CoA was readily formed and was converted in the presence of NADPH into the long-chain fatty acids, myristate, palmitate, and stearate by the second highly purified protein fraction (R_2g), later named the fatty acid synthase. The Institute for Enzyme Research team was also the first to show that this reaction required the presence of acetyl-CoA, which acts as the stump to which C_2 units are added. The C_2 unit in acetyl-CoA thus provides the two-carbon units, the C_{15}-C_{16}, of palmitate. Furthermore, the Institute for Enzyme Research team demonstrated that the product of this reaction was the free acid and not the acyl-CoA, as might have been expected.

The discovery of this novel pathway for the synthesis of long-chain fatty acids; the general characterization of the enzymes involved; the demonstration of the requirement for bicarbonate, ATP, and the biotin-bound protein; and the identification of malonyl-CoA as an intermediate in the synthesis of fatty acids were major contributions of the Institute for Enzyme Research under Green's leadership, support, and guidance. Gibson and Wakil, who continued their studies of the fatty acid synthetic pathway at other institutions, perpetuated the legacy of these contributions. In 1958 Gibson joined the Department of Biochemistry at Indiana University Medical School. A year later Wakil joined the Department of Biochemistry at Duke University. At Duke University and later at Baylor College of Medicine Wakil studied the enzyme systems involved in fatty acid synthesis and their regulation in more detail and established the individual steps and mechanisms involved.

With the recognition in the early 1950s that the active ingredients in the cyclophorase preparation were actually mitochondria, the word cyclophorase slowly vanished from the literature. After his success with the fatty acid oxidation system Green set out to isolate and describe the components of the mitochondrial respiratory chain and to determine how mitochondria produce energy by oxidative phosphorylation. While soluble preparations of succinate and NADH dehydrogenases, and cytochrome C were available, the link between these components was missing. In preparation for this new work Green discontinued the production of acetone powder and organized the production of mitochondria from beef and pig heart and liver. Fred Crane, who had become a group leader, was entrusted with this task, and soon the mitochondrial factory, for which the Institute for Enzyme Research would become renowned, was in operation.

Creating the mitochondria factory was no small feat. It represented a frontal attack on a major scientific problem, which Green knew could not be solved without an abundance of the material needed to carry out the work successfully. Even his competitors admitted that fact. Many laboratories in the United States and abroad, often those of former fellows of the Institute for Enzyme Research, eventually became frequent customers of the factory. Helmut Beinert even made use of this facility well into the 1990s until it was abandoned to make room for other programs, after one had learned to use *Escherichia coli* to make proteins for us.

In the years that followed the fatty acid *blitzkrieg* the Institute for Enzyme Research grew and its organizational structure changed. As Green added a number of capable lieutenants his more direct involvement, which had been considerable during the fatty acid project, became noticeably diminished. Plans for a new wing to the institute were worked out, and the new facility was occupied in 1960. It was generously equipped and air conditioned, and the rows of high-speed centrifuges in the prep room was impressive. It was in this new environment that the attack on the mitochondrial respiratory chain began after a discovery of lasting significance had been made: the discovery of ubiquinone (CoQ) as an indispensable component of mitochondria and the respiratory chain.

During the 1950s R. A. Morton in England identified and described a Q-series compound while working up the non-saponifiable fraction of fatty tissues, such as animal intestine and liver. He named the compound ubiquinone because of its apparent ubiquitous occurrence. At the Institute for Enzyme Research, Crane, Hatefi, Widmer, and Lester discovered a lipid soluble and water insoluble factor that was required for electron transfer form the primary dehydrogenases to the cytochrome system. The unknown factor had properties of a quinone and was therefore called coenzyme Q (CoQ). The structure of it was unknown for some time, but in collaboration with Karl Folkers at Merck it was shown to be an isoprenoid compound closely related or identical to Morton's ubiquinone, for which no function had been known. Thus, ubiquinone proved to be the missing link between the primary dehydrogenases, which were then known to be iron-flavoproteins, and the cytochrome system in complexes I, II, and III (discussed below) that were derived from mitochondria. Without this discovery, progress on the electron transport chain would have been severely hampered. Its significance was formally recognized when Crane received the Eli Lilly Award for this work after he had left the institute for a faculty position at the University of Texas in Austin.

After Crane's departure Hatefi, Ziegler, and their groups continued the work on mitochondrial electron transport. Elaborate spectroscopic work on the sequence of the electron-transport system components had been done in other laboratories, but the individual components had not been separated and characterized. Hatefi, Ziegler, and their groups had learned from their work on mitochondrial subfractions that by judicious use of a variety of tools: differential centrifugation; variations in temperature (including freezing), protein concentration, pH, choice of salt, and salt concentration; and chaotropes such as urea or perchlorate, so that fractionations in which the individual complexes (I-III) were free of the activities of the other complexes could be achieved.

Hatefi and his group also succeeded in combining the complexes under suitable conditions so that the whole electron-transfer system—from succinate (or NADH) to cytochrome C to oxygen—could be reconstituted, after complex IV (cytochrome c oxidase) had also been purified. These were outstanding accomplishments, which had never even been dreamed of a few years earlier. The methods used to prepare the complexes, possibly with some minor variations, were used for many years. Hatefi eventually left the institute for a position at the Scripps Research Institute in La Jolla, California, where he contributed further to this research area.

In the 1960s Green undertook a monumental effort to study the structure of mitochondria and their behavior under various conditions. He expected that this information would furnish leads toward solving the process of oxidative phosphorylation, which was the ultimate goal of all biochemists active in the bioenergetics field. Many clever minds outside the Institute for Enzyme Research were focused on the problem, and Green welcomed the challenge of the stiff competition he would face. The work on mitochondria relied heavily on electron microscopy, with which Green had no direct experience, and he therefore was dependent on collaborating colleagues for their expertise.

Most of Green's work on mitochondrial structure did not stand the test of time. Electron microscopic techniques and instrumentation have greatly improved since the 1960s, such that greater resolution of images can now be obtained. In addition, novel staining and imaging techniques have considerably refined our picture of mitochondria themselves. Nevertheless investigators at the Institute for Enzyme Research, and independently Hackenbrock, were the first to observe the transition between the orthodox and condensed conformation of mitochondria.

More significantly three of Green's postdoctoral fellows—Douglas Hunter, Robert Haworth, and James Southard—studied the relationship between configuration, function, and permeability in calcium-treated mitochondria and concluded that "mitochondria have a built-in mechanism which responds to low levels of calcium, phosphate, and fatty acids, resulting in simultaneous changes, including increased permeability, induction of ATPase, uncoupling of oxidative phosphorylation, and loss of respiratory control." These observations are considered today by many scientists to represent the first experimental observation and description of the permeability transition that is fundamental for the process of apoptosis, a topic at the forefront of biomedical science today.

The observations of the different conformational states of mitochondria led Green to conceive that the process of energy conservation and transfer might be coupled to such conformational transitions. Green realized that ordered and useful conclusions could be arrived at from the large amount of experimental material only by developing suitable theoretical concepts. Consequently in the late 1960s and throughout the 1970s he preferred to have some postdoctoral fellows in his group who were skilled in theoretical chemistry and mathematics; several publications resulted from these collaborations. Although Green's ideas of an all-embracing theory of electronic transport and energy conservation had elements that are expected to be part of any sound theory of these processes, they were too simplistic and too rigid to have influenced developments in this field.

Drawing such a conclusion about Green's ideas today are unfair, especially when we have the benefit of all the knowledge amassed during the last 30 years to 40 years. The scientific record now includes a large number of high-resolution protein structures that show the actual electron carriers and proton channels in mitochondria, well-founded and experimentally supported theories of electron transfer through peptide chains, and knowledge of electron and hydrogen tunneling. Nevertheless Green did not subscribe to a 1970s theory that has stood the test of time: Peter Mitchell's chemiosmotic theory. For that reason his name does not appear among the signatories—Paul Boyer, Britton Chance, Lars Ernster, Peter Mitchell, Efraim Racker, and Bill (E. C.) Slater—of the now famous reconciliation and acceptance statements that were published in volume 46 of the *Annual Reviews of Biochemistry* in 1977.

David Green was a complex person who had an extraordinary personality. His life was dedicated fully to research in the field of enzymology. His career in enzymology began in the 1930s when he traveled to Cambridge University in England to pursue a Ph.D. degree in biochemistry. By 1940, at the age of 30, he had written and published his classic work, "The Mechanisms of Biological Oxidations." Shortly thereafter, at the age of 31, he wrote a classic chapter that was published in volume 1 of the new treatise titled *Advances in Enzymology*, in which he projected his ideas about the role of vitamins and other trace substances as participants of enzyme function. By the middle of the twentieth century Green was the leading experimentalist in the field of enzymology. He had made significant enough contributions to merit the first Paul-Lewis Award in Enzyme Chemistry in 1946.

Green began his research career isolating and characterizing single enzymes. But when he was confronted with the complexities of the intact cell, he directed his energy to detailed studies of organized enzyme systems. As his fame spread throughout the United States immediately after World War II he attracted many junior collaborators both at Columbia University and later at the Institute for Enzyme Research. He took an active interest in his junior colleagues, not only by encouraging and inspiring them but also by allowing them to develop their own independent careers. Remarkably and unlike many of his senior colleagues in biochemistry he never insisted on placing his name as coauthor on many papers written by his junior colleagues, even when these papers described major discoveries. He established this policy while at Columbia and continued it throughout his career.

Green's enthusiasm for research was infectious to those who worked side-by-side with him, particularly at Columbia University. When he moved to Wisconsin to set up the Institute for Enzyme Research, he became burdened with the many problems of organizing and operating the institute, finding and hiring talented colleagues, and the ever present problem of procuring funding for his many research activities. Nevertheless Green continued to exhibit the same enthusiasm for the research conducted by his colleagues.

Green's wife, Doris, was an excellent companion for him and played a very supportive role throughout his career. The Greens had two daughters. Rowena, their elder daughter, was inspired by her father's enthusiasm for biochemistry. She is now a distinguished biochemist at the University of Michigan and was elected to the National Academy of Sciences in 2002. Their younger daughter, Pamela, did not choose an academic career. She married and had a daughter, Tammy Baldwin, who currently is a congresswoman representing the Madison, Wisconsin, district.

In recognition of his many contributions to the field of biochemistry the National Academy of Sciences elected Green to membership in 1962. In 1977 a symposium was held in New Orleans to honor Green's sixty-seventh birthday. His former colleagues Sidney Fleischer, Joe Hatefi, David McLennan, and Alex Tzagoloff organized the symposium under the theme "The Molecular Biology of Membranes," and many other former colleagues were present to give honor to Green as the scholar and the innovative scientist that he was. A book of the same title is available from Plenum Press, New York and London, 1978, eds. S. Fleischer, Youssef Hatefi, David H. MacLennan, and Alexander Tzagoloff, in which Green presents a summary of his life's work, and there are anecdotes from many of his collaborators that illustrate the relationship of Green and his disciples. There also was a celebration of his seventieth birthday that was held in Madison.

Green became ill during the last years of his life, and his illness and the chemotherapy with which it was treated took a heavy toll on him. Nevertheless he bore his illness with great composure and bravery and never spoke of it. Green succumbed to his illness on July 8, 1983, shortly before his seventy-third birthday, and so ended a life full of great aspirations and accomplishments. According to his wishes there was only a modest memorial service with family and friends, at which Helmut Beinert gave a eulogy. An obituary by two of us (H.B. and P.K.S.) was published in *Trends in Biochemical Sciences* in 1983; another by Frank Huennekens was published in *Bioenergetics* in 1984.

WE WISH TO THANK Professor Rowena Matthews, Green's eldest daughter, and his granddaughter, Congresswoman Tammy Baldwin, for their valuable input into the writing of this biographical memoir, and Professor Frank Huennekens and Youssef Hatefi for the background information on the Institute for Enzyme Research. We especially thank H. F. F. Dixon in the Department of Biochemistry at Cambridge University for his very helpful assistance in providing material from Green's years at Cambridge. Finally, we thank Jolita Young in the Office of the Home Secretary at the National Academy of Sciences for making available archival biographical material written by Green at the time of his election into the Academy in 1962.

AWARDS

1946 Paul-Lewis Award in Enzyme Chemistry (first recipient)
1960 American Academy of Arts and Sciences
1962 National Academy of Sciences

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David E. Green was a prolific scientist. During his pre-Cambridge and Cambridge days (1931-41) he published, alone or with colleagues, an amazing 36 peer-reviewed papers. During his Harvard stay of one year he and his colleagues published 3 papers, and during his Columbia stay (1942-49), he and his group published 24 papers. With his move to Wisconsin, over a period of 33 years, 559 scientific publications, including books and review articles, were issued. He was the author, coauthor, or editor of 8 books. Listed below is a partial list of notable publications.

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1937

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1940

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1941

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ERNEST GRUNWALD

November 2, 1923–March 28, 2002

BY EDWARD M. ARNETT

ERNEST GRUNWALD OCCUPIED an important and unique position in the development of physical-organic chemistry. To a field that has tended to be primarily identified with the elucidation of organic reactions through the kinetics of specially designed molecules, Grunwald was able to add an unusually broad perspective from the detailed physico-chemical analysis of solvent effects on the structures and energies of the principal types of reacting species. Friends and colleagues remember him as a friendly, generous man of great integrity and scientific insight.

From the mid-1940s to the mid-1990s he established a position of authority on the basic principles behind organic reactions in many ways similar to that of Louis Hammett, who originally coined the term "physical organic chemistry" as the title of his groundbreaking monograph of 1940. Unlike his illustrious predecessor, Grunwald arrived on the scene when most of the traditional barriers between physical and organic chemistry were breaking down and a postwar golden age of enthusiasm and financial support for chemistry was in full swing. Grunwald was fortuitously the right man in the right place to take full advantage of the new opportunities as they opened before him. It did not need to be so.

Born in Wuppertal, Germany, in 1923, Grunwald grew up in a middle-class Jewish family. His father had served in the German army and had received terrible wounds that required two years of hospitalization and finally amputation of his leg. During the chaotic post-World War I period of financial collapse, depression, hyperinflation, and the rise of the Nazi party the elder Grunwald established a successful business manufacturing high-quality shirts. As Ernest and his sister were growing up during the 1930s persecution of Jews became increasingly menacing, and the family made plans to emigrate to the United States. They escaped only by the skin of their teeth as is recorded in a small monograph "The Life and Graphic Arts Collection of Fred Grunwald" published by Ernest Grunwald after his father's death.

Immediately after the infamous Kristalnacht attack on the Jewish community by the Nazis, Grunwald's father was arrested and taken to Gestapo headquarters, where he seemed to be surely headed for a concentration camp with all the others who had been picked up; however, his leg wound probably saved his life, and that of the family. The Gestapo official who interviewed him was also a veteran and, out of sympathy for their shared experience, managed to save Grunwald and his family, an act of charity that may have cost the official his life later on the Russian front. After surmounting a series of gut-wrenching bureaucratic obstacles the Grunwald family managed to board a steamer to the United States and finally settled in Los Angeles in 1939. At the age of 15 young Ernie completed his high school education in the Los Angeles system and enrolled at the University of California, Los Angeles, where he earned a B.S. in chemistry and a B.A. in physics in 1944.

While still an undergraduate, he struck up a friendship that would last his lifetime with Saul Cohen, a National

Research Council fellow, who was sharing an office-laboratory with Saul Winstein, a young faculty member in the newly organized UCLA graduate program. Ernie spent many stimulating hours talking chemistry with Cohen, who had taken a number of young German-Jewish immigrants under his wing.

Winstein was exploring the details of organic displacement reactions through solvolysis (reactions where the solvent is the displacing agent). He gave particular attention to neighboring group effects, where a properly placed group within the reacting molecule participates in the displacement. Winstein was an enormously creative and demanding research director who insisted on the highest standards of rigor in the prosecution of his projects. Upon graduation from his undergraduate program Grunwald began his doctoral program with Winstein and was soon identified as a major addition to his group. A steady flow of publications came from their collaboration during and after Grunwald's doctoral research, summarized in his 1947 dissertation, "Solvolytic Substitution in the Presence of Neighboring Groups." Grunwald's introduction to the study of solvent interactions in Winstein's laboratory established a theme that would continue throughout the 50 years of his research career

After receiving his Ph.D. Grunwald continued for a brief period as an instructor at UCLA, where he had been elected to Phi Beta Kappa, but then spent the next year in an industrial position as a research chemist at the Portland Cement Company. Fortunately he was granted a Jewett Fellowship in 1949 to study at Columbia for a year. He had already been identified as a rising star, and in 1949 was recruited to join the chemistry department at Florida State University, which had embarked on a major program hoping to convert it from a prewar teachers college into "the Harvard of the South." In 1952 he married his wife, Esther.

During the years at Florida State from 1949 to 1961 Grunwald delivered on all the hopes offered by his promising early work at UCLA. Working with a relatively small group of students and doing considerable experimental work with his own hands, he began a series of imaginative physicochemical studies on systems relevant to the understanding of organic reaction mechanisms.

Although the determination of activity coefficients is scarcely the type of research that normally stirs the blood of organic chemists, it is required if one is to make a quantitative accounting for solvent effects in free-energy terms. Thanks to his wide exposure in Winstein's group to the current problems of importance to organic chemists, Grunwald was strategically positioned to identify the most relevant systems to study by classical physico-chemical techniques. He developed a number of elegant methods of electrochemistry, solution thermodynamics, and acid-base chemistry to demonstrate quantitatively how the free energies of neutral molecules and ions respond to solvent change across a variety of water-organic binary mixtures and water-salt solutions.

In a series of carefully conceived and executed papers during the 1950s Grunwald demonstrated his understanding of the basic physical problems that needed clarification for the development of theoretical organic chemistry and also his mastery of the experimental techniques needed to quantify them. During the same period he established himself as a leading authority on the physical chemistry underlying organic reaction mechanisms through his lecture presentations at important conferences and his incisive comments during discussions.

The exciting progress being made in mechanistic organic chemistry during this period inevitably led to vigorous arguments at conferences that occasionally became quite acrimonious. I remember a number of meetings where the quiet and judicious contributions of Ernie Grunwald helped to lower the emotional temperature of a debate that was getting beyond the limits of objective, impersonal discussion. His talent for diplomacy, which served him later, began to be widely recognized.

Grunwald's interest in teaching led to a productive collaboration with a colleague, Russell H. Johnson, in writing a textbook for nonscience general-education students. *Atoms, Molecules and Chemical Change* was published in 1960, and almost at once became a runaway best seller. It was adopted swiftly by over 100 institutions of higher learning and was translated into several languages. Eventually over 100,000 copies were sold as second and third editions were published. The book led the way in trying to make chemistry interesting by discussing the development of chemical principles with a history-of-science perspective instead of the traditional approaches of stoichiometrical problems and balancing equations. Grunwald's concern for undergraduate teaching was manifested again when he published *Introduction to Quantitative Analysis* in 1972 with Louis Kirschenbaum, previously a student, who later became professor of chemistry at the University of Rhode Island.

During his tenure at Florida State Grunwald also began productive discussions with his colleague, John Leffler, on how the structures of organic molecules affected their thermodynamics in solution and those of associated ions and transition states. The results were published in 1963 as *Rates and Equilibria of Organic Reactions,* a volume whose lasting influence on the development of organic chemistry is probably second only to Hammett's original monograph. Grunwald and Leffler demonstrated by comparing a wide variety of kinetic and thermodynamic properties for organic chemical reactions that they were related through linear extrathermodynamic correlations of which Hammett's well-known linear free-energy correlations were a special case. Hammett showed that the logarithms of the rates of various reactions in the side chains of substituted benzoic acids were correlated linearly with the free energies of dissociation of the correspondingly substituted benzoic acids in water at 25°C. These relationships are called extrathermodynamic because there is no inherent reason why kinetic activation parameters should be rigorously correlated with thermodynamic ones as the structures of an organic series of compounds is varied.

By the early 1960s a much larger database of kinetic and thermodynamic properties for different series of organic compounds in reaction was available than Hammett had in the late 1930s. This made it possible for Grunwald and Leffler to probe not only the free energy relationships between rates and equilibria as a function of structure but also correlations of enthalpies of activation with heats of reaction and the corresponding free-energy terms. A solid theoretical understanding developed of how the effects of structure change were related to the energies of neutral reacting species, reactive intermediates (such as ions or radicals), and transition states. This provided a rationale for every stage of an organic mechanism in a continuous coherent way. The approach could be used qualitatively for planning syntheses and was teachable to sophomore college students. Furthermore, it could be expanded almost infinitely for quantitatively predicting rates and equilibria of an almost endless variety of engineering and biochemical systems, including pharmacology, environmental science, agriculture, and medicine. The interactions of the different thermodynamic and kinetic properties were explained clearly and rigorously and a suitable notation was given for discussing them.

Needless to say, the impact of Grunwald's many contributions was appreciated by the scientific community and recognized by important fellowships (Chaim Weizmann, 1955; Alfred P. Sloan, 1958-61). In 1959 he received the American Chemical Society Award in Pure Chemistry, the most prestigious award of the society at the time for a young chemist. Within the Florida State faculty he was recognized as a Distinguished Professor in 1960.

This was also a happy time for the Grunwald family as the opportunity to enjoy the outdoors was available through the year-round comfortable Florida weather. Esther Grunwald, his wife of 50 years, remembers especially their pleasure in canoeing on the small, clear rivers and streams of the Florida panhandle.

Grunwald's interest in solvent effects increasingly fueled his curiosity about the nature and interconversion of solvent species, such as hydrogen-bonded polymers and their formation of solvated complexes with solutes. The favored physical chemical treatment at the time was the electrostatic one of Born, Debye, and Scatchard, which treated ion solvation in the general terms of a charged sphere in a continuous dielectric medium. This approach was a major theoretical advance for describing the behavior of "round" ions such as those of alkali halides, but could scarcely be expected to apply to oddly shaped organic ions with a variety of acidic and basic groups in hydrogen-bonding solvents, such as alcohols or water. Benefiting from his training in organic chemistry, Grunwald was one of the leaders in recognizing that site-specific solvent-solute interactions could also play an important role.

His careful studies of ion solvation in dioxane-water mixtures bear particular comment. The dioxane molecule is, like benzene, a six-membered ring, but is composed of two oxygen atoms symmetrically placed between two $–CH_2$-CH_2-

groups. Like benzene it has a low dielectric constant (2.21 at 25°C). However, it is not only readily miscible with many nonpolar organic solvents but also forms a continuous series of solvent mixtures with water. Grunwald and his students demonstrated, surprisingly, that alkali cations (and also some organic cations) are solvated selectively by dioxane in preference to water. This dramatically counter-intuitive discovery played an important role in developing the practical use of a variety of dipolar aprotic solvents and polyethers to selectively tie up organic cations and thereby activate organic anions that otherwise would be associated with their counter-cation, and thus opened up a whole new area of synthetic organic chemistry.

In one carefully crafted and analyzed study after another through the years Grunwald and his students added London dispersion forces and hydrogen bonding to the list of important interionic interactions. Although his research at Florida State provided powerful and illuminating new information about the interactions of solvents and solutes, it was mostly limited to the overall thermodynamic behavior of the systems studied with very little direct structural information about the various subspecies or kinetic measurements of their rates of interconversion. During the late 1950s a variety of new relaxation techniques were being developed to measure rates of reactions that were several orders of magnitude faster than any previously observed. Systems at equilibrium were suddenly subjected to a shock of some kind and the rate of return to equilibrium was followed by an appropriate method of observation.

Nuclear magnetic resonance was turning out to offer a cornucopia of opportunities for the study of an almost endless variety of previously inaccessible problems. At the Bell Telephone Laboratories Saul Meiboom was developing techniques for analyzing the broadening of nuclear magnetic resonance spectral lines as a means to investigate fast reactions. Grunwald saw this as the opportunity to study the structural and kinetic details for the interconversion of the kinds of hydrogen-bonded solvent-solute complexes whose presence he had inferred from his thermodynamic studies. In 1961 he resigned from Florida State and joined Saul Meiboom, who had the best equipment in the world for studying the systems of interest to Grunwald. The prospect of having hands-on access to this marvelous new tool was overpoweringly attractive.

At the time, Bell Labs enjoyed a prestige equal to or greater than any research institution in the world. An apocryphal tale of the times referred to a leading scientist at Bell who was being courted by the provost of one of the leading universities in his field. His rejection letter is quoted as saying, "I know I could make more money as a Distinguished Professor at X university, but I wouldn't have nearly as much freedom as I have here at Bell to work on problems of interest to me."

In this high-powered environment the Grunwald-Meiboom collaboration was indeed highly productive and a steady series of papers appeared during Grunwald's three-year stay at Bell illuminating the questions of greatest interest about the thermodynamics and kinetics of hydrogen bonding and proton transfer for a variety of classical Brönsted acid-base systems (e.g., methylamines in water, methanol, and acetic acid). These studies broke important ground by identifying the particular players and their interactions in some of the simplest and most fundamental systems in chemistry. One can only imagine the enthusiasm with which the great solution chemists of the past would have welcomed these revealing studies.

Idyllic as the sharply focused environment at Bell may sound Grunwald began to miss the more varied life of the university. In several chats with him during these years I began to hear wistful comments comparing the sometimes stressful life of total concentration on one or two problems at a research institute to teaching and working with students on a variety of problems and enjoying the interactions with colleagues in different areas of the sciences and other disciplines. Fortunately, he was able to contact Saul Cohen, his old friend from UCLA days, to see if he could arrange a professorship in the Brandeis Chemistry Department. Grunwald and his family were able to move in 1964 and spend the rest of his life in a friendly and stimulating environment.

In 1965 he was chosen to chair the Chemistry Department. The department was planning the construction of a new building and to the surprise of some of his colleagues Grunwald threw himself into the planning with great enthusiasm and saw several important opportunities for improving the design. His tenure as chairman is remembered very favorably by his colleagues; to quote one, "Ernie's old-world courtesy and gracious good humor were endearing. His unassuming bearing contrasted sharply with his critical thinking and vigorous, enterprising research style! In his editing and administrative duties, his judgments were firm, and shaped by the integrity of his values, humane outlook and common sense."

His style as chair is remembered as being totally consistent with his normal demeanor at home or in the laboratory. He was invariably friendly yet self-contained. He represented the department to the higher administration assertively yet politely. In approaching departmental problems, as with scientific problems, he is remembered for listening patiently to the various sides of a problem and then coming up with a totally new approach of his own. He was generally apolitical, although he had very strong feelings of concern for the welfare of individuals, especially students. Despite his strong bonds to the Jewish community his view was that of practical, cultural Judaism rather than that of religion. In the broader academic community of Brandeis Grunwald was chosen to chair the School of Sciences Council, a position that he held through four terms—a testimony to his diplomatic talent.

The Grunwalds moved into a house close to the campus so that he was only a short, pleasant walk from the laboratory and his NMR machine. His love of the outdoors and walking also were able to find expression in hiking trips in the White Mountains in New Hampshire, where he and Esther spent many a happy weekend. Later in his retirement Grunwald wrote several reminiscing essays, apparently for his own enjoyment as much as anything. One of them, entitled "My Hikes in the White Mountains," describes his enjoyment in taking five- to six-hour hikes with Esther and one of their dogs on trails that were carefully chosen to be invigorating. Later the Grunwalds built a vacation home on Cape Cod as a retreat from their active life at Brandeis.

Back in the department Grunwald's responsibilities expanded as he became an associate editor for physical organic chemistry for the *Journal of the American Chemical Society* in 1977, serving for four years under Cheves Walling and later Allen Bard. Earlier he had served on the editorial boards of *Accounts of Chemical Research* and *The Journal of Solution Chemistry.* Again he wrote an essay, "Peer Review," describing his perspective and experiences in performing this important job. His approach was typically careful in evaluating the number of minutes that would be required for each stage of reviewing and editing each paper and finally budgeting two and a half days a week for his work. The problems that he faced in finding suitable reviewers and negotiating between authors, reviewers, and editors would be familiar to most members of the National Academy of Sciences, but I do not recall having seen them organized and described step by step in one place before. His essay would be of value to any young faculty member entering the complicated and often misunderstood culture of peer review on which so much of the credibility of the scientific effort depends. As both Walling and Bard make clear in their letters responding to the essay, many of the problems of handling manuscripts that Grunwald found to be most frustrating have been dealt with through computer systems and improved policies.

The illness and death of Fred Grunwald, Ernest's father, in 1964 meant that Ernest and his sister, Lottie Talpis of Beverley Hills, found themselves responsible for handling the Los Angeles shirt factory that their father had established soon after their arrival in the United States. Furthermore, Fred Grunwald had gathered an outstanding collection of over 3,500 prints, including numerous pieces by Renoir, Picasso, Toulouse-Lautrec, and Kollwitz. Ernest Grunwald and his sister donated these and a number of other prints, drawings, photographs, and notebooks to UCLA, where they are housed as the Grunwald Center for the Graphic Arts in the Armand Hammer Museum of Arts and Cultural Center in Westwood Village.

By 1971 Grunwald's stature in the scientific community had led to election to membership in the American Academy of Arts and Sciences (in 1969) and to the National Academy of Sciences (in 1971). His search for new methods to study the rapid interconversion of solvated subspecies now led him into a series of investigations using electric dipole moment measurements with particular interest in solvation of ion pairs and relaxation of the ionic atmosphere. Inevitably these studies combined with his ongoing use of NMR line broadening focused his interests on the making and breaking of hydrogen bonds between ions and the solvent and eventually on the perennial problem of the microscopic structure of liquid water.

In the mid-1970s his research moved in the direction of attempts to use megawatt infrared lasers to selectively stimulate and cleave specific bonds in organic molecules. Early announcements of this approach by Russian chemists had excited both great interest and skepticism. Clearly, control of chemical reactions could be moved to a new level of sophistication if a blast of infrared light at a sharply tuned frequency could be used to break a specific bond and initiate reaction at a chosen locus in a molecule. However, the majority of experts in the relevant fields of spectroscopy believed that even if the energy burst could be absorbed preferentially by the desired bond, it would be redistributed throughout the molecular framework before the bond could be selectively pumped up to the point of cleavage.

In a series of 10 papers Grunwald described his carefully designed studies to test the basic requirements for selective bond cleavage by infrared lasers. The work culminated in a book, *Megawatt Infrared Laser Chemistry* (1978), with D. F. Dever and P. M. Keehn as coauthors. In the ensuing years it has turned out that selective bond cleavage is not impossible, but that so far the early hopes for a powerful tool of broad usefulness were too optimistic.

In 1989 Grunwald retired from Brandeis as Henry F. Fischback Professor of Science Emeritus. Following his retirement he completed several research papers and then turned to writing a book, *Thermodynamics of Molecular Species* (1997), which attempted to pull together and organize his life's work in a coherent theoretical overview. This task was accomplished well before his death on March 28, 2002, at the age of 78. He also took pleasure in writing an essay on his volunteer work helping elderly low-income people prepare their income taxes.

Grunwald's principal lifework was dedicated to understanding the ways that solvents affect the behavior of organic chemical reactions. To pursue this goal he took advantage of every advance that was offered by new instrumentation during the 50 years of his research career. At the beginning, in the 1940s, he relied on classical physicochemical methods to determine important thermodynamic properties, such as activity coefficients, in order to analyze solvent effects on reaction rates. Later, as new relaxation methods for studying fast reactions developed he was able to identify many of the short-lived, interconverting solvent-solute species whose presence could only be inferred or speculated upon from the gross thermodynamic or kinetic measurements. His studies are notable for their theoretical rigor as well as experimental ingenuity and elegance. Ernest Grunwald occupied a unique niche in the turbulent interface between physical and organic chemistry. He is survived by his wife, Esther; daughter, Judith; and two granddaughters. He is remembered with much affection by his many friends in the Brandeis and scientific communities,

I APPRECIATE THE HELP in preparing this memoir that I received from Esther Grunwald, his wife, and his colleagues in the Brandeis Chemistry Department, especially professors Saul Cohen, Emily Dudek, Myron Rosenblum, Colin Steel, Phillip Keehn, Henry Linschitz, and Robert Stevenson.

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WILLIAM REDINGTON HEWLETT

May 20, 1913–January 12, 2001

BY ROBERT J. SCULLY AND MARLAN O. SCULLY

WILLIAM REDINGTON HEWLETT passed away on January 12, 2001, endowing that day with a national significance. The Silicon Valley miracle was in large part fostered by William Hewlett and David Packard. Indeed, the ideas and ideals of the Hewlett-Packard Corporation set a high standard for the industry. During his life Hewlett was recognized by his profession, his country, and his peers as the hero that he was. He was president of the (now) Institute of Electrical and Electronic Engineers in 1954 and was elected to membership in the National Academy of Engineering in 1965 and the National Academy of Sciences in 1977. President Reagan awarded him the National Medal of Science in 1983, and he was awarded the prestigious "Degree of Uncommon Man" by Stanford University in 1987.

Hewlett-Packard was and is a testament to the success of the free enterprise system and the American dream. The simple, honorable ideals and intensely productive practices they employed propelled a business started in a garage during the Depression to stardom. William and his lifelong friend and partner, David Packard, owned and ran a unique company dedicated to the premise that profits were based on the well-being of its most important assets: its employees. It was in the dark days of the Depression, in a small Palo Alto, California, garage where the country witnessed the birth of an era. How William Hewlett and David Packard started that era is a fascinating story in the annals of engineering science.

William was born on May 20, 1913, in the intellectual Mecca of Ann Arbor, Michigan. His father, Albion W. Hewlett, was a doctor who taught medicine at the University of Michigan. When Will was three, his dad moved the family back to their native California, where he taught at Stanford University. Despite having dyslexia, Will attended a prep school, where he excelled in math and the sciences. But he had problems with everything else. Many dyslexics have problems adapting and developing in society, but Hewlett dealt with this reading disability in his usual engineer fashion. He adapted by learning to memorize and repeat subject matter over and over to himself. Life's future obstacles would be dealt with in a similar fashion; they were intriguing challenges begging a solution. Will would prove to be a solution master.

At an early age he began his engineering career the way many others in his profession do: by blowing up things. His preferred method was stuffing doorknobs full of explosive. Years later he stated that a doorknob was hollow and compact, and you could put it to good use as a bomb. Despite this not so docile hobby, Will was a good and well-behaved kid. Compared with his adult life, he preferred to keep to himself as a young man. He wasn't nearly as socially active as his partner, Dave Packard. Dyslexia does not do wonders for a person's self-esteem, and it is likely that he spent much of his energy growing out of his disability.

But grow out of it he did, developing a love for the outdoors as an avid mountain climber with a penchant for camping. To Will there was a bright new world, including the Sierra Nevadas waiting to be discovered. At age 12, Will's father died of a brain tumor. Deeply troubled by his father's death, he sought the refuge of the school science lab and the looming mountains. To help the family cope with the tragedy Will was moved with his sister Louise to France for a year. While there he was tutored by his mother and grandmother.

High school would not prove him to be an outstanding student. Nevertheless he had his sights set on attending Stanford University. The principal initially refused to recommend him to the university, until she learned his father had been Albion Hewlett. The surprised principal exclaimed, "He was the best student I ever had." The letter of recommendation to follow would allow Hewlett to open the doors to a new world of technology. An ROTC cadet at the college, he became a reservist officer in the army, with a specialization in ordnance. He received a B.A. from Stanford in 1934. Two years later he was awarded the degree of Electrical Engineer and three years after that he achieved his M.S. in Electrical Engineering, this time from the Massachusetts Institute of Technology. Hewlett wasted no time; immediately after graduation he formed his own company.

Will had become close friends with David Packard while an undergraduate at Stanford. They had many things in common, apart from the social scene. Both liked to blow up things. Both had an avid love of outdoor activities. Hunting, fishing, skiing, and mountain climbing were the early trademarks of the future fathers of the Silicon Valley. And both had a burning desire to discover, develop, and invent.

So it was that Hewlett-Packard came into being in 1939 in their garage with an investment capitol of $538. The Depression didn't make things easy for the new company, but it did push the two partners to perform. Hewlett recalled, "In the beginning we did anything to bring in a nickel. We had a bowling lane foul line indicator. We had a thing that would make a urinal flush automatically as soon as a guy came in front of it. We had a shock machine to make people lose weight."

Over the next two years Hewlett-Packard inventions would become more practical. One of their notable early achievements was the development of an audio frequency oscillator. Those were the days when electronics was a new field: half science and half art. The resistance-tuned oscillator was a new idea that hadn't come to fruition because of inherent stability problems. Hewlett came up with an ingenious solution. He invented a variable frequency oscillator that was stabilized by a small light bulb. This simple addition to the circuit made the device into an inexpensive, reliable instrument.

They called their oscillator the 200A, and it was used to calibrate the sophisticated sound systems of the large studios. They called it 200A because calling it, say 1A, would reflect inexperience to potential customers. Walt Disney purchased eight of them for use in the film *Fantasia*. The success of these and other devices like it helped set the stage for Hewlett-Packard's future and present missions in regard to research and development. Although pioneers in the field of new research, many of the technologies they would field would be testing and calibrating equipment for technological equipment already in existence.

The spring of 1941 would see the young company thrust into World War II before Pearl Harbor. Will was called to active duty. Hewlett had no problem adapting to the rigors of military discipline; in fact, even in time of war he found the army to be too lax for his liking! Decades after the war he would recall his military service for biographers in that chronological, duty-station manner that is a trademark of former servicemen. It's a reflection of the veteran's former integrity, motivation, and even optimism when he sees his service time in such a manner; he is a small cog in a big wheel, equal and generic but nonetheless important and useful.

Hewlett originally worked in the Aviation Ordnance Department. It wasn't a good use of his technical skills but rather a result of his ROTC training. Here his buddy Dave would look out for him, as he would continue to do many times in the coming years. Dave Packard was making contacts left and right in the defense sector, and his technological breakthroughs in the young company were earning him friends in high places. As such, he wrote Colonel Colton of the Signal Corps, explaining the benefits of having Hewlett transferred.

Hewlett was transferred to the Signal Corps at Fort Monmouth. Here he would put his electrical engineering skills to good use. But not for long; that summer Hewlett-Packard would go from a partnership to becoming a corporation. The difference this made for Hewlett was that the government now recognized him as an essential employee. He was released back to Palo Alto for a couple of months, only to be called back to Washington after Pearl Harbor.

Again he worked for Colonel Colton. For most of the war he worked for him in Washington, learning among other things the bureaucratic ways there. Hewlett was unaccustomed to working less than 12 hours a day, but he was forced to as the bureaucrats in Washington insisted on locking the safe at six o'clock each night. This meant Will had to hand over his documents and call it a day. Although Will found this frustrating, his new wife, Flora Lamson, a biochemist whom he married in 1939, was delighted. Flora was a loyal wife who accepted the fact that a man like William spent most of his time away from home. But she would be quite busy herself, eventually taking care of their five children, Eleanor, Walter, William, James, and Mary.

Hewlett had more energy and ambition than the army required, but he was still well liked. He had that slightly rugged athletic-looking face that is popular in the military. World War II would go out with a bang for Hewlett. He would be transferred to a staff job, working for General Wharton in the new products division. He was sent to the Philippines toward the end of the war, where he helped with assimilating new technologies into the frontline units of the military.

At about the time of the surrender he was given an intelligence assignment that would take him to Japan. He was part of a team that would take a quick look at what the Japanese had been doing. Hewlett suspected that part of the purpose was to discover what they had been doing with the atomic bomb, but he wasn't told that. Will took an interest in a man named Yagi, who was the civilian head of research and development. Yagi was helpful and knew the right directions in which to steer the investigators.

Yagi was a very frustrated man. He explained an example of his situation to Hewlett, who recalled that the Japanese government had announced the development of a "death ray." Yagi knew it was nonsense, but he had to appear to work on developing one anyway.

Hewlett found the Japanese electronics to be underdeveloped and primitive. Contrary to popular opinion, there was little if any cooperation between the Japanese army and navy or between the government and the civilian research and development community. Hewlett made the observation that the Japanese navy had been around the world for 10 years prior to the war; they knew what was out there and what the country was getting into. On the other hand the army had spent the previous 10 years occupying Manchuria in China, where they met ill-equipped, untrained forces. Will noted the result was that the army believed it could defeat the world. It was a fitting discovery for the lieutenant colonel on which to end a military career. Mr. Hewlett returned to Palo Alto in 1945.

If Hewlett did not get shell shock in combat, he certainly got a shock when he came home. The company was no longer garage based, but had become a thriving industry of over 200 employees. It was growing at 100 percent per year. But any shock he had would soon turn to gratification, as he was named vice-president of the burgeoning company. And if David hadn't done enough in securing that for him, William could always look at his old friend's past pay stubs. David thought it unfair to stay home and make more money than his buddy who went off to war, so he kept his salary at a level lower than that of Will's service pay.

Sound like today's corporate executives? Things were different then. They were different because men like Hewlett made them different. The country's greatest generation came of age during the Depression and left its youth behind on the battlefields of World War II. They were free to work at adult jobs in their youth, blow up things, and generally find their niche in life as they saw fit. From his defeat of dyslexia to his unusual role in the war, Hewlett saw humanity as a raw resource of power that could only grow when left alone and encouraged from the sidelines. As such he brought a unique style of management to his company.

Will set the foundation of one of the world's greatest and most effective philosophies. Like his designs, it is simple and straightforward. It is no more complicated than the level of enthusiasm and encouragement that develops with compassion, trust, and loyalty. Today it is backed by enthusiastic and loyal employees who extend the frontiers of technology while adding economic wealth to the nation.

Will was a leader of engineers and the soon-to-be Silicon Valley was becoming their Mecca. All of the United States' eastern institutions loved him, and after the war, from MIT to Bell Labs, a western migration of the country's technological think tanks began. They came for the free and open environment that was as enticing to them as a trophy buck is to a hunter. Once the engineers got settled in their new home they lit up the "developmental skies" with their fireworks. Free from micromanagement and given great personal empowerment and discretion, Silicon Valley personnel cooperated greatly with each other, turning the wheels of invention as never before. Hewlett-Packard employees in particular were loyal and dedicated men and women who took their work home with them; they slept with it, they worked it on napkins, they discussed it on the golf course and at dinner.

From his early work on electronic oscillators to the development of the H-P pocket calculator, Hewlett was a problem-solving pathfinder. He didn't manage by directive; his style was "management by walking around." Paul Ely recalls Hewlett's visits to his microwave lab with pleasure, in his words: "Hewlett knew more about more things than any person I ever met."

As Hewlett-Packard prospered the Silicon Valley prospered around them. But the Hewlett-Packard Way never adjusted or deviated in any way from those original principles of employee empowerment and management and subordinate teamwork. The ideals set forth were so pure and special that they spoke, and continue to speak, for themselves. Will employed them as a "code," one that transcended written guidelines and needed not a great deal of training to grasp.

One major difference exists between the Hewlett-Packard Way of then and many of those that try to emulate it today. Today's highly technological and highly complicated businesses feel that ideals and mores must change and advance right alongside technology. True, technology must adapt and reinvent itself to continue to be productive. But the definition of ethical behavior must not be constantly changing. To do so is superficial and can't foster the lifelong nurturing necessary in developing a truly loyal and honest workforce.

William did not intend the Hewlett-Packard Way to be conveyed by some public relations types. No illustration of the Hewlett-Packard Way is better given than by that of Will himself. In the mid-1960s the company was well into its noted reputation for making quality printers. But a quality problem arose with one of their models, which exposed the company to warranty problems in the tens of millions of dollars. A frazzled Rick Belluzzo was required to describe the situation to the board, including Hewlett.

As Belluzzo described the printer holocaust that was losing large sums of money, he couldn't tell whether Hewlett was even paying attention. The dreadful proceedings dragged on and on, until Will finally asked, "Rick, what have you learned from this experience?" They talked about the mistake and found out where they went wrong, to which Will said, "Make sure your number one responsibility is to take care of our customers."

Such is the HP Way. Rather than belittling people for their mistakes, management works with them to solve the problem and bring out the best in themselves. Finger pointing and politics can find no place in this. It is a strong, brotherly concept requiring no great deal of explanation. It is, in the words of our founding fathers, "a truth that is self-evident."

As the company expanded to become the backbone of the Silicon Valley, Will seems to have realized that while the HP Way was not subject to change, his role in the world was. He was now one of the richest men in the world (and to think his partner, Dave, had deliberately kept his salary below that of his service pay). Will would extend the HP Way beyond profits and productivity to enter into the world of development through philanthropy. Flora was an active supporter with Will in this until her unfortunate death. Will would go on to marry Rosemary Bradford, helping to raise his five new stepchildren. And his interests in supporting the community would skyrocket.

As such Will was keenly interested in the fields of medicine and education. He even served as the director of the Drug Abuse Council in Washington, D.C., from 1972 to 1978. He was on the board of numerous colleges and hospitals and was the recipient of numerous titles and honorary degrees. In 1995 he donated $70 million to the Public Policy Institute of California, a group that studies the economic, social, and political issues facing California.

This last action would be his final major endeavor. It was opposite in scale to his opening of the "HP Garage," but equal in vision. The Silicon Valley had brought enormous growth and prosperity to southern California, but with it came the traditional challenges of overpopulation, massive immigration, destruction of the once plentiful forests and so on. William contributed to the conglomerate he had laid the foundation for with an open heart of compassion, sympathy, and understanding—with the hope that the next Silicon Valley would be a more perfect organization, not just a group of for-profit companies, but a society. A society of people from all walks of life that would live and work together with dignity and respect. It was a step toward restoring the spirit William remembered in the California of half a century ago, so much of which has been lost.

It was as if William sensed his time was up when he made that donation. In the late 1990s he was stricken with a series of debilitating strokes that left him in a wheelchair. However, the HP Way is alive and in full force. On that day of January 12, 2001, William Hewlett's influence did not end. His enduring and legendary contributions continue to enrich us all.

EDUCATION

1934	B.A., Stanford University
1936	M.S., Electrical Engineering, Massachusetts Institute of Technology
1939	Degree of Electrical Engineer, Stanford University

PROFESSIONAL EXPERIENCE

1941-45	United States Army, on the staff of the army's chief signal officer and later transferred to the New Development Division, serving in Washington, D.C., the Philippines, and Japan. He attained the rank of lieutenant colonel.

At Hewlett-Packard Company:

1939-47	Cofounder and partner
1947-57	Vice-president and director
1957-64	Executive vice-president and director
1964-68	President and director
1977-78	President, chief executive committee, chief executive officer and director
1978-83	Chairman of the Executive Committee and director
1983-87	Vice-chairman, Board of Directors
1987	Director emeritus, Board of Directors

SERVICE

1956-58	Palo Alto Stanford Hospital Center, president of the board (director, 1958-62)
1958-68	Trustee, Mills College, Oakland, California

1962-72	Trustee, The RAND Corp.
1963-68	Trustee, California Academy of Sciences
1963-74	Trustee, Stanford University, Stanford, California
1965-68	Member, President's General Advisory Committee on Foreign Assistance Programs, Washington, D.C.
1965-74	Director, FMC Corp.
1966-69	Member, President's Science Advisory Committee
1966-83	Director, Chrysler Corp.
1966-94	Chairman, William and Flora Hewlett Foundation
1968-90	Trustee, California Academy of Sciences (honorary)
1969-70	Member, San Francisco Regional Panel of the Commission on White House Fellows (chairman, 1970)
1969-77	Director, Overseas Development Council
1969-80	Director, Chase Manhattan Bank
1969-81	Member, San Francisco Bay Area Council
1971-90	Trustee, Carnegie Institution of Washington (trustee emeritus, 1990)
1972-74	Consultant, The RAND Corp.
1972-74	Director, Drug Abuse Council, Washington, D.C.
1972-78	Director, Kaiser Foundation Hospital and Health Plan Board
1974-85	Director, Utah International, Inc.
1980-81	Coordinator, Chapter on "Research in Industry," National Academy of Sciences Five-Year Outlook Report
1980-86	Chairman, Carnegie Institution of Washington, Board of Trustees
1982-83	Member, National Academy of Sciences Panel on Advanced Technology Competition
1986-88	Director, University Corporation for Atmospheric Research Foundation
1986-92	Member, International Advisory Council, Wells Fargo Bank
1986-2001	Director, National Academies Corporation
1987-88	Member, Advisory Council on Education and New Technologies, the Technology Center of Silicon Valley
1987-2001	Director, Monterey Bay Aquarium Research Institute

PROFESSIONAL RECOGNITION

1969	California Manufacturer of the Year, California Manufacturers' Association
1970	Business Statesman of the Year, Harvard Business School of Northern California
1971	Medal of Achievement, WEMA (Western Electronic Manufacturers Association)
1973	Founders Medal, the Institute of Electrical and Electronics Engineers (IEEE), to Hewlett and Packard
	Industrialist of the Year to Hewlett and Packard, California Museum of Science and Industry and California Museum Foundation
1975	SAMA (Scientific Apparatus Makers Association) Award to Hewlett and Packard
1976	Vermilye Medal to Hewlett and Packard, the Franklin Institute, Philadelphia
	Corporate Leadership Award, Massachusetts Institute of Technology
1977	Medal of Honor, City of Boeblingen, West Germany
	Herbert Hoover Medal for Distinguished Service, Stanford University Alumni Association
1984	Henry Heald Award, Illinois Institute of Technology
1985	National Medal of Science, U.S. National Science Committee and former President Reagan
1987	Santa Clara County Business Hall of Fame Laureate Award, Junior Achievement
	World Affairs Council Award, World Affairs Council of Northern California
	Degree of Uncommon Man, Stanford University
	Commander's Cross of the Order of Merit of the Federal Republic of Germany
1988	National Business Hall of Fame Laureate Award, Junior Achievement
1990	John M. Fluke, Sr., Memorial Pioneer Award, Electronics Test Magazine
1991	Silicon Valley Engineering Hall of Fame Award, Silicon Valley Engineering Council

HONORARY DEGREES

1966	LL.D., University of California, Berkeley
1976	LL.D., Yale University
1978	D.Sc., Kenyon College
	D.Sc., Polytechnic Institute of New York
1980	Eng.D., University of Notre Dame
	Eng.D., Utah State University
1983	Eng.D., Dartmouth College
	LL.D., Mills College
1985	L.H.D., Johns Hopkins University
	Doctor of Public Policy, RAND Graduate Institute
1989	Doctor of Electronic Science, University of Bologna, Italy
1991	Doctor of Humanities, Santa Clara University

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1951

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With D. Packard. Timing apparatus. U.S. Patent 2,558,249.

1952

Modified Wien-Bridge Oscillator. U.S. Patent 2,583,649.

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1953

With H. M. Zeidler. High-frequency generator. U.S. Patent 2,652,511.

Evaluation of Institute of Radio Engineers professional group plan. *Inst. Radio Eng. Proc.* 141(Aug.):964.

1955

With H. E. Overacker. Adjustable coupling device and monitoring means therefor. U.S. Patent 2,724,799.

1959

Broad band waveguide directional coupler. U.S. Patent 2,871,452.

1961

With J. M. Cage. Direct current amplifier and modulator therefor. U.S. Patent 3,014,135.

1966

With W. B. Wholey. Fixed coaxial line attenuator with dielectric-mounted resistive film. U.S. Patent 3,227,975.

With H. T. Friis. High-frequency impedance bridge utilizing an impedance standard that operates at a low frequency. U.S. Patent 3,260,936.

1967

Ohmmeter utilizing field effect transistor as a constant current source. U.S. Patent 3,328,685.

1971

With G. Justice. Distance measuring apparatus. U.S. Patent 3,619,058.

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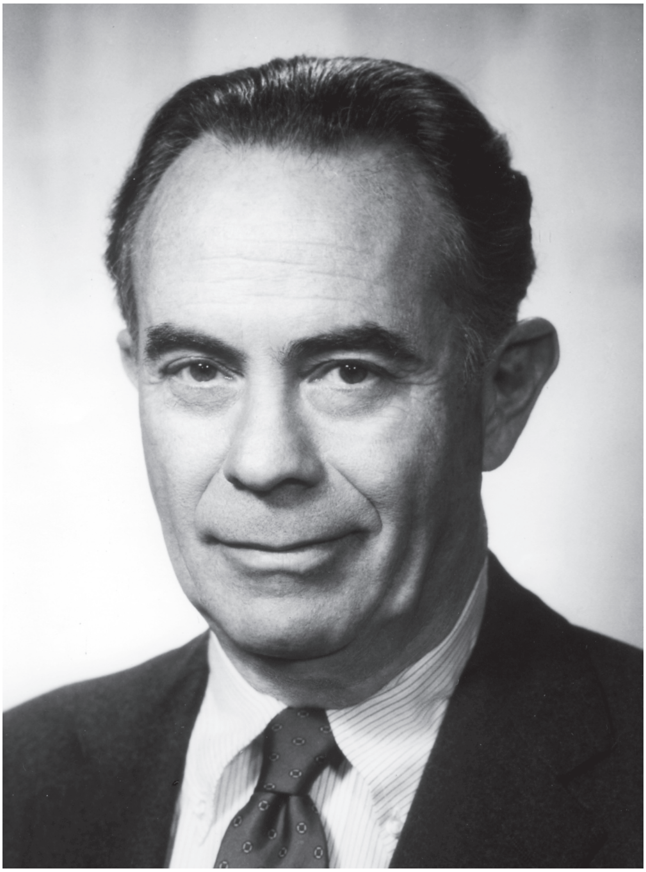

Photo by Bachrach

JAMES GERALD HIRSCH

October 31, 1922–May 25, 1987

BY CAROL L. MOBERG AND RALPH M. STEINMAN

JAMES HIRSCH WAS A PIVOTAL figure in leukocyte biology. He helped turn a mid-twentieth-century focus on bacterial cells to a new consideration of the cell biology of the host's responses. His talents touched upon a full spectrum of responsibilities: physician, investigator, educator, author, and statesman of science. Trained as a chemist and clinician, he arrived in the laboratory of René Dubos in 1950. As a biologist he made his earliest contributions in fields as diverse as therapy of human tuberculosis and the discovery of antibacterial peptides. Soon his laboratory became a nucleus for those who wished to understand the cell biology of the immune response. His cinematography and electron microscopy made his discoveries more exciting. As a professor and dean of graduate studies he was a gentleman-scientist and inspiring mentor who took great pleasure in nurturing other people's talents. As a foundation president he brought the fascination of science and medicine to new and larger audiences. Above all, as a caring doctor, he extended a warm welcome, took time to listen, and offered generous, heartfelt advice.

James Hirsch was born in St. Louis, Missouri, the only child of Mack J. and Henrietta B. (Schiffman) Hirsch. His father, who was born near Odessa, Russia, emigrated as a young boy, first to Manhattan's Lower East Side, and soon thereafter to southern Illinois, where he and his brother operated several dry goods stores. Jim's mother emigrated with her parents and four siblings from Poland to St. Louis, where they opened a clothing store.

Jim was raised 60 miles southeast of St. Louis, in Pinckneyville, Illinois, a coal strip-mining and farming town of 3,000 people. His father's store was in the old opera building on the town square. There his mother became the buyer for ladies' clothes and household linens, and she often took Jim with her on buying days in St. Louis and New York City. Jim occasionally worked in the store, but he disliked it immensely, saying he hated to ask for money. Until the end of his life, however, as a philanthropist he relished the idea of giving money to worthy causes.

His childhood playmates, John Sheley, later the editor of the Pinckneyville *Democrat*, and Robert Bortle, now a retired coal miner, remember Jim as an average boy in every way. He liked swimming and fishing in the creek, riding bikes, and playing ball. He owned the first electric model railroad in town, a hobby he pursued even as a medical resident. Always fascinated by ingenious machines, which he called "toys," he loved to build or tinker, to repair or restore, to fashion or play with things.

No one from either side of his family had an academic or medical background. Jim attended a German Lutheran preschool before entering the Pinckneyville public grade schools. At age 12 his parents sent him to Western Military Academy in Alton, Illinois, where he took a keen interest in photography and was active in several sports. He was salutatorian of his class of 1939. To prepare for college and a major in chemical engineering, Jim worked summers at St. Joe Lead Company. He had originally planned to attend Massachusetts Institute of Technology, but he changed his plans when a close friend applied to Yale. Jim was accepted at Yale, but his friend was not, so he went alone to Yale's Sheffield Scientific School. He liked to recount that entering an Eastern establishment school had its eye-opening moments, such as the reception he got when he stepped off the train in New Haven dressed in a kelly green suit, a parting gift from his parents.

Former roommate Charles Frankenhoff described Jim's college years as "happy go lucky, never, but fulfilling." Motivated and disciplined to get his studies done, Jim urged friends to do the same. He was an avid photographer and managing editor of *The Yale Scientific Magazine*, for which he contributed major articles on strip-mining machines, speech synthesizers, and Yale's civil pilot training course for students. Sporting activities were severely curtailed after a knee injury suffered while playing basketball. In his sophomore year he received a rare gift for the time: an automobile that gave him freedom to date girls from nearby schools (Yale was then all male) and to keep alive his romance in Illinois with Marjorie Manne. Marjorie and Jim married in June 1943 in St. Louis.

Disenchanted with prospects for a career in chemical engineering in the aftermath of the Great Depression and saying that "anyone graduating as a chemical engineer sold apples," he considered other possibilities. After Pearl Harbor he focused on military service. During his junior year he decided to enter medical school and took a "hurry up zoology course," the first biology he remembered studying. The Yale class of 1943 had an accelerated senior year, and Jim graduated with honors in December 1942.

PHYSICIAN (1943-1950)

Jim's lifestyle at Columbia University's College of Physicians and Surgeons was somewhat unusual because he was married and commuted to classes from an apartment on West 72nd Street in Manhattan. Due to the war, medical school was compressed into three years, leaving little time for friends or extracurricular interests. All students wore military uniforms, reported to a local military post weekly, and were obligated to spend two years on active duty after medical training. Jim's favorite pastimes of bridge and crossword puzzles had to be satisfied while standing in the cafeteria line. Bonta Hiscoe, his "cadaver-mate," described Jim as quiet, friendly, and businesslike, always prepared and helpful to classmates, particularly in biochemistry labs. From the beginning his major interests were infectious diseases and pediatrics, although he was inclined toward laboratory research.

An internship with W. Barry Wood, Jr., at Barnes Hospital in St. Louis in 1946 was a major turning point in Jim's career. Wood was a legendary figure, having been All-American quarterback and a member of the Davis Cup tennis team. At age 32 he became a professor and chairman of medicine at Washington University, where he had a research program focused on white blood cells and mechanisms of host defense. He gave Jim 24 hours to accept an internship in his small department of medicine; Jim accepted with pleasure, and the next few years served to solidify his research interests.

Wood was a proponent as well as practitioner of a combined clinical and research career. In an unusual arrangement Wood spent six months a year on clinical and administrative duties and the other six months on full-time research. According to Wood's associates Robert Glaser and Thomas Hunter, Wood attracted many bright young physicians into science. A philosophy that Wood passed on to his staff was that "if you know what's going on, then you can do something to help." And he believed that to be a stimulating teacher "you must do research—be creative at the cutting face of the subject and be in clinical medicine."

Jim flourished in this creative environment at Barnes Hospital. On the clinical side Stanford Kroopf, an assistant resident in 1946, remembers that Jim "always cared a great deal about his patients. He had a wonderful touch with people and was just the kind of physician I would want for myself." Jim took his work and patients seriously, something that was bound to cause pain. Once he asked his mother to bake a birthday cake for a sick child, but when this patient died, he told his mother that he would not practice medicine because he could not stand to lose a patient. What emerged was a new motivation to find cures rather than to treat symptoms. On the research side Jim asked Thomas Hunter, then assistant professor of medicine, whether he could try some experiments in his laboratory. Hunter never learned the nature of Jim's project but saw that he could learn lab techniques quickly and always washed his own glassware. Jim initially planned to specialize in endocrinology, apparently because of his concern for a maternal uncle suffering from Klinefelter's syndrome. However, Wood influenced his shift to infectious diseases.

Entering active military duty in 1948, Jim was assigned to Warren Air Force Base in Cheyenne, Wyoming. The base had extensive medical facilities, including a pediatrics section. It also housed the nonmilitary research facilities of the Streptococcal Disease Laboratory (SDL) of the Armed Forces Epidemiological Board under Charles H. Rammelkamp, Jr. Taking advantage of the high incidence of streptococcal infections in this Rocky Mountain area, the SDL was carrying out clinical studies of environmental, bacterial, and host factors to understand the relation of these infections to the development of rheumatic fever. While SDL performed laboratory tests, Jim was the chief of medicine who coordinated the base's hospital patients with SDL protocols, which called for equally large groups of penicillin- and nonpenicillin-treated patients. On the basis of these clinical studies Rammelkamp's group confirmed that serious heart damage of rheumatic fever could be prevented by treating primary strep throat and tonsil infections with penicillin.

Jim's clinical observations on acute rheumatic fever in this patient population resulted in his first two publications. With David Flett he reported that absolute bed rest was not necessary and that aspirin (no antibiotics) could relieve symptoms, but this therapy neither shortened the course of disease nor prevented valvular heart disease. These observations were the prelude to his 1957 clinical studies of bed rest and drug therapy in pulmonary tuberculosis.

Although Jim worked as a clinician at the Warren hospital, he was exposed to scientific research carried out by bacteriologists and immunologists. In particular he became acquainted with three consultants to the SDL project—Oswald Avery, Colin MacLeod, and Maclyn McCarty, whose research on the pneumococcus led to the 1944 discovery that DNA is the genetic material. These men worked in the place where Jim would soon begin his own research career—the Rockefeller Institute for Medical Research.

SCIENTIST (1950-1981)

On Jim's discharge from the Air Force, Barry Wood suggested that he get laboratory experience with René Dubos. This French-born bacteriologist had joined the Rockefeller Institute laboratory of Oswald Avery in 1927 and had become head of his own laboratory in 1940 after isolating antibiotics from soil bacteria. Following the death of his wife from tuberculosis, Dubos spent two years on the faculty at Harvard Medical School and returned to Rockefeller in 1944 to study tuberculosis. Dubos embraced an ecological approach to medical science and was studying interactions among living organisms as well as environmental influences on infection and disease. In 1950 when Jim began his research, Dubos and his colleagues were studying factors that govern the growth of tubercle bacilli in vivo.

A standard practice of Dubos, whose early fame caused him to be besieged by young postdocs, was to reject all first inquiries regarding laboratory openings. Hirsch's application was no exception. Only after writing two letters, then boldly traveling to see Dubos in person, was Jim offered a position and awarded a National Research Council Fellowship. In 1960 when he became a member, professor, and senior physician, he established his own laboratory of cellular immunology. He remained close to Dubos, with an office just a few steps away. Both relished their proximity and their daily conversations. Those of us who worked in this special fourth-floor laboratory of the Bronk building had the great good fortune to share many of these heady exchanges.

The scientific environment Jim encountered was not unlike the one Dubos found on entering Avery's laboratory. There was no formal indoctrination or training. Newcomers were left to find their own way, causing them to complain about a "cold shoulder treatment." Dubos's purpose, however, was to create an environment free of constraints that would foster investigators and not mere problem solvers. He did not assign problems to investigate but left newcomers to find a project suited to their own taste and gifts. The atmosphere required secure people with strong inner direction. In Jim's case the first six months were seriously discouraging and he considered returning to the practice of medicine. Several good breaks during the next six months led to his first scientific paper at Rockefeller, coauthored with Dubos, on the antibacterial effect of spermine on tubercle bacilli.

In an unpublished 1974 interview Jim reflected on the experience and intellect needed to be a good scientist.

> If you're going to do work at the basic level, ninety-nine days out of a hundred are going to have frustration and disappointment. Only two or three times a year, or maybe a little more than that if you're lucky, will you really run across something. Now that takes a pretty thick skin, because it means you go home most nights and either you haven't accomplished anything or you tear apart what you thought you accomplished the day before or the week before. . . . When the two or three times a year you do find something, and you know it's for real, and it's knowledge that didn't exist before, the kind of gratification you get out of that, qualitatively, is entirely different than the gratification you get out of performing a service for somebody, which is really what medicine is.

The discovery that won Jim to research was isolating substances from tissues that hinder the growth of the tubercle bacillus. In little over a year he published four papers describing the isolation of a crystalline substance from kidney that he identified as spermine. He then showed that a protein had to be present in the medium for spermine to exert its activity against the bacillus, and identified this as a new amine oxidase specific for spermine and spermidine. He next isolated a group of cationic peptides from thymus that were potent antimycobacterial agents and described their mode of action and limitations.

By the mid-1950s Jim decided he had "never been a microbiologist from the point of view of being interested in the germs. I would much rather study the host, because to me it's more interesting." He turned from antibacterial substances in tissues to active killers of bacteria in the body, the phagocytic white blood cells. At the time the study of leukocytes was in its infancy and hematology was ruled by the erythrocyte. Barry Wood provided some precedents with his important discoveries of surface phagocytosis. Other individuals in the Dubos laboratory were also influential: Samuel Martin, Gardner Middlebrook, Merrill Chase, and Emanuel Suter were studying various effects of tubercle bacilli on white cells. In 1954 David Rogers, a visiting investigator, was studying how staphylococci survive in human leukocytes. That same year, Dubos devoted a chapter of his monograph *Biochemical Determinants of Microbial Diseases* to the fate of microbes during inflammation and he suggested experimental problems that posed novel and alluring challenges.

Jim began by studying the most easily collected white cells, the polymorphonuclear leukocytes (PMNs), to see whether something could be extracted from them that would kill bacteria. He first developed methods for obtaining large quantities from the rabbit peritoneum, primarily neutrophilic granulocytes. In 1956 he published a classic study of a bactericide obtained from an acid extract of these leukocytes, which he termed phagocytin, and characterized its effects on Gram-positive and Gram-negative bacteria. He also looked briefly at the antibacterial activity of hemoglobin and histones. Although there was no further work on phagocytin, this discovery set off a chain of searches for natural host antibiotics, including what are today called defensins. Phagocytin marked a new beginning on the biology and function of phagocytic cells.

In 1957 Zanvil Cohn joined Dubos's laboratory and within three years he and Hirsch found that phagocytin and other cationic polypeptides were localized in cytoplasmic granules of leukocytes. Using new techniques in cell biology that were being pioneered at Rockefeller by Albert Claude, Keith Porter, and George Palade, Jim and Zan isolated a morphologically homogeneous population of granules by differential centrifugation. The granules of the phagocyte, an object of tinctorial delight for decades, were now a cell biological entity, analogous to the lysosomes identified biochemically in 1955 by Christian de Duve and his colleagues in Belgium. In 1960, the year that Jim became a full professor, he and Zan published three historic papers on this work in the *Journal of Experimental Medicine*, and thereby placed lysosomes in the dynamic context of intact cells.

Hirsch and Cohn next worked out methods to prove that PMNs degranulated during phagocytosis and that degranulation took place only in cells with ingested organisms. Jim produced elegant motion pictures to visualize this process. An ultimate perfectionist, he labored long and patiently to tell the story of phagocytosis in graphic detail. Using a phase contrast microscope, he captured live leukocytes in the process of engulfing bacteria and discharging lysosomes during phagocytosis. These films, which remain an ideal component for many courses in biology, allowed them to propose that granule lysis played a key role in the destruction of microorganisms.

The motion picture evidence also suggested that a membrane around the granule fused with the membrane around the vacuole containing the microbe, allowing the granule contents to be discharged into the vacuole and not into the cytoplasm of the cell. Yet, details of the granules as they broke could not be seen because they were at the limit of resolution of the light microscope. Jim then arranged to buy an electron microscope, which at the time took more than a year for delivery. Meanwhile, he teamed up with hematologist Dorothea Zucker-Franklin at New York University, and together they produced seminal micrographs of the process of phagocytosis. This was accomplished by feeding zymosan particles to PMNs, stopping the process with osmic acid at various intervals to freeze the cells' action, fixing the cells in an epoxy resin block, slicing them with a microtome, and placing slices on electron microscope grids. After studying hundreds of slices and taking even more micrographs, Hirsch and Zucker-Franklin visualized how an invading organism is engulfed by the white cell's outer membrane and then fuses with a similar membrane around the lysosomes, and how the granules in this phagolysosome, or digestive compartment, fire with explosive force onto the germ. For the first time this secret in the white cell was revealed for all to see and understand. An important link between the cell biology of endocytosis and host resistance had been made.

During these early years there was also time to consider two clinical problems. In a 1957 study with Russell Schaedler, Cynthia Pierce, and Ian Smith, Jim evaluated the longstanding treatment of tuberculosis with complete bed rest. They tested alternating periods of bed rest and physical activity on patients who were also treated with the new antibiotic therapy. Their forceful conclusion was that "bed rest is a potentially harmful treatment . . . as dangerous and unjustified as the use of a potentially toxic drug." Unexpectedly they observed one of the earliest incidences of multidrug resistance to tuberculosis. Two of 23 patients were eliminated because of "triple drug resistance" to streptomycin, isoniazid, and PAS; five others were resistant to one drug; and one developed resistance during the study. Another decade-long study in a large group of patients evaluated the pathogenesis of the still puzzling sarcoidosis, its treatment with chloroquine, and the value of the Kveim skin test for diagnosis.

With a succession of postdoctoral and graduate fellows through the late 1960s and 1970s, seminal contributions were added to leukocyte biology. Eosinophils, basophils, mast cells, monocytes, macrophages, and lymphocytes entered Jim's sphere of interest. Studies on PMNs with Ralph Nachman and Marco Baggiolini led to isolation of primary (azurophil) and of secondary (specific) granules. Membranes were also prepared from these purified granule populations and differences demonstrated in their protein content. Further work with Earl Parr on the mechanisms of contraceptive action of intrauterine devices revealed that copper in these devices exerted their antifertility effect by evoking a chronic local inflammatory response by the PMNs. With Sally Zigmond, investigations on the effects of cytochalasin B demonstrated that this agent blocked rapidly and reversibly glucose transport into PMNs. In other, now classic studies with Zigmond, methods were devised to visualize and measure locomotion and chemotaxis of PMNs and to distinguish between these activities.

In 1966, when Zan Cohn became a full professor, he and Jim formed a joint laboratory named Cellular Physiology and Immunology. Zan began independent studies of the cultured mouse peritoneal macrophage as a model for further pioneering studies in cell biology. With the arrival of an electron microscope in the lab, Martha Fedorko, a hematologist and investigator trained in using this still new technology, joined Hirsch and Cohn to produce clear views of macrophage lysosomes as well as their derivation. Another series of motion pictures on pinocytosis was made showing the formation and movement of vesicles in the process of ingesting fluid and solutes in macrophages.

Jim also joined Zan and Ralph van Furth to help identify the blood monocyte as the precursor for tissue macrophages and bone marrow as the source of monocytes. Later Hirsch, van Furth, and Fedorko did electron microscope studies on bone marrow colonies containing these precursors and described their production capacities and kinetics. These experiments led to a redefinition of the reticuloendothelial system, using cell biological criteria to identify the mononuclear phagocytes and their endocytic pathway as the source of clearing colloids, organisms, and antigens.

In studies of the monocyte Zan and Jim devised conditions under which monocytes would undergo differentiation to macrophages when cultured in vitro. They were able to visualize the formation of pinocytic vesicles, movement into the cell center, and fusion with lysosome granules.

The role of macrophages in the development of antibody responses in culture was studied with graduate fellow Chang Chen. They found that macrophages promoted the viability of lymphocytes and could be replaced by mercaptoethanol. However, the mercaptoethanol-supplemented cultures were on occasion unable to make antibody, implying that some cell other than a macrophage was required. This helped set the stage for Ralph Steinman and Zanvil Cohn and their discovery of dendritic cells as essential accessories for the antibody response.

Continuing earlier studies on interactions between phagocytic cells and infectious agents, Thomas Jones and Hirsch devised a model system using *Toxoplasma gondii* to study its entry into the phagocyte, its intracellular localization, and its evasion from cellular attack. Of particular interest, *Toxoplasma* remained and grew within the phagosome compartment, thus avoiding fusion with lysosomal vacuoles for killing.

Author of more than a hundred scientific articles, Jim also served as editor of the *Journal of Experimental Medicine* from 1973 to 1981. He held other editorial appointments on the *Journal of Bacteriology* (1964-70), *Blood* (1967-73), *Journal of Infectious Diseases* (1968-72), and *Cellular Immunology* (1969-83). With René Dubos he coedited the fourth edition of the influential textbook *Bacterial and Mycotic Infections of Man* (J.B. Lippincott, 1965).

Although there was no formal teaching at the new

Rockefeller University, Jim was a highly respected teacher in the laboratory and in public lectures. Two memorable occasions were his Christmas lectures for high-school students in 1964, and then with Zan Cohn in 1970. The topic was his beloved white cells, and he delighted in presenting the history of his role models Elie Metchnikoff and Paul Ehrlich, as well as his sophisticated electron micrographs and superb motion pictures of white cells in action. Phagocytes as large as the screen engulfing bacteria caused the young audiences to marvel. Jim told the students that he studied cell biology rather than molecular biology because "many phenomena must be studied in terms of the organized operations of whole cells and living organisms." The 1970 lectures were given after Don Herbert, known as Mr. Wizard, created a television science program featuring Jim and Zan at work in the laboratory. A book *Secret in the White Cell* written by Herbert and Fulvio Bardossi was based on this television documentary (Harper & Row, 1969).

Hirsch was recognized for his achievements by election to the National Academy of Sciences in 1972 and to the Institute of Medicine in 1974. He was appointed to many committees and was chair of the Medical Sciences section of the National Academy of Sciences and chair of the Assembly of Life Sciences of the National Research Council. It was easy to be impressed with the low-key manner in which he expressed his views and succeeded in conciliating highly divergent opinions of others.

DEAN (1972-1980)

Jim's work in the laboratory was greatly curtailed in 1972, when Rockefeller president Frederick Seitz appointed him dean of graduate studies. The student program, conceived in 1953 by president Detlev Wulf Bronk, had transformed the Rockefeller Institute for Medical Research into the Rockefeller University. This program was distinctive for its intensive ratio of one student to five faculty members and the fact that students were given stipends plus research and housing support—everything that Bronk believed provided them with the freedom necessary for the progress of scientific thought. Instead of a fixed curriculum, grades, exams, and course credits, students advanced through their scholarly accomplishments and not through classroom observations. New on the campus in 1972 when Jim took over from Frank Brink, Jr., was the first class of five M.D.-Ph.D. students.

Jim was uniquely suited to be dean, having mastered Rockefeller's traditional postdoctoral apprenticeship under Dubos and then having trained both postdoctoral fellows and graduate students for 15 years in his own laboratory. In particular he enjoyed nurturing the oncoming generation of scientists in the basic disciplines of medical science. His commitment was exemplified by "the question." To every aspiring Ph.D. candidate Jim's first question was, "What are the four functions of the liver?" He was frequently bothered that too many able, sophisticated young biologists could not give an adequate account of the function of a major organ. John Bruer, a Rockefeller graduate and later Jim's colleague at the Macy Foundation, said Jim strongly believed that "if we did not have passable knowledge of physiology and pathology, we could not see the relevance of basic biology to clinical problems."

His tenure as dean brought a firm but kindly administrative hand in selecting 15 graduate and 5 biomedical fellows each year. This task was something on which he, the associate deans, and his assistant Beate Kaleschke Fried spent six months a year. (In the mid-1970s Jim and Marjorie divorced, and he married Beate, who had been his laboratory technician before bringing her fine organizational skills to the Dean's Office.)

Jim interacted dynamically with every one of his "junior colleagues," a name he preferred because it conveyed his aim in fostering research careers. He made time to personalize relationships. Once, when a new student asked Jim for help to get his wife a job, Jim interviewed her and found she was equally qualified to be a student; the next day she was also enrolled in the program. According to Clarence M. Connelly, his successor as dean, Jim had a gift for tactfully solving clashes between brittle students and short-fused scientists. He interceded, mediated, and smoothly integrated the trying academic requirements into traditionalist research laboratories. Two activities he initiated were the Student Journal Club and Student Representative Committee. When necessary, he even looked after moving furniture and removing marijuana plants from the Graduate Student Residence. On graduation day Jim always remembered to thank the parents for sending their sons and daughters to Rockefeller.

PHILANTHROPIST (1981-1987)

After eight years Jim wrote to president Joshua Lederberg that he wanted to pursue new directions. In particular he wanted to write a biography of German immunologist Paul Ehrlich, whose copybooks and correspondence had recently been given to the university archives by Ehrlich's grandson, Günther Schwerin. Jim turned down several other opportunities and sold his house overlooking a Long Island salt marsh and beloved wooden motorboat *Bluenose*. He made plans to move to Florida to pursue this project with his German-born wife, Beate, while enjoying a quiet private life with their young daughter Rebecca.

Within the year the Josiah Macy, Jr., Foundation asked him to be its president, and he embraced the offer. He saw it as *the* opportunity in the Rockefeller tradition, so strongly influenced by Dubos and Bronk, to mobilize scientific knowledge for the benefit of humanity.

Not surprisingly, medical education was the primary theme of Jim's philanthropy at the Macy Foundation. He expanded their commitment to increase participation of minorities and women in medicine and biomedical research, thus affirming what he had practiced in staffing his own laboratory. Specifically, he redirected the program to support high-school rather than medical-school or college projects. A cognitive science program was initiated to reassess the content and method of medical education using computers and artificial intelligence. His concern with inadequate answers to "the question" and to a decreasing number of physicians entering research became the focus of a pathobiology program to provide Ph.D.s in science with intensive background and expertise in human disease. It was time, he said, "to pay more heed to morbidity rather than mortality, time to devote more of our resources to improving the quality of life, rather than merely prolonging it."

Another mission was to reach an informed public about the excitement of science and ideas. He often wondered why people were not awestruck by the complexity and beauty of living things and why they were not fascinated by knowledge of how their bodies work. In his 1984 president's message he remarked: "The listener and viewer of radio and television is exposed regularly to crimes, baseball scores, even the price of gold in London; but seldom does one encounter a well-done report on science and medicine." To this end he introduced a fellowship program to support science broadcast journalism.

During this period Jim also served as a valued trustee on the boards of the Trudeau Institute in Saranac Lake, Irvington House Institute, and his alma mater Yale University, where an endowed fellowship in his name supports medical students pursuing research.

The final years were happily spent as a science historian. Jim wrote National Academy of Sciences *Biographical Memoirs* of his mentors Barry Wood (51[1980]:386-418) and René Dubos (with Carol Moberg 58[1989]:132-161). He and Beate finished translating into English hundreds of Ehrlich's letters and documents, and they published an article on Ehrlich's discovery of the eosinophil. They had just begun to organize materials for a biography when both of them were diagnosed with cancer. Jim died of a brain tumor on May 25, 1987. Beate died of breast cancer on what would have been Jim's seventy-first birthday, October 31, 1993. He is survived by two children, Ann and Henry, from his marriage to Marjorie, who died February 17, 2002, and his daughter Rebecca, from his marriage to Beate.

According to Zanvil Cohn, Jim Hirsch "laid the groundwork for all that was to follow" in the field of cellular immunology. "Although Jim's life was devoted to research," he added, "he was indelibly imprinted by his training as a physician. His life and work typify the noble tradition of the physician-scientist."

Many friends, colleagues, and family members of James Hirsch contributed to this memoir: Fulvio Bardossi, Robert Bortle, John Bruer, Zanvil Cohn, Clarence M. Connelly, Osborne Day, James Ellis, Marie Flett, Charles Frankenhoff, Robert Glaser, Don Herbert, Beate Hirsch, Phil Hirsch, Bonta Hiscoe, Thomas Hunter, Stanford Kroopf, Maclyn McCarty, Sara Schiffman, Dolores Schucart, John Sheley, and Clifford Tepper. We are grateful to Seymour Klebanoff, Sally Zigmond, and Carl Nathan for constructive comments on the manuscript.

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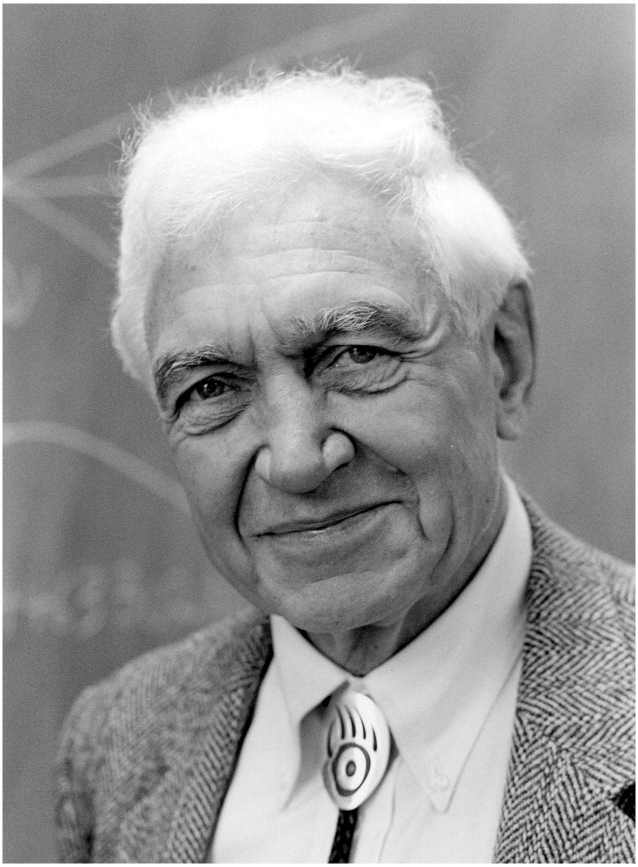

VERNON WILLARD HUGHES

May 28, 1921–March 25, 2003

BY ROBERT K. ADAIR

VERNON WILLARD HUGHES, Sterling Professor Emeritus and senior research associate at Yale University, died on March 25, 2003, at the age of 81 at Yale-New Haven Hospital from medical complications after an operation for an aneurysm. Hughes began research in physics in 1942 when he worked on radar at the MIT Radiation Laboratory. During his terminal stay in the hospital, he wrote letters of recommendation for a postdoc working with him on a major experiment. Thus, Hughes worked at physics research—largely at the cutting edge of atomic, nuclear, and elementary particle physics—for 61 years.

Born in Kankakee, Illinois, on May 28, 1921, Vernon Hughes was raised in the Morningside Heights sector of New York City by his mother, Jean Parr Hughes, who was a librarian at Teachers College of Columbia University. His father, Willard Vernon Hughes, died when Vernon was three years old. As a New York City boy of his time Hughes played stickball in the streets—he later told his sons Gareth and Emlyn that he had been a very good stickball player—and he played tennis on the local Columbia University courts. His sons recounted with amused affection that when he played tennis against them in later years, he gave no quarter.

Entering Columbia in 1938 as a freshman pre-law student with the intention, he wrote, "of doing good things for the world," Hughes took enough time from his studies to play on the tennis team and work on the school newspaper, *Columbia Spector*. He wrote later that while he found some of the Columbia core courses in humanities and contemporary civilization valuable, the mathematics and physics courses were more interesting to him, and he decided to direct his efforts toward mathematics, physics, and engineering. Growing up in modest circumstances during the Great Depression years of the 1930s, he tells of his concerns about a choice of schooling that would result in a job after college.

After completing an especially heavy academic schedule with excellent grades in only three years and winning the Van Buren Math Prize, he graduated from Columbia as a physics major in the spring of 1941 just after his twentieth birthday. Looking for a new environment, Hughes enrolled as a graduate student at Caltech that fall. A picture from that time shows Hughes atop Mt. San Gorgonio in California along with fellow students Pief Panofsky, Bill Eberhardt, and Ed Deeds.

Hughes writes that at Caltech he found Smythe's course in electricity and magnetism, which consisted largely of blackboard presentations by the students of problems assigned from Smythe's book, especially challenging, but he wrote that the acceptance of his work was helped by his "being an acceptable tennis partner for Professor Smythe."

After receiving his M.Sc. degree from Caltech in 1942, with the country at war, Hughes went back east to work on radar at the MIT Radiation Laboratory. Here he joined a group directed by Burton Chance that was especially concerned with accurate time measurements—at the microsecond level—of the reflected radar pulse and thus target range information. He was later coauthor with Chance and others of the Radiation Laboratory volume titled *Waveforms* (1949).

After World War II, in January 1946, Hughes returned to Columbia for graduate study with the goal, he writes, of doing his thesis work with I. I. Rabi. While he found the theoretical work of Yukawa on nonlocal field theories and Selig Hecht's biophysical investigations of vision interesting, he settled on a research topic in molecular beams with Rabi. A decade later Rabi wrote that Hughes "was one of the best students I ever had." With molecular-beam electronic-resonance apparatus that he "built from scratch" Hughes and Lou Grabner measured nuclear electric quadrupole interactions. His thesis for the Ph.D. degree he received in 1950 was based on those measurements. In the course of that work he and Grabner discovered the first clear two-photon transition in their molecular beam electric resonance studies, and Hughes worked out the theory. Hughes commented later that "at that time at Columbia the theoretical course work was extensive and one was expected to handle the theory relevant to one's experiment."

After receiving his Ph.D. in the fall of 1950 Vernon Hughes married Inge Michaelson. German-born Inge had left Germany in 1938 with her family as refugees from the Nazi racial laws. Vernon had met Inge when she was a student in a summer class in mathematics he taught. Inge, who had just graduated from Barnard, took the course in preparation for graduate work in biology. Later Inge received her Ph.D. in biology from the University of Pennsylvania. Their son Gareth was born in 1955 and their son Emlyn in 1960.

After Inge's death in 1979 Hughes married Miriam Kartch, who teaches at the Mannes College of Music. He had known Miriam when he was a student at Columbia. They first met in 1947 when Miriam was assigned as the teacher when the

26-year-old Hughes enrolled as a beginning piano student. He is survived by Miriam, his sons, and four grandchildren.

During a two-year period as a postdoc at Columbia, Hughes, always deeply interested in fundamental matters, found by measuring deflections in an atomic beam that the electron-proton charge difference, $|q_e - q_p| < 10^{-13}\, e$, and the neutron charge, $|q_n| < 10^{-13}\, e$, supporting the view that e was indeed a fundamental quantity. Thirty years later, with his students, he reduced the limits by a factor of 10^6 using atomic measures of these quantities though bulk measures on the charge of gases were by then somewhat more sensitive. In 1953 Hughes left Columbia for a position at the University of Pennsylvania and then joined the Yale faculty in 1955.

While much of Hughes's work was on the properties of atoms, he regarded atoms primarily as laboratories for the study of the fundamentals of electromagnetism and preferred to consider simple atoms "where the theory was adequate." Thus he concentrated on studies of the helium atom, on the electron-positron atom, positronium, the simplest of atoms, and then—arguably his most nearly unique contribution to physics—on muonium, the atom made up of a muon and an electron.

His extensive work on helium from 1950 to 1980, largely using atomic beam methods where Hughes and his colleagues did the experiments and Hughes the theoretical calculations, provided rigorous tests of modern quantum electrodynamics (QED) for two-electron systems and a precise value of α, the fundamentally important dimensionless ratio of the square of the electric charge to the product of Planck's constant and the velocity of light.

With Martin Deutsch discovered the electron-positron atom, positronium, in 1952, Hughes began studies of that simplest of atoms with C. S. Wu, again emphasizing connections with QED. At Columbia, with Wu, he made a measurement of Δv, the interval between the states 1^3S_1 to 1^1S_0, that was somewhat more precise than Deutsch's pioneering measurement. His later high-precision measurements at Yale gave a result about three standard deviations from theory, a discrepancy that has not yet been resolved.

After the discovery of parity nonconservation by Wu, Ambler, and others, Hughes with Jack Greenberg measured the longitudinal polarization of the electrons emitted from Co^{60} as a function of their momentum using Mott scattering as an analyzer and found that the polarization was accurately proportional to the electron velocity, β, a result in accord with the Yang-Lee model of weak interactions. Hughes noted that it was his work on this experiment that kindled his interest over two decades in the design of polarized electron beams.

He focused his efforts to create a polarized electron source on the photoionization of a polarized beam of alkali atoms, especially ^{39}K and ^{6}Li. Ten years of work culminated in 1972 with a source that produced a 1.5 μs pulse of a 20 μA current of polarized electrons. That source was then used to produce a high-energy polarized electron beam at the Stanford linear accelerator.

About 1960, elaborating on a suggestion by Cocconi and Salpeter, Hughes used nuclear magnetic resonance methods to study the isotropy of mass. Mach considered that the mass of an object should derive from the distribution of distant matter—far-off galaxies—and thus its inertial mass might be slightly different when accelerated in different directions even as the mass of the universe might be slightly anisotropic. Hughes set that anisotropy for the $p_{3/2}$ proton outside of the closed shell in the lithium nucleus as $\Delta m/m < 10^{-22}$, which from Mach's principle meant that the Universe was isotropic to about one part in 10^{22}.

Hughes with his colleagues McColm, Prepost, and Ziock "discovered" the electron-muon atom named muonium, *M*, in 1960 by observing its characteristic Larmor precession frequency. His following 40 years of experimentation on that atom, concentrating on ever more accurate measurements of the 1^3S_1 to 1^1S_0 interval, $\Delta\nu$, the Lamb shift in the $n=2$ state, and the 1S-2S transition, verified to high precision that the muon is indeed a "heavy electron," gave us new avenues into the experimental study of quantum electrodynamics and created a tool to probe the highest energy scales of elementary particle physics. Aside from this "conventional" physics Hughes established important limits on the muonium-antimuonium transition rate, again testing fundamental concepts.

In 1967 Gisbert Zu Putlitz, later rector at Heidelberg, who had just completed his Ph.D. at Heidelberg came to Yale and worked with Hughes for nearly two years before returning to Germany. Hughes wrote that their collaboration, largely on muonium physics, and their friendship and personal association, which continued until Hughes's death 35 years later, was "among the better experiences in my life." Hughes also wrote of the importance of his close association with Val Telegdi to the muon work that engaged them both in the decade beginning in the late 1960s.

In his atomic physics experiments Hughes worked unceasingly to increase the accuracy of his measurements. Quantities such as the magnetic moment of electrons and muons stem primarily from the elementary electric charges of these elements as observed statically. Then there are "corrections" that represent effects at very small distances or complementarily at very high energies. These modifications to the static result connect the simple leptons with all other particles, including the strongly interacting quarks. Thus, the measurements of very small corrections to the simple model of leptons approaches very much the same kind of physics as cruder measurements of the interactions of the leptons at very high energies. Hughes looked in both directions.

A colleague famous for an important breakthrough once said admiringly that Vernon Hughes was the only physicist he ever knew who would mount an experiment to improve the precision of some fundamental measure by a factor of two. But Hughes's attack went on unceasingly, with improvements by two, and two, and two, and two, which added up to new insights.

As early as 1958, Hughes, along with Wheeler, Beringer, and Gluckstern at Yale, began a serious design study of a proton linear accelerator "meson factory." This machine was meant to place a 1 mA beam of 800 MeV protons on a target to produce meson and muon fluxes a thousand times greater than those then available. Such a meson factory, the Los Alamos Meson Physics Facility, based largely on the Yale design was built, but not at Yale. (Hughes later wrote that Rabi advised him: "If you can't beat them, join 'em. . . . Its a wonderful place to spend the summers with your family." Indeed Hughes did start going to Los Alamos in the mid-1960s to help develop the muon facility there.)

Those summers were wonderful and led to Hughes's longtime friendship with the San Idlefonso Indians neighboring Los Alamos and a special appreciation of their remarkable art—especially their pottery and sand paintings. With his wife, Inge (and after Inge's death and his remarriage, with Miriam), Hughes amassed a collection of that art that gave the comfortable living rooms of their home in New Haven some of the ambience of a charming museum set.

Hughes's close association with Los Alamos and the Los Alamos Meson Physics Facility (LAMPF) continued until

1996, when LAMPF was shut down. During that time his major program directed toward the properties of muonic atoms was conducted there. In particular, Hughes's group measured Δv and the ratio of the magnetic moments of the muon and proton, μ_μ/μ_p, with ever increasing accuracy. As he did always and everywhere, Hughes worked intensively at LAMPF. In 1988 the director of LAMPF wrote an unsolicited letter to the Yale department chair saying that Hughes, then 67 years old, "was still absorbed in physics, indifferent to fashion, and is a true inspiration to younger scientists. . . . He insists on taking the 4:00 AM shift [the experiments on accelerators always ran three shifts, 24 hours a day, every day] so that he can still put in a full working day in addition to taking a shift."

Hughes's development of polarized electron ion sources was fundamental to the use of polarized electrons in high-energy accelerators beginning in 1963, when he developed the first polarized source for the Stanford two-mile accelerator. That vision led to the observation of parity nonconservation in deep inelastic electron scattering and in electron-positron scattering and to measurements of the spin-dependent structure of the proton.

After the groundbreaking Stanford linear accelerator experiment led by Friedman, Kendall, and Taylor identified partons, with quark-like properties, as point-like physical constituents of nucleons, Hughes became deeply interested in measuring the spin-dependent structure of the proton. Thus, he began in 1972, with Peter Schüler, Kunita Kondo and his group from the University of Tsukuba, and a Stanford linear accelerator (SLAC) group led by Dave Coward, measurements of the deep scattering of polarized electrons by polarized protons. The large asymmetries—electrons and protons with their spins opposed scattered more than those with their spins parallel—that were measured over the next five years supported the general quark-parton model of the nucleon.

Following a suggestion by Charles Prescott, the group also investigated the parity-violating scattering of longitudinally polarized electrons by unpolarized protons. Initial measurements, using the Yale polarized electron source, showed no effects at a sensitivity level of 10^{-3}. However, with the development at SLAC of a higher-intensity polarized electron source, the group did see effects at a level of 10^{-5} that were in accord with the electro-weak theory of Glashow, Salam, and Weinberg.

Hughes had hoped to continue work with polarized electrons and protons at SLAC and his initial proposals were received positively. SLAC decided, however, to go in other directions at that time. His disappointment was ameliorated somewhat when 10 years later, SLAC resumed measurements of polarized electron scattering with a very successful program led by Vernon's son, Emlyn Hughes (now a professor at Caltech).

In spite of the termination of the SLAC program Hughes was still deeply interested in polarized lepton-nucleon scattering; he was therefore pleased to accept an invitation by Erwin Gabathuler of Liverpool for the Yale group to join the European Muon Collaboration (EMC) at the European Organization for Nuclear Research (CERN). The collaboration had previously discovered a change in the nuclear structure function when the nucleon is in a nucleus (the EMC effect) and most of the EMC collaborators were much more interested in that effect than in polarized muon-nucleon scattering. With the Liverpool group and the Lancaster group lead by Sloan, Hughes with his Yale collaborators directed a portion of EMC efforts toward polarized muon-nucleon scattering in a kinematic region that had not been explored by the previous SLAC experiments.

The results of that work were very interesting. It was known that the spin of the nucleons was generated by the spins of the constituent quarks together with the orbital angular momentum of the neutral charged gluons coupled to the quarks. The charged muons interacted only with the charged quarks and thus measured the portion of the nucleon spin held by the quarks. That portion turned out to be much lower than expected (by the Ellis-Jaffe sum rule); the quarks carry only a small portion of the nucleon spin—a result called the "spin crisis" or "spin puzzle."

After this result a new group was formed in 1987 (the spin muon collaboration), with Vernon Hughes elected as spokesperson. That group, with about 150 physicists from European, American, and Japanese institutes, as well as strong internal CERN contributions, began taking data in 1992 and continued through 1996. The results were in very good agreement with general QCD (quantum chromodynamics) models (the Bjorken sum rule) but strongly violated the conventional view of the nucleon quark structure (the Ellis-Jaffe sum rule) supporting, with more extensive and more accurate data, the previous spin-puzzle results. Hughes continued to work on designs for more extensive and powerful experiments and was planning a trip to Europe to meet with his collaborators at his death.

Over about the same time span Hughes conceived of and led an experiment to improve the measurement of magnetic moment of the muon by a large factor. The deviation of the magnetic moment, *g*, from the elementary Dirac value of 2, in natural units, thus $(g\text{-}2)_\mu$, serves as a benchmark for the testing of new ideas in particle physics. The precession rate of muons moving in a magnetic field is proportional to the product of the field and $(g\text{-}2)_\mu$. Hence, an accurate measure of the anomalous magnetic moment of the muon, $(g\text{-}2)_\mu$, requires very precise determinations of both the magnetic field and the precession frequency.

A previous major experiment at CERN had established the value of $(g\text{-}2)_\mu$ to the remarkable accuracy of 7 ppm (parts per million). That value was in agreement with the theoretical value—also the product of a massive effort—that was considered accurate to 8 ppm, where much of the error reflected uncertainties in the hadron physics contribution to the moment. Thus, that nominal theoretical uncertainty could be reduced by improved hadron experiments, in particular by improved measurements of the production of hadrons in high-energy positron-electron collisions.

A result as accurate as that reached at CERN, and in agreement with theoretical results calculated assuming conventional physics, already served to exclude many interesting and plausible extensions of that conventional physics. Hughes understood that a significantly better measurement of $(g\text{-}2)_\mu$ could place even more rigorous limits to the character of the extensions of the conventional models of elementary particles that were required and that a better experiment could be conducted at the Brookhaven National Laboratory AGS accelerator, which by 1980 generated beam intensities, and then muon fluxes, superior to that available at CERN.

Hence, beginning in 1982 Hughes began serious studies of methods that might lead to a more accurate measurement. Then in 1984 he began to assemble a group of experienced physicists, many with leading roles in the previous CERN experiment, who were prepared to design and conduct the experiment. Aside from significant contributions from the Brookhaven National Laboratory and CERN, major contributions were made by groups from KEK in Japan and the Budker Institute for Nuclear Physics in Novosibirsk.

A highly accurate measurement made at Novosibirsk of the hadron production cross-section by electron-positron collisions was of major importance, because that measurement served to accurately set the hadron contribution to the anomalous moment and thus significantly reduced the theoretical uncertainty.

By 2002 the collaboration had reached an accuracy of better than 0.7 ppm and the theoretical calculations were accurate to about the same level. The values differed but not quite beyond chance. Somewhat more accurate results were still possible, but as of 2003 government fiscal constraints on Brookhaven physics seemed to have precluded further measurements.

At his death Hughes (at 81) was still playing a major role in two international groups, one working on the large muon $(g\text{-}2)_\mu$ experiment at Brookhaven and another on the design of an experimental program to further study nucleon spin constituents and the problem with those constituents that he had been instrumental in uncovering.

Although he spent his youth in physics very much in the trenches building the equipment for his thesis work "from scratch," his early years at Columbia, Pennsylvania, and Yale usually found him in the laboratory or on the accelerator floor with his hands on his apparatus. In the course of time Hughes found himself occupied more with the tasks of organization and leadership.

Over the decades Hughes had worked on the frontiers of physics, and the complexities of experiments had increased greatly. Along with that increased complexity came increased monetary costs and, sociologically most important, a significant increase in the scientific effort required to conduct an experiment. While Hughes's early experiments involved two, three, and four scientists with a few technicians and typically one or two scientist-years of effort, there are 60 authors on the final Brookhaven paper, representing 11 laboratories from 4 different countries. The paper, describing an effort of more than 100 scientist-years of work, was published 20 years after Hughes had begun working on the problem. And there were 142 authors from 24 institutions from 15 countries on the last SMC publication. With so many participants in experiments that are so complex, the organization of effort is important and only a physicist who is knowledgeable about all experiment details and has the trust and confidence of everyone can exercise leadership. Vernon Hughes was special in his broad knowledge of the experiment and singular in how he held the confidence of his colleagues.

This confidence and special breadth led Hughes into leadership positions. (He was usually a senior spokesman for the experimental groups he worked with.) In those positions Hughes often represented his collaborations in the presentations before laboratory program committees, the committees that effectively accepted or rejected a proposed experiment. With his energy and interests—both deep and broad—he was usually involved in several rather different programs, and the program committees were often concerned with the division of his time; committee members wondered whether Hughes was really going to work personally on the experiment he was advocating. With this concern in mind, when Hughes appeared before a European committee addressing a proposal to support an experiment on deep electron-proton scattering (which would be supported by the U.S. Department of Energy budget for elementary particle physics) and knowing that Hughes had heavy commitments on the $(g\text{-}2)_\mu$ experiment at Brookhaven (also supported by the U.S. Department of Energy elementary particle physics budget), the committee chair asked Hughes what portion of his time would he spend on the experiment he was advocating. Hughes answered, "50 percent," noting that he would spend the other 50 percent on the Brookhaven experiment. One of the committee members then said, "but what about your LAMPF experimental programs at Los Alamos?" Hughes answered, slightly affronted, "But that's nuclear physics!" (LAMPF was supported by the Department of Energy nuclear physics division). But all was well; the committee recognized that all his life Hughes had worked at a 150 percent level.

While his researches in physics took the highest priority, Hughes was ever sensitive to the goals of his youth, to do "good things for the world," and lent his weight and substance to social goals that he found meritorious; thus, Hughes worked hard and effectively on administrative tasks that he found worthwhile.

With the impact of the radar that Hughes worked on at the MIT Radiation Laboratory, which was sometimes said to have *won* World War II, and the neutron chain reaction bombs, which could be said to have *ended* the war, the level of financial support of research in physics and other science at major universities increased so greatly as to change forever those universities. The newly configured institutions became "research universities," with research money from the government that reached a level near or in excess of the instruction budget.

While Yale continued to emphasize undergraduate education (at Yale College) more than many other universities, it had to follow other schools in shifting its institutional priorities sharply toward scientific research and graduate education. With its historic emphasis on the humanities, not science, Yale was not well placed to make that change in general, and not well set in physics, in particular.

In the late 1950s Yale president Whitney Griswold became aware that in an era of great physics, Yale was not playing a major role and asked J. Robert Oppenheimer, then director of the Institute for Advanced Study at Princeton, to review the department and report back to him and the corporation. Oppenheimer's critical report was devastating, perhaps to the point of being unfair. Yale had considerable strength in experimental nuclear physics—with Schultz, Beringer, and Bockelman, and the strong effort in nuclear theory by Gregory Breit—but by 1958 many of the more fundamental questions in nuclear physics had been answered, and nuclear physics itself was sliding behind the frontiers of physics. Though the low-temperature work of C. T. Lane and Henry Fairbank was also first rate, Oppenheimer recognized only the work of Hughes as lying at the cutting edge of physics. Hence, at Oppenheimer's urging Hughes was appointed department chair in 1962 and with special resources from the university was given the task of bringing the Yale department into the first ranks.

Hughes served in that office for the university's statutory limit of two terms, or six years. During that tenure he moved aggressively and effectively, bringing in many new faculty members and new programs while constraining some of the roles of older faculty members. Hughes's changes had consequences: In the decade of 1961-70 the average number of students receiving the Ph.D. in physics per year at Yale rose to about 20, giving Yale then by that measure the country's eighth largest graduate school in physics.

Of course, the changes that Hughes instituted did not come without costs in personal relations. Hughes was perhaps Gregory Breit's best friend among the senior faculty, and they had established a tradition of having lunch together each Saturday at a popular campus restaurant. Greatly displeased by Hughes's actions as chair in taking theoretical physics outside of Breit's personal control and broadening its intellectual base, Breit stopped speaking to Vernon for a time, but as Hughes later told a friend bemusedly, they continued to meet Saturdays for a silent lunch. Hughes's very rare clashes with his peers were always without personal animus on his part. While he strongly disagreed with Breit on department matters, his deep respect for Breit's accomplishments in physics was untouched. In 1999, when Hughes was 78, he organized a symposium at Yale on Gregory Breit's lifework in physics to mark the 100th anniversary of Breit's birth.

Hughes also contributed administratively by serving on the Board of Trustees of Associated Universities, Inc., for more than 40 years, from 1962 until his death. An independent organization created by scientists and administrators from nine northeastern universities, including Yale, Associated Universities established Brookhaven National Laboratory in 1947 and the National Radio Astronomy Observatory 10 years later.

Hughes was elected to the National Academy of Sciences in 1967. In 1978 he was awarded the Davisson-Germer Prize in Atomic Physics and in 1990 the Tom R. Bonner Prize in Nuclear Physics, both from the American Physical Society.

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Photo by Roy Porello

GORDON JAMES FRASER MACDONALD

July 30, 1929–May 14, 2002

BY WALTER MUNK, NAOMI ORESKES, AND RICHARD MULLER

Gordon wrote extensively with force and conviction about his life and work; readers of these biographical memoirs will want to learn in his own words of his successes—and of his failures. This is not an exercise in hagiography; to suppress Gordon's weaknesses would discredit his formidable strengths.

Following a discussion of Gordon's early career in the 1950s, we have chosen to feature two major late twentieth-century issues in which Gordon played a significant role. The first issue, dealing with Gordon's resistance to plate tectonics, is excerpted from "How Mobile Is the Earth?" It is excerpted from an essay he wrote shortly before his death.[1] The second, dealing with Gordon's policy work on weather modification and climate change, is excerpted from a set of articles he wrote between 1968 and 1988.[2] We close with an account of his activities in the 1990s and an attempt to evaluate Gordon's extraordinary accomplishments.

WALTER MUNK WRITING ON YOUNG GORDON

Gordon grew up in San Luis Potosi, Mexico. His father was among the many Scots who emigrated to Mexico; for some years he was an accountant with American Smelting and Refining Company. He met Gordon's mother, an American, while she was working at the U.S. Embassy in Mexico City. As a child Gordon contracted polio, and spent the next 60 years trying to prove to everybody, himself included, that this was not a problem. Gordon's application to Harvard for a football fellowship was a case in point. (He graduated summa cum laude at the age of 20.)

Many years later President Nixon, when questioned about the intellect of his administration, replied, "I have three members of the Harvard class of 1950 on my staff, all summa cum laude." They were Kissinger, Schlesinger, and MacDonald.

After graduation Gordon was among the 20 privileged junior fellows (under Dean McGeorge Bundy, a previous fellow) who were supported to do anything they wished. Gordon spent some of his fellowship climbing in the Alps. On the island of Unst in the Shetlands he stumbled upon a great Arctic skua with a wingspread of 8 feet, and this converted him to bird watching, in fierce competition with Murray Gell-Mann. Gordon himself attributes his subsequent interest in water quality to his early experience in bird life.

Gordon entered Harvard for a degree in chemical engineering, but he switched to geology as an undergraduate, and got his Ph.D. in geology at the age of 25. He then moved across town to an assistant professorship in geophysics at MIT.

I met Gordon while he was a Harvard undergraduate. I was giving a seminar on the variable rotation of the Earth associated with a seasonal change in the high-altitude jet stream (just discovered), feeling reasonably secure that no one in the audience knew anything about this. A student in the first row interrupted with some rude comments about neglect of tides, variable ocean currents, and such like. Four years later I gave a much-improved account at MIT; there he was again in the front row, complaining that I had not answered his questions of four years ago.

We decided to write a book together. The scope grew beyond bounds. To quote from the preface: "The diversity of the subject is appalling. It touches on every branch of geophysics. By the time it is covered, information will have been gained concerning wind and air masses, atmospheric, oceanic and bodily tides, sea level, rigidity and anelasticity of the Earth's mantle, and motion in its core." Quoting Gordon,[1] "Walter and I had a mild debate on whether or not to include discussions of continental drift and polar wandering. I (GmacD) argued we should, so as to tweak the geologists into considering limitations on their wilder speculations. The final chapter of *The Rotation of the Earth* takes up the subject of the Earth's mobility, as we understood it in 1960." I was moved to learn from Naomi Oreskes, who interviewed Gordon on his views of plate tectonics, that Gordon considered his writing of *Rotation of the Earth* as the most satisfying experience in his scientific career.

GORDON MACDONALD IN HIS OWN WORDS

On Plate Tectonics.[3] "In the 1950s, polar wandering and continental drift were controversial subjects, often leading to heated discussions between North American and European geophysicists and geologists. . . . I started serious work on these topics in 1957, when Walter Munk and I began the research and writing for our book, *The Rotation of the Earth*. . . . The final chapter takes up the subject of the earth's mobility, as we understood it in 1960."

"When I was an undergraduate at Harvard in the late 1940s, my professors ignored or dismissed (with ridicule) speculation that continents move relative to each other, the poles tip, and convection currents constantly stir the interior of the earth. However, I was very much impressed in 1949 by reading Reginald Daly's book, *Our Mobile Earth*."

"Whatever sympathy I had for Daly's notion of continental drift was overwhelmed by the work of two giants of 20th century geophysics, Cambridge Professor Sir Harold Jeffreys and Harvard Professor Francis Birch. . . . I found Jeffreys' reasoning about the strength of the earth . . . to be convincing. . . . Elastic materials have what physicists call a 'finite' strength, which means that upon the application of a stress . . . they will deform a certain amount in proportion to that stress. But no matter how long the stress is applied the deformation is limited . . . Birch felt that his demonstration of the homogeneity of the mantle in both the upper and lower regions ruled out large-scale convective motions . . . there would be no driving force for large-scale convection. Based on my readings of Jeffreys and my close interaction with Birch, I concluded that the earth indeed possessed a finite strength."

"In the early 1960s, new observations and interpretations of the sea floor data led to the theory of plate tectonics. According to this theory, low-intensity long-term stresses drive the horizontal motion of the plates. I argued in two papers that the large-scale difference between continents and oceans . . . extended to several hundred miles' depth . . . and that the mantle possesses a finite strength, as argued by Harold Jeffreys. . . . My insistence that geophysical constraints must be discussed led many participants . . . to dismiss me as a troglodyte who was slowing the convergence of thought that was later to be labeled either as a revolution or a paradigm shift."

"In all science there is a strong 'herd instinct.' Members of the herd find congeniality in interacting with other members who hold the same view of the world. . . . Before the 1950s, the North American herd of geologists found it comforting and amusing to ridicule those foreign geologists who advocated continental drift. In the early 1960s . . . (several) respected leaders . . . decided to shift directions and the herd soon followed."

"The Royal Society sponsored (a meeting) March 19-20 1964. Teddy Bullard, a relatively late convert to drift, presented what he regarded as proof that there was a precise fit between the two coasts of Africa and South America. . . . I once again argued for deep roots to continents and the difficulties these imposed on any drift scheme. Teddy Bullard, in a masterful putdown, responded 'Many precedents suggest the un-wisdom of being too sure of conclusions based on supposed properties of imperfectly understood materials in inaccessible regions of the earth.'"

"Although I maintained an interest in the structure of the earth's interior, I had actually begun to disengage from the field of continental drift in 1962, when I was asked to chair a National Academy of Sciences Committee examining weather modification."

On the Science and Politics of Rain Making.[4] "Weather modification was one of many areas in which the federal government, through both its armed forces and its civilian agencies, was funding scientific research aimed at improving our capacity to understand, control, and modify the environment. In 1961, I was appointed to the National Academy of Sciences Committee on Atmospheric Sciences (1961-1970) and the President's Science Advisory Committee Panel on Atmospheric Sciences (1961-1964); three years later I was appointed to the National Science Foundation Advisory Panel for Weather Modification (1964-1967)."

"Weather modification was a highly contested topic, dating back to the establishment in 1953 of the Congressional Advisory Committee on Weather Control. Throughout the 1940s and '50s, there had been considerable enthusiasm for weather modification projects. In the early 1940s, Irving Langmuir and Vincent Schaeffer, at the General Electric Company, demonstrated that clouds could be modified by seeding them with dry ice pellets; Bernard Vonnegut demonstrated that silver iodide could do the same (thus inspiring the ice-9 of his brother, Kurt's, novels)."

"Weather modification was taken up with enthusiasm by those who hoped to use it on behalf of matters ranging from warfare to world hunger. The 1953 advisory committee was charged with evaluating the various government and private initiatives in cloud-seeding. In 1957 this committee reported to President Eisenhower that 'seeding of wintertime storm clouds in mountainous areas in the western U.S. produced an average increase in precipitation of 10-15% from seeded storms, with heavy odds that the increase was not the result of natural variations in the amount of rainfall.'"

"In 1963, the Committee of Atmospheric Sciences of the NAS appointed a Panel on Weather and Climate Modification to 'undertake a deliberate and thoughtful review of the present status of activities in this field and of its potential and limitations for the future.' The report was issued in 1966. The tone was cautious, but the conclusion positive: 'There is increasing but somewhat ambiguous evidence that precipitation from some types of clouds and storm systems can be modestly increased or redistributed by seeding techniques.' Statisticians attacked this conclusion. Alexander

Brownlee of the University of Chicago, writing in the *Journal of the American Statistical Association* (June 1967) had the following closing: 'That such nonsense should appear under the aegis of the National Academy of Sciences is deplorable.' My own conclusion, consistent with the panel report, was that there is no in-principle objection to the possibility of weather modification, and in some meteorological conditions, precipitation reaching the ground can be increased perhaps by a substantial amount by seeding."

"Over the next several years I became increasingly convinced that scientists should be more actively engaged in questions of environmental modification, and that the federal government should have a more organized approach to the problem. While such research could take place in both the public and private sector, the government should take the lead in large-scale field experiments and monitoring, and in establishing appropriate legal frameworks for private initiatives."

On Environmental Sciences.[5] "I felt that changes were needed within the scientific community. The environment was not merely important politically and socially, to my mind, it presented complex and intriguing scientific problems, which the scientific community might be enthusiastic to tackle. Yet they were not. Left to their own decisions, the scientific explorers will push those areas that are exciting or perhaps fashionable. In the past it has been far more acceptable and more praiseworthy to investigate the Earth's deep interior than to puzzle over the problem of predicting earthquakes. In the past, questions of the origin and development of the atmosphere have proved far more attractive than investigations of atmospheric pollution."

"Scientists in the 1960s were generally reluctant to take on society's problems or to allow for the idea that their research should be directed from without. To do so, they felt, would threaten the purity of scientific research. The widely held views of the time were typified by Michael Polanyi, who wrote, 'The pursuit of science can be organized . . . in no other manner than by granting complete independence to all mature scientists. The function of public authority is not to plan research but only to provide the opportunities for its pursuit.' My own view was different: I believed the scientific community needed to find a balance between the pressures from within and without, advancing basic knowledge and translating those advances into tools for society."

"My experiences with weather modification convinced me that the topic could not be isolated from other developments in atmospheric sciences, and indeed, environmental sciences as a whole. The uncertainty over weather modification illustrated our lack of basic scientific understanding in many areas of environmental sciences. Our ability to modify the atmosphere depends on our proficiency in describing and predicting its behavior. Indeed, it would be both ineffective and perhaps unsafe to attempt weather modification in the absence of the capacity to predict the consequences of such activities in some detail. I proposed at the time that the federal government establish a new agency whose task was to promote and foster research and development in environmental prediction and modification—not just of the atmosphere, but also the oceans and the solid earth. While the agency I envisaged did not come to pass, some of these considerations were addressed when I served on the President's Science Advisory Committee (PSAC) under Lyndon Johnson (1964-68), and again when I served on the newly established Council on Environmental Quality (CEQ) under Richard Nixon (1970-72)."

"A critical event in this period was the Santa Barbara oil spill, just two weeks after Richard Nixon's inauguration. Television media covered the blow-out and the impacts on the birds and sea life on an hour-by-hour basis. The administration faced its first real crisis by quickly appointing a small group of scientists and engineers to recommend solutions. I was a member of that group. At the time, I was Vice Chancellor of the University of California, Santa Barbara, and a holdover member of President Johnson's Science Advisory Committee (PSAC). I quickly flew to Los Angeles to meet the President. In order to demonstrate to the public that all was well, the President was to walk along the beach, with coverage by TV photographers who would be backing up. I was to be on Nixon's right, and on his left, Fred Hartley, president of Union Oil Company. As the choreographed walk proceeded, Hartley continually asserted that there had been no damage. He also emphasized that there really was no oil on the beach. Upset at Hartley's statements, I contradicted him, stating that the tide came in, the tide went out, and each time the tide came in it deposited a layer of oil. Impulsively, I kicked at the sand, sending an oily glob of sand onto a highly strategic area of the President's trousers."

"With this incident, 'environment' became a central issue of American politics for the next decade. Two of the principal accomplishments of this period were the passage of NEPA—the National Environmental Policy Act—and the establishment of the Environmental Protection Agency. This turned out be the most important work I did in the political domain, an example of how scientists being involved in politics does make a difference."

"At the time, NEPA's critics said it was vague and inconclusive. Yet with its clear statement of intent—that it be the policy of the federal government to 'use all practicable means and measures . . . to create and maintain conditions under which man and nature can exist in productive harmony and . . . [meet] the needs of the present without compromising the ability of future generations to meet their own needs,' NEPA anticipated the concept of sustainable development."

"The establishment of the Environmental Protection Agency is another example. CEQ played a major role in this. The framework of laws that today give the federal government authority to protect the environment all came out of the work carried out by the CEQ between 1970 and 1972. But by late 1972, the shadow of Watergate had crept over the White House, and I resigned."

"People don't generally think of Richard Nixon as having been a great environmentalist, but he was a very astute politician and he knew that environmentalism was going to be a big issue in the 1972 election. So he wanted to do something about it, to have something tangible to point to, and he took the advice of his scientific advisors (at least in this instance). EPA and NEPA were the result. I was very proud of this work."

"My work on the CEQ convinced me that environmental problems had to be addressed from multiple angles and required discourses between the environmental scientist, the economist, the lawyer, and the sociologist. We needed to develop overall strategies and policies for dealing with the whole problem of the environment. These conclusions motivated me in 1972 to become the first director of the Environmental Studies Program at Dartmouth College. The principal mission of this program was to provide an opportunity for undergraduates to assess the seriousness and complexity of environmental problems and to understand how these problems can be solved."

On the Segue from Weather Modification to Climate Change.[6] "Why was I optimistic about weather modification, when so many others were skeptical? I considered my optimism justified on three grounds. 1) The basic understanding of the physical processes of the atmosphere had been achieved. The atmosphere was complex, but not mysterious. 2) High speed computers were making it possible to model atmospheric processes, including the effects of cloud-seeding experiments, and 3) A new array of instruments, particularly satellites, was making it possible to observe and detect atmospheric changes. Satellites in particular would soon make global coverage possible. It seemed to me that the nonscientific aspects of weather modification—political, economic, sociological—would prove far more difficult than the scientific ones. At the same time, I also became an advocate for increasing our basic understanding of the environment through the growth of environmental science."

"Perhaps more important, I became convinced that inadvertent weather modification was already occurring. Like many earth scientists, my initial concern was the opposite of what concerns us today: global cooling. In a 1970 lecture to the Industrial College of the Armed Forces, I said: 'Apart from changing the character of the air, the vast quantities of material introduced into the atmosphere may be changing the climate of the planet. While we do not know whether the changes observed result from putting carbon dioxide and particulate matter into the atmosphere, or indicate basic natural changes, it is unmistakable that the atmosphere is cooling off and has been cooling for the past 30 years. The average temperature worldwide has dropped about half a degree Fahrenheit over these last 30 years.' This perspective was consistent with the geological understanding of the time that we live in an inter-glacial period and are heading towards the next ice age. Our worry was that our actions might be accelerating that journey."

"Yet, at the same time, we knew that carbon dioxide could have the opposite effect as particulates, and induce global warming. In same lecture I continued: 'We do know that the carbon dioxide content of the atmosphere has increased by about 10 percent over the last 70 to 80 years, the period of the great industrial revolution.' Elsewhere I suggested that the addition of carbon dioxide to the atmosphere had produced an increase in the average temperature of the lower atmosphere of a few tenths of a degree Fahrenheit—an increase that might have been greater were it not for the countervailing effects of urban and industrial pollution. The key point was that the long-held assumption that the land, water, and air can absorb waste products in unlimited quantities was wrong. The ocean, the atmosphere, and even the solid Earth had been viewed as receptacles of essentially infinite capacity; now we were recognizing that on a local, regional, and even worldwide scale we might have exceeded nature's capacity to dilute the effluence of our technology. And we knew too little about the paradoxical effects of warming and cooling to tell what the net outcome might be."

"We cannot detect changes, either desirable or undesirable, without repeated observations and established baselines. One of the most important and convincing monitoring programs was that of Charles Keeling, who by 1969 was already able to show that atmospheric CO_2 was increasing by approximately 0.2 ppm per year, and that, of all the CO_2 produced by combustion, two-thirds are absorbed by the oceans and biomass, the remaining one-third remaining in the atmosphere. At that rate of deposition, the amount of man-made atmospheric CO_2 was doubling every 23 years. I argued for immediate attention to the issue: to a high priority for increased support for research on inadvertent modification, with particular attention to the effects of altering the thermal balance by changes in the albedo, CO_2 and dust particles."

"In 1969, it seemed plausible that our activities could either lead to a disastrous ice age or to an equally disastrous melting of the polar ice caps. Interest in the topic mushroomed, and, as a member of the JASON committee, and through the MITRE Corporation, I undertook a series of studies, funded by the U.S. Department of Energy, Office of Energy Research, which convinced me that the scientific basis for the greenhouse effect was sound."

"Keeling had continued his painstaking measurements of atmospheric CO_2 at a remote site in Hawaii, now the Mauna Loa Observatory, and demonstrated continued exponential growth—with concentrations approaching 350 ppm by the late 1980s. The exponential growth in carbon dioxide levels paralleled the increased worldwide use of carbon-based fuels, while calculations of the expected increase in average temperature of the Earth's surface since 1900 led to a value of about 0.5°C, matching the detailed analysis of tens of millions of surface-temperature observations. Given continued growth in fossil fuel use, major climatic shifts could be expected as warming proceeded at an increasing pace."

"Past climate change—such as the Little Ice Age in 1500-1850 AD—had had a profound effect on human history. But before19th century industrialization, man's activities were of too small a scale and too low an intensity to alter global climate. With the mechanization of agriculture and the greatly enhanced use of carbon-based fuels, particularly coal, the situation changed. Both the burning of coal and the greater development of agriculture released carbon that had been stored in the soil and rocks for thousands or millions of years."

"In view of the heightened interest in long-term climatic change, the question naturally arose as to whether the warming trend would have been noticed if theory had not predicted that it should be there. I was convinced of the warming long before the detailed analyses of the temperature records were available. I had observed that the snouts of the glaciers on the Alps on the south island of New Zealand had moved from sea level to high up the mountain between 1900 and the present. New Zealand, having a relatively isolated geographical setting in the ocean, was more likely to capture longer-term trends than glaciers in more continental regions. Critics will undoubtedly question the reality of the derived warming. Nevertheless, the statistical base for the inference is strong, and the independent confirmation from the Arctic [permafrost] may prove persuasive."

RICHARD MULLER WRITING ON GORDON'S LATE CAREER

I had known Gordon since the 1980s when I became a member of JASON, a Department of Defense scientific advisory committee established in the 1950s, of which Gordon was a longstanding member. But I had never worked with him on any projects. However, I had a vivid memory of a talk he had given in the late 1980s concerning the state of knowledge of climate modeling, particularly with regard to the cycles of the ice ages. The "standard model" was the Milankovitch theory, as modified by John Imbrie and others. This theory explained the ice age cycles as due to variations in the eccentricity of the Earth's orbit, and changes in the tilt of the poles, formally known as the "obliquity." I became interested in this subject, and developed a hypothesis that extraterrestrial dust could affect climate. I asked Gordon whether he could point me to a paper that would convince me that the Milankovitch theory was basically correct. "Yes, and I'll give you a copy in a moment." He rummaged through his desk, and gave me a copy of a review paper he had written in 1990.

This paper should be considered a classic (1990). It covered two subjects, the Milankovitch theory and the potential role of marine clathrates in climate. He brought to the work a sophistication in statistical analysis he had learned from one of the fathers of the field John Tukey, and that was far above the standards being used in the field. After I read the paper he lamented that "now there were two people who have read it." So we began to collaborate.

I remember a day when I did a calculation of some oxygen isotope data with my laptop and showed it to Gordon. He was disturbed. "The 100 kiloyear peak is too narrow," he pointed out. "The Milankovitch theory doesn't have such a narrow peak." His observation became the basis for a major follow-up effort that we did over the next eight years.

In "Glacial Cycles and Astronomical Forcing" (1997) we extended Gordon's 1993 insight: that the narrow 100 kiloyear peak was prima facie evidence that the cycles of the ice ages were driven by orbital forcing and were not the result of internal changes in the earth or the sun. Our book *Ice Ages and Their Astronomical Causes* (2000) had two goals: to explain in detail all of the aspects of paleoclimate that we had uncovered and to prepare a primer for proper statistical analysis of such data.

In 1990 Gordon left his job with the Mitre Corporation and returned to academia. His passion had evolved over the decades from pure science to the use of science to address world issues. Rather than take a job in geophysics he accepted a position as a professor of international relations in the Graduate School of International Relations and Pacific Studies at the University of California, San Diego. He also served as the research director for international environmental policy at the Institute on Global Conflict and Cooperation on the same campus. He continued as a member of the Board of Directors for the Environmental Research Institute of Michigan and as one of the most active participants in JASON, with much of his effort directed toward environmental issues—which he considered to be central to U.S. national security. His academic research was largely directed toward climate—including some mysterious behavior in the radiation balance of clouds—and work in frozen deep-ocean methane deposits known as clathrates. He argued that they probably played an important role in paleoclimate, since their existence was potentially unstable to changes in temperature and sea level. The role of clathrates is still largely mysterious, but Gordon is recognized as one of the first people to draw attention to them.

Gordon loved the environment at San Diego, but he was not happy with his administrative burdens. He had little help and had to spend much of his own effort organizing visitors and meetings. Those duties limited his ability to teach, to study, and to continue his environmental investigations; so he resigned. Then in a strange twist he accepted a job with far greater administrative burden but one that had potentially greater impact on world affairs: director of the International Institute for Applied Systems Analysis (IIASA) at Laxenburg, on the outskirts of Vienna, Austria. He saw this institute as an organization that could exert important influence on its member countries.

IIASA was in serious financial difficulties, and Gordon applied his incredible intuition about the stock market to the institute's portfolio, outperformed the professional investors, and greatly enhanced the institution's finances. He worked hard to expand membership in IIASA, and managed to get Norway to join. He hoped other countries would follow. He upgraded the mathematical standards of the institute, insisting on rigorous statistical methods. He refused to reappoint people whom he considered to be "dead wood," but this made enemies, leading to an acrimonious parting with the institute's Governing Council.

CONCLUDING THOUGHTS

The book on Gordon is yet to be written. We recall his academic career, from chemical engineering and geology at Harvard (his *summa cum laude* was the first in the department), to an assistant/associate professorship in geology and geophysics at MIT, to a full professorship (at the age of 29) at the Institute of Geophysics at the University of California, Los Angeles, to a professorship in physics and geophysics at the University of California, Santa Barbara, to the chair in environmental studies and policy at Dartmouth, to the Institute of Global Conflict and Cooperation (IGCC) at the University of California, San Diego, to the directorship of the International Institute for Applied System Analysis (IIASA). Interspersed is a one-year residence at NASA and seven years as vice-president and chief scientist with the MITRE Corporation. Add to this his many, many services on national and international committees, and the enumeration alone would fill the allotted pages of this biographical memoir.

Gordon was elected to the National Academy of Sciences in 1962 at the age of 32!

We have chosen to emphasize two major late twentieth-century issues in which Gordon played a significant role: plate tectonics and climate. On plate tectonics, in the long run Gordon's opposition to Earth mobility turned out to be in error; he based his reasoning on a model Earth of finite strength rather than the high-temperature creep of nearly (or partially) molten material. In any event he chose not to be one of the late jumpers on the bandwagon. Perhaps he was just stubborn. But Gordon was not alone; he was joined by other distinguished geophysicists, such as Maurice Ewing and Harold Jeffreys (as well as by various aging directors of Soviet geology institutes).

On weather and climate modification Gordon was one of the earliest scientists to call attention to carbon dioxide as a specific problem, to push forward the scientific understanding of the likely impacts of increased atmospheric CO_2, and to bridge the science policy divide.

A final word needs to be said on Gordon's contributions to national defense and intelligence issues. Gordon served on JASON for 37 years; during the Vietnam War he chaired a JASON committee on designing the "McNamara Fence" (an instrumented frontline for preventing enemy intrusion into South Vietnamese territory), probably the earliest version of the instrumented battlefield. Gordon was passionate about JASON, where he brought his formidable intellect to bear on an enormous diversity of problems. He always spoke his mind, driven by his insatiable curiosity in a multitude of fields and his ability to convey the excitement of the research endeavor to scientists and lay people, to politicians and bureaucrats.

Gordon chaired the MEDEA Committee (initially the Environmental Task Force) of the Central Intelligence Agency, a brainchild of Senator (later Vice-President) Gore for the application of “overhead assets,” that is, information collected by intelligence satellites, to the solution of environmental problems. Here the intelligence and academic communities, two disparate communities meeting initially under conditions of mutual mistrust, developed over the years a feeling of trust and respect. Gordon was at his best, combining his unique environmental background with patience and perseverance. In 1994 the CIA presented Gordon with the Seal Medallion, the highest civilian honor of the agency.

In the early 1990s after leaving MITRE and having changed his interests and residence so often, he found himself without a stable home base. Neither the Graduate School of International Relations and Pacific Studies of the University of California, San Diego, nor the Governing Council of IIASA, nor his body now weakened by childhood polio, could accommodate to his singular style of work and living. He was reluctant to use a walker or wheelchair, and Austria was not friendly to his developing handicaps. It became yet another burden on his energy, and that of his wife, Margaret. Upon his return to the United States from Vienna Gordon took up residence in Cambridge, Massachusetts, but without formal association with any of the institutes or departments he had helped to develop, or indeed had founded.

We remember Gordon for his warm friendship, insatiable curiosity, and powerful intellect. He was an inspiration to his students and to all who knew him. He was never dull. In the words of Freeman Dyson, "It is bad for the world that Gordon's informed and critical voice is silent." Gordon is survived by his wife, Margaret Stone MacDonald, three sons, one daughter, and five grandchildren.

NOTES

1. G. J. MacDonald. How mobile is the Earth? In *Plate Tectonics: An Insider's History of the Modern Theory of the Earth,* ed. N. Oreskes with H. Le Grand, pp. 111-27. Boulder, Colo.: Westview Press, 2001.

2. Most of the prose in this section is taken directly from Gordon's articles (with minor editorial modifications or in a few cases sentences added to clarify points based on e-mail communications in the autumn of 2001) and my conversation with Gordon at his home in Cambridge, Massachusetts, on October 17, 2001.

3. These are short selected quotes from "How mobile is the Earth" in *Plate Tectonics: An Insider's History of the Modern Theory of the Earth,* ed. N. Oreskes with H. Le Grand. Boulder, Colo.: Westview Press, 2001. According to Margaret McDonald, this was Gordon's last serious writing.

4. Excerpted primarily from G. J. F. Macdonald. Science and politics of rainmaking. *Bull. At. Sci.* 4(1968):8-14, with additional materials based on *Weather and Climate Modification,* Report of the Panel on Weather and Climate Modification of the National Academy of Sciences, pub. no. 1350 NAS./NRC, Washington, D.C., 1966; MacDonald. How to wreck the environment. *New Sci.* 25(Apr. 1968):180-82; and MacDonald. Weather modification as a weapon. *Technol. Rev.* 78(1975):56-63.

5. Excerpted from MacDonald, 1965. Environmental sciences—problems and prospects. *Trans. Am. Geophys. Union* 46(2):373-76, 1965; G. J. F. MacDonald. Environment: The evolution of a concept. In *Yesterday, Today and Tomorrow: The Harvard Class of 1950 Reflects on the Past and Looks to the Future.* Arlington, Mass.: Travers Press, 2000; G. J. MacDonald. Environmental studies program: Dartmouth College. *Chemosphere* 5(1977):185-205; and conversation with Naomi Oreskes, Cambridge, Massachusetts, October 17, 2001.

6. This section is compiled from several of MacDonald's extensive writings on climate change from the late 1960s onward. These include: Science and politics of rainmaking. *Bull. At. Sci.* 4(1968):8-14; Weather modification. *Sci. J.* 4(1968):39-44; Our beleaguered environment. In *Perspectives in Defense Management,* autumn 1971, pp. 11-20; How to wreck the environment. *New Sci.* 25(Apr. 1968):180-82; Caring for our planet: How man endangers the planet. *Current* 114(1970):17-24; Climatic consequences of increased carbon dioxide in the atmosphere. In *Power Generation and Environmental Change,* ed. David Berkowitz and Arthur Squires, pp. 246-62. Cambridge, Mass.: MIT Press, 1971; Pollution, weather and climate. In *Environment: Resources, Pollution, and Society,* 1st ed., ed. W. W. Murdoch, pp. 326-36. Stamford, Conn.: Sinauer Associates, 1971; The long-term impacts of increasing atmospheric carbon dioxide levels. JASON Technical Report JSR-78-07 and JSR 789-04. Arlington, Va.: SRI International, 1982; Scientific basis for the greenhouse effect. *J. Policy Anal. Manage.* 7(3)(1988):425-44.

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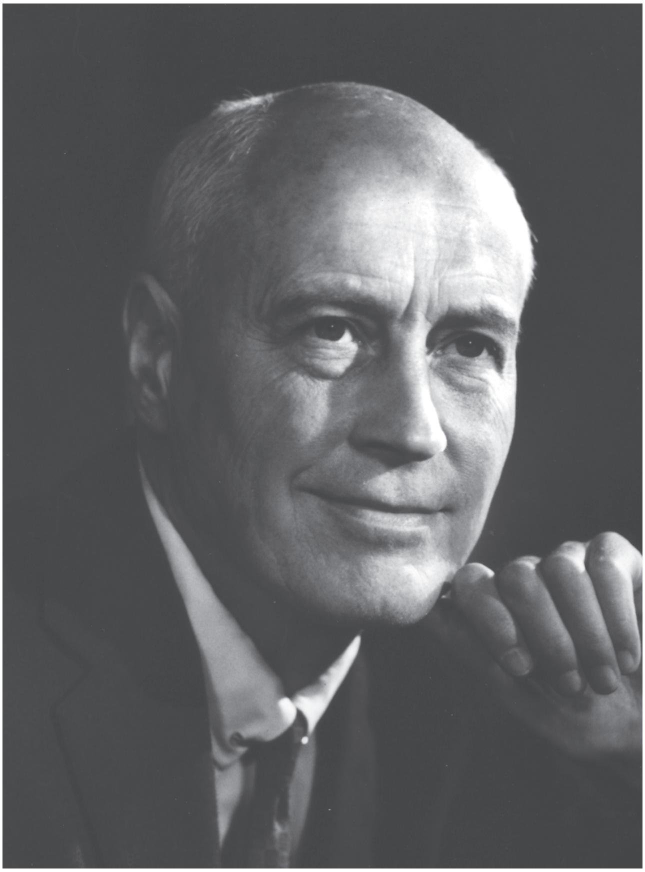

Photo by Karsh, Ottawa

H. W. Magoun

HORACE WINCHELL MAGOUN

June 23, 1907–March 6, 1991

BY LOUISE H. MARSHALL

HORACE WINCHELL MAGOUN was an emigrant from chilly New England to the warm clime of Southern California. Between his residencies on the North American coasts, a 20-year Midwestern interlude intervened, a period during which he developed a conceptual synthesis of one of the major integrating elements of the animal nervous system. With clear, direct evidence and bolstered by supporting clues, the idea of the brain stem reticular formation as an arousal system interposed between incoming (afferent) channels and outflow to motor, endocrine, and autonomic effectors, satisfied the test of time and became accepted knowledge of some mechanisms related to cerebral and behavioral integration. Continuing studies by Magoun and other neuroscientists revealed the function of the nonspecific systems of the central core of the brain stem in modulating the excitability of the central nervous system in response to internal and external environmental stimulations. The diffuse influence of reticular core activity on the cerebral hemispheres was suggested experimentally in the maintenance of subjective conscious wakefulness and attention to novel stimuli, phenomena that implied a philosophic relationship to higher nervous activity.

Born in Philadelphia, the middle of three children, Magoun had become a thorough New Englander by the time he left the East Coast. His baby sister called him “Tid,” a nickname he much preferred to his given name. Their father, Roy Winchell Magoun, bore the surname derived from the Scottish, meaning son of a blacksmith. Roy was ordained in the Episcopalian ministry, and so the early years of the young family were peripatetic, with successive, short assignments to parishes throughout New England. During that period Tid spent several summers with his mother’s two sisters living in their Coe homestead in West Newton. They were retired teachers from the Boston public schools, and they perhaps instilled a sensitivity to the English language in their bright nephew. Roy Magoun’s final ministry was to Newport, Rhode Island, as founding superintendent of the Seaman’s Church Institute. This charitable establishment—still serving its ministry today—was the gift of a few concerned citizens to provide a home away from home, where seafarers could rest in clean, inspirational quarters between voyages, an enterprise that reflected the progressive era of extending a helping hand to the downtrodden. An affable man and adept at socializing with Newport’s summer colony, Roy was active in the city’s civic life and related well to the summer “cottagers.”

Magoun’s mother, Lucy Coe Perkins, came from an exotic background. Her just-wed parents had embarked for Yokohama in the wake of the opening of Japan to foreigners and Grandfather Perkins had set up a successful dental clinic that undoubtedly fostered the Japanese proclivity for a gleaming smile. Subsequently they moved to Shanghai, where Lucy was educated in a French convent. Dentist Perkins eventually returned to practice in Boston, and his grandson remembered formal visits as a child to ensure his own gleaming smile. Another memory was of shopping trips with his mother to the local French purveyor of cheeses and the ensuing animated conversation. She eventually was known in Newport as "the cat lady" in recognition of her compassion for stray felines, an attitude shared by her son, who became adept at handling cats in the laboratory.

When the Magoun family arrived in Newport in 1919, Horace Magoun was an impressionable 12-year-old. During summer vacations he reveled in lazy days with his peers at their special beaches and evenings hanging around the theater crowd. Later he learned to drive and became part-time chauffeur to his high-school history teacher. That association instilled an appreciation of history, a lifelong motivating interest of the future scientist. Four years at Rhode Island State College, plugging along with his major sequence in biology and washing dishes in exchange for meals, were enlivened by registering three times for a course in nineteenth-century French theater and the vicarious idyll of "la vie Bohême." On graduation from "Little Rhody," as Magoun called his college, in 1929, coincident with the Great Depression, he gratefully accepted a half-time teaching assistantship in zoology at Syracuse University in western New York.

For his part-time research at Syracuse, Magoun concentrated on the embryogenesis of two giant equilibrium-controlling neurons situated on each side of the brain of Petromyzon, a predator invading New York's fishing waters. After receiving, in 1931, his master's degree and marrying a companion graduate student, Jeannette Alice Jackson, he continued his westward migration to take up an appointment to the newly organized Institute of Neurology at Northwestern University Medical School situated beside Lake Michigan in Chicago. There he commenced full-time studies toward the doctorate under Stephen Walter Ranson (1880-1942), a leading neuroanatomist-physiologist. Ranson's closely orchestrated research program initially concerned the neural aspects of posture and locomotion and later focused on the hypothalamus and nearby subcortical structures in the integration of somatic and visceral processes in homeostasis and emotional behavior.

By his acceptance into Ranson's graduate program, Magoun had the good luck to participate prominently in one of the early outstanding world centers of research on the nervous system. All work in the institute was centered on a novel instrument, the Horsley-Clarke stereotaxic apparatus, whose role in furthering progress in neuroscience cannot be overestimated.[1] After two dormant decades in England, use of the instrument was revived and its vast utility revealed by Ranson's commissioning a clever machinist to replicate the specifications published in 1908 (*Brain* 31:45-124). Paired with an atlas of the cat brain prepared by Ranson's associate, Walter ("Rex") Ingram, the stereotaxic instrument opened the uncharted realms of subcortical brain tissue to experimental discovery and manipulation by providing a tool for accurate and replicable localization of clusters of neuronal cells. Biomedical research in the neurologic sciences at that time was largely centered on the peripheral nervous system and analysis of the nerve action potential, culminating in the Nobel award to Gasser and Erlanger in 1944. There were still many unsolved problems, the most contentious of which was the nature of the action potential—electrical or chemical. The "axonologists" were in full stride, sharpening the contrast between the "new" subcortical investigations with behavioral components and the search for attributes of the nerve action potential.

By the time Magoun joined the flourishing program at Northwestern, exploration of the hypothalamus was underway. He immediately took part in studies in cat of the effects of stimulation of the gland on respiration and other motor functions and in June 1934 submitted a dissertation titled

"The Central Path of the Pupilloconstrictor Reflex in Response to Light." Curiously the previous year an article of that title was published bearing Ranson's name as first author[2] and presenting most of the dissertation data. The jarring experience of having his work appropriated may help explain Magoun's later insistence on placing his name last. Five additional collaborative papers on the occulo-pupillary constrictor reflex appeared in 1935, but it was not until nine years later that Magoun delineated the corresponding dilator pathways.[3] Magoun's role in establishing this new direction in neuroscience research was fundamentally significant. He not only imparted his skill in the use of the instrument but also had major responsibility in carrying out studies on visceral integration, emotional expression, the regulation of feeding, fighting, mating, and other vital behaviors that constitute the responses to the internal and external environments and preserve the individual and the species. During the 11 years of his association with Ranson and institute personnel, 38 collaborative papers were published; the ubiquity of Ranson's name on studies in which he did not touch the apparatus (attested by at least two students) was a sensitive point with the younger scientists.

Ranson's death in 1942 and the incipient dismantling of his institute thrust Magoun into an unaccustomed independence. He moved into basement space of the department of anatomy, commenced teaching medical students, and successfully applied to the National Foundation for Infantile Paralysis (March of Dimes) for support of a study of the neuropathology and neurophysiology of the bulbar type of polio. He found the injury was to the reticular core of the brain stem, and in experimental animals stimulation of that region either facilitated or inhibited ongoing motor activity, depending on site of stimulus and suggesting extrapyramidal tract involvement, in addition to the usual pyramidal motor innervation. Another series of experiments, in collaboration with Ruth Rhines, an M.D. seeking her Ph.D., extended the work to skeletal muscle physiology and was summarized in a slim monograph in 1948.

In contrast with the formal and reserved atmosphere that had permeated Ranson's institute, Magoun took great delight and inspiration from association with a group recruited by Percival Bailey to the neurophysiology research laboratory of the Illinois Neuropsychiatric Institute in Chicago. There the high spirits and relaxed camaraderie with Bailey, Warren McCulloch, Gerhardt von Bonin, and John French brought him within the circle of the second of three neuroscience centers of excellence in Chicago in that era.[4] During his last four years in Chicago, when the INI association bloomed, Magoun was a coauthor on six papers; they dealt with descending modulating pathways to the reticular formation and with tremor.

The Magoun research program was enhanced in 1947 when he offered space to Donald B. Lindsley, a new appointee to the department of psychology of Northwestern University. Its Evanston campus had no animal facilities, and Lindsley brought expertise in the field of electroencephalography. An additional extension occurred in the fall of 1948, when Giuseppe Moruzzi from the University of Pisa, Italy, arrived as a Rockefeller fellow. Finding the Institute of Neurology bereft of space, he was glad to accept Magoun's invitation to take part in studies on the cerebellum. Their collaboration yielded the famous paper of 1949, which became a citation index classic.[5] As the senior author later wrote, in lightly anesthetized animals stimulation of the brain stem seemed to abolish the cortical EEG waves, however "[w]hen amplification of the cortical record [was] by chance turned up . . . [t]hen we saw the large slow waves give way, during reticular stimulation, to a record of low-voltage fast activity, called 'EEG arousal,' a pattern which was characteristic of alert attention in the human EEG" ("Autobiographical Material," unpublished manuscript, n.d., p. 12). After Moruzzi returned to Italy, Magoun showed with Lindsley and several students, including Thomas E. Starzl, that reticular formation impulses projected forward diffusely and across sensory modalities, that they were conducted through medial core pathways, and that they persisted after cessation of the initiating stimulus. This ascending reticular system was shown to be associated with alert wakefulness as a background for sensory perception, higher intellectual activity, for voluntary movements and behaviors, and to provide insights about brain and mind. Those findings have become a piece of accepted wisdom of how brain and behavior are coordinated.

In 1950, boxed in by lack of space and an uninterested administration just when his breakthrough program was underway, Magoun made a final migration, to Los Angeles, to become founding chair of the department of anatomy at the "southern branch" of the University of California. With construction incomplete and the need to accommodate newly recruited scientists, contact was renewed with John Douglas French (1911-88), then head of neurosurgery at the Long Beach Veterans Administration Hospital. French's recommendation through administrative channels resulted in conversion of unused dependent wards into laboratories for basic neuroscience. During that decade teams of investigators were in place at Long Beach, 30 driving miles and 118 traffic lights from Westwood, and at other, closer VA and California state facilities.

There is no doubt that Horace Magoun arrived in Los Angeles with visions of an interdisciplinary institute that would bring together collaborative studies and report directly to the chancellor. As he later wrote, "Those eleven years [at Ranson's institute] of essentially full-time research . . . thoroughly oriented [me] to an institute way of life, and throughout the balance of [my] investigative career all [my] research was pursued collaboratively." (1984). And again, his experience at Northwestern and the INI "had imprinted a high regard for collaborative and interdisciplinary research. . . ."[6] Coincident with settling his new faculty recruited from various neuroscience subdisciplines, he confronted the local academic traditionalists, many of whom were wary of losing turf to the novel idea of a multidisciplinary research unit. With legendary persistence and regental backing, the neuroscientist prevailed and in the political atmosphere of Vannevar Bush's "endless frontier," the availability of funds for construction and training programs was no problem. By means of federal and state dollars a 10-story building of research laboratories, offices, and animal quarters was completed in 1961 at the corner bridging the medical school with the Neuropsychiatric Institute. The Brain Research Institute (BRI) was one of the earliest interdisciplinary and multidisciplinary American organizations committed to the production of knowledge on brain and behavior and represented the outcome of Magoun's persuasive powers to surmount departmental barriers. Unfortunately, territorial animosities were not entirely resolved and while Magoun was on sabbatical at the National Institutes of Health, the freestanding aspect of the proposal was rewritten and the BRI became a subsidiary of the medical school. The BRI's prime mover took a pragmatic view of the damage and flew home to ensure the best possible preparation for the site visit. Instrumental in the success of the BRI in attracting a steady flow of students and guest investigators was the award of one of the innovative training programs in the biological basis of mental health, activated by the National Institute of Mental Health in 1957, and its continued renewal to the present.

Magoun's plans for the nascent BRI benefited from his ranging participation in advisory positions at the national level, notably at the National Institutes of Health. An effective committee member, he rotated among appointed terms at the five institutes that had stakes in the nervous system, and thus was aware of their interest in receiving novel proposals. Among the adjunct facilities established as support systems for the emerging discipline at UCLA, the Brain Information Service (BIS) was the most successful, and is a survivor today. The Data Processing Laboratory was a pioneer in computer analysis of the EEG. The Biosphere program served its short usefulness in the federal space program. And finally the Neuroscience History Archives, with an oral history project and digital photographic collection, was established in 1980 and continues to promote the preservation and knowledge of neuroscience history.

His gradual preoccupation with neuroscience infrastructure was further manifested by Magoun's involvement in securing funds to organize two conference series sponsored by the Josiah Macy, Jr., Foundation. Three conferences each were held on *The Central Nervous System and Behavior* (1958-60), and on *Brain and Behavior* (1961-63), distinctive for their inclusion of scientists from the Soviet bloc. That gesture earned Magoun's designation as the leader of the U.S. delegation in 1958 to the so-called Moscow Colloquium on Higher Nervous Activity. The ebullient success of the colloquium led to the formation of the International Brain Research Organization (IBRO), and Magoun had a seat on its central council. He was editor of the three-volume nervous system section of the first *Handbook of Physiology* published by the American Physiological Society (1959-60). That period also saw the first edition of *The Waking Brain* (1958), an acclaimed delineation of the ascending reticular system and its ramifications, with multiple reprints and an expanded second edition (1963). A solid reputation for effective writing is attested also by two invited chapters in the *Annual Review of Physiology* (1943, 1949) and again in *Physiological Review* (1950).

The acceptance of the ascending reticular formation as an important integrating concept in the knowledge of brain and behavior was acknowledged by many awards and invitations to lecture. A non-award, however, was a source of deep embarrassment in 1957, when the Nobel selection committee's designation of Magoun was broadcast by the media, then superseded by the Karolinska Institute Assembly with "one of our own." Magoun internalized the disappointment, mentioning only once during a long friendship that "Ragnar Granit beat me out of the Nobel."

The next phase of Magoun's career took him officially out of the laboratory and into the upper halls of academe. Installation of the research programs of members of the BRI in the new building offering advanced laboratory facilities under the directorship of John French had lessened the pressure, and Magoun had stepped down from the chairmanship of the department of anatomy in 1955. By that time the department was one of the largest and most productive in the country, and it was characteristic of its founder to move to more challenging endeavors when old ones were doing well. He served two terms as dean of the graduate division and in that capacity, 1962 to 1972, he promoted the participation of minorities in higher education and spoke and published on educational topics. The new challenge seemed to be to promote the ranking of his university in the context of the older established schools. The rivalry had a sense that the West Coast had overtaken the eastern axis in many of the subdisciplines of neuroscience.

In view of his mother's upbringing it is not surprising that Magoun relished visits to Japan. In Yokohama in the springtime he imagined his mother as a little girl playing beneath the cherry blossoms. His presentation to the Japan Medical Society at Osaka in 1963 was titled "Plasticity and Memory Process in the Nervous System." Four years later and as a member of the medical sciences panel of the United States-Japan Committee on Scientific Cooperation, he addressed the medical society in Tokyo on "The Role of Organized Research Units in the Development of the Neurosciences," a quasi-historical paper. The crowning fulfillment came in 1971 with the award of the Second Class of the Order of the Sacred Treasure by the Japanese government in recognition of Magoun's role in training the country's neuroscientists. Some Japanese researchers who had spent time at the BRI banded together in an alumni association, the Japanese Reticular Society, but it did not survive the passage of time. In 1980 the Japanese scientific monthly, *Kagaku Asahi*, published an interview with Magoun, asking the mind and brain question. The translation, by Masako Isokawa, provides a fortuitous coda to Magoun's scientific contributions: "Mind is a melting pot of all experiences."

The final phase of Magoun's career commenced when he returned to the Los Angeles campus in 1974. The two previous years he had been in Washington as director of the Fellowship Office of the National Research Council. There, characteristically, he had fostered the appointment of minority personnel to the numerous selection panels of the large NRC associates program administered for the National Science Foundation. At home in the BRI, Magoun assisted in the development of a division of behavioral sciences in the department of psychiatry. That relatively undemanding responsibility afforded time to renew his serious interest in brain history, the grounds for which had been set much earlier, in 1958, at a CIBA Foundation symposium; Magoun's topic was the development of ideas relating mind and brain. In 1959 he inserted a panel on brain history into the scientific panels of IBRO. Magoun was thus an ideal prospect when the Josiah Macy, Jr., Foundation asked him to contribute an essay on neuroscience for its two-volume publication *Advances in American Medicine: Essays at the Bicentennial* (1976), in collaboration with the National Library of Medicine; he completed the assignment several weeks ahead of the deadline with minor collaborative help. The next project was a set of 40 posters accompanied by a 27-page brochure on the history of the human brain, prepared for the annual meeting of the American Neurological Association in 1979. The twenty-fifth anniversary of the BRI occasioned the publication of its history, a 325-page monograph written by the institute's prime mover (1984). Meanwhile, in direct result of his essay for the bicentennial volume, Magoun embarked on what he considered would be his magnum opus, a history of American neuroscience in the twentieth century; it was published posthumously in 2003.

Formally attired and groomed, at 6 feet and reflecting his college training as a long-distance runner, Magoun was an impressive figure in any gathering. With his supportive wife, Jean, a mutual attraction to achieving women was manifested by both scientific and historical collaborative projects. Having shared with Tid Magoun the Neuroscience History Archives office for many years, I could bask in the warmth of his enthusiastic welcome of visitors. These included both old and new friends: William Windle, who had Parkinson's disease, Jack French with Alzheimer's, and particularly students. He retold jokes on himself, such as the time at Long Beach when he rhetorically asked, "Now what stupid idiot would do a thing like that?" and a lab assistant spoke up, "I'm that stupid idiot. You want to make something of it?"

Magoun claimed to detest large social events, yet as a connoisseur of sherry he invariably took the opportunity to discourse on a current research project, a kind of rehearsal for the clean composition that he would later hand to a typist. Associates were often urged to undertake projects that Magoun had conceived, a gesture of friendly encouragement from the "900-pound gorilla" whose stature needed no boost. This was in the pattern of abandoning a project when it became viable and moving to a greater challenge. The gesture was easily misinterpreted, however, as an attempt to unload boring details and could arouse resentment. There was also a perception of arrogance generated by Magoun's overcompensation for shyness behind his cool facade. When Magoun berated staff or friends (librarians dubbed him "Earthquake Magoun" and rushed to help him) he was quick to apologize.

The ability to conceptualize with a broad brush a synthesis of the evidence discovered in the laboratory was sustained by Magoun's compartmentalization of his daily life. Events at home (especially after Jean's cerebrovascular accident in 1962) did not seem to impinge on the demands of his official commitments. He was decisively in command until a succession of small strokes preceded his death in Santa Monica, California. A pioneering career of major contributions to the knowledge of how the brain functions was coupled with a drive to promote the discipline that fosters the production of that knowledge.

Preparation of this memoir was possible only through many channels of assistance. I am grateful for cooperation of the BRI (Allan J. Tobin, Director) from all levels, and also thank Carmine D. Clemente, Charles H. Sawyer, and Arnold B. Scheibel for comments and the Magoun family for help with details. Direct access to the Horace Winchell Magoun Papers, in the Louise M. Darling Biomedical Library, University of California, Los Angeles, and the bibliography and finding aid prepared by archivist Russell A. Johnson

(copies available on request) were essential to bring this memoir to fruition.

SELECTED AWARDS, LECTURESHIPS, AND HONORS

1952 Harvey Lecture, New York Academy of Medicine
1953 Max Weinstein Award, United Cerebral Palsy Association
1954 James Arthur Lecture, American Museum of Natural History, New York
1955 Member, National Academy of Sciences
1956 George W. Jacoby Award, American Neurological Association
Israel S. Wechsler Lecture, Mount Sinai Hospital, New York
Menas S. Gregory Lecture, New York University College of Medicine
Thomas William Salmon Lecture, New York Academy of Medicine
1958 Honorary D.Sc., Université Aix Marseilles
1959 Honorary D.Sc., Northwestern University Medical School
1960 Honorary D.Sc., University of Rhode Island
Fellow, American Academy of Arts and Sciences, Boston
1961 City of Hope Award, City of Hope Hospital, Los Angeles
Borden Award, Association of American Medical Colleges
1963 Passano Award, Passano Foundation
1965 Honorary L.H.D., Wayne State University
1970 Karl Spencer Lashley Award, American Philosophical Society
1971 Order of the Sacred Treasure Second Class, Japanese Government
1974 Distinguished Alumni Lecture, Northwestern University Medical School
1988 Co-recipient, Ralph W. Gerard Prize, Society for Neuroscience

NOTES

1. H. E. Hoff. John Fulton's contribution to neurophysiology. *J. Hist. Med. Allied Sci.* 17(1962):16-71.

2. S. W. Ranson and H. W. Magoun. The central path of the pupilloconstrictor reflex in response to light. *Arch. Neurol. Psychiatr.* 30(1933):1193-202.

3. A. J. Harris, R. Hodes, and H. W. Magoun. The afferent path of the pupillodilator reflex in the cat. *J. Neurophysiol.* 7(1944):231-44.

4. The third active center was at the University of Chicago headed by Ralph Gerard in the department of physiology of the medical school.

5. G. Moruzzi and H. W. Magoun. [This Week's Citation Classic]. *Curr. Cont. Life Sci.* 24(1981):21.

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1938

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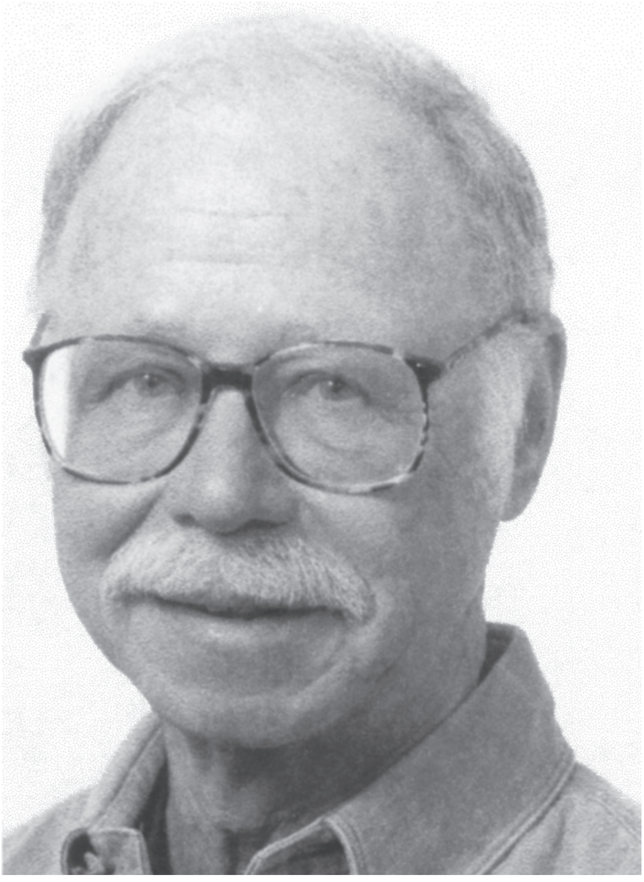

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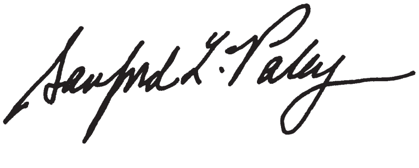

SANFORD LOUIS PALAY

September 23, 1918–August 5, 2002

BY ALAN PETERS, JACK ROSENBLUTH,
GEORGE PAPPAS, LAWRENCE KRUGER, AND
ENRICO MUGNAINI

SANFORD LOUIS PALAY, a member of the National Academy of Sciences since 1977, died on August 5, 2002, at the age of 83. He was buried in the Sleepy Hollow cemetery in Concord, Massachusetts. With his death modern neurocytology lost one of its founders. From the beginning of fine structural studies of the nervous system the high quality of electron micrographs produced by Sanford Palay set standards that others would strive to emulate, and he contributed much toward the interpretation of electron micrographs of the nervous system and the advancement of knowledge on the principles of organization of the nervous system. It must be remembered that prior to fine structural studies, stains had been developed that could be used selectively to show specific components of the nervous system in light microscopic preparations. In electron micrographs all of the diverse neuronal and glial components are seen, but much of the continuity of cell bodies and processes is lost in the extremely narrow plane of the ultrathin section. Therefore, one of the earliest challenges toward which Sanford

Based on a memorium published in the *Journal of Neurocytology*, vol. 31, 2002. With permission of the publishers, Kluwer Academic Publishers.

Palay made many contributions was to determine which profiles belonged to which parts of cells and which criteria could be used to selectively identify the myriad profiles encountered in electron micrographs.

Sandy was born in Cleveland, Ohio, of Russian Jewish immigrant parents. In 1940 he received his bachelor's degree in English from Oberlin College, a place for which he had such fond memories that he donated his collection of neuroscience journals and histological slides to the college. In 1940 he entered the School of Medicine at Western Reserve University (now Case Western Reserve), with the intention of becoming a bacteriologist. In the spring of his first year at medical school he applied for a fellowship that would allow him to do research in the summer break. He chose to work in the laboratory of Ernst and Berta Scharrer, where he was given the project of trying to stain droplet-laden cells in the meninges of the toad. In 1944 he published the results of this investigation. The Scharrers taught Sandy a great deal about scientific investigation, about neuroanatomy, and about cytology, and eventually Sandy went on to work on neurosecretion (1945,1), which was Ernst Scharrer's prime interest (1945,2). Sandy continued to work with the Scharrers throughout his time in medical school, and he developed a close relationship with Ernst Scharrer, who was to have a great influence in guiding Sandy's scientific career.

After completing his M.D. degree in 1943 Sandy spent a year as an intern at New Haven Hospital, where in the evenings he continued his research in the Department of Anatomy at Yale University. He worked on tracing the neurosecretory pathway from the preoptic nucleus to the neurohypophysis in catfish, using material that he had brought from Cleveland. At the end of the internship Sandy returned to Western Reserve University as a resident in medicine, with appointments as a teaching fellow in medicine and a research fellow in anatomy. This allowed him to continue his association with the Scharrers, and he took part in a study on chemical sense and taste in gourami and sea robins (1947). It was Ernst Scharrer who suggested that Sandy ought to meet and work with Albert Claude, who was at the Rockefeller Institute, pursuing his pioneering studies on the biochemistry of cellular components. Sandy applied and was awarded a postdoctoral fellowship to work with Claude, but when his residency in Cleveland came to an end in 1946, he was called up to serve with the Army Medical Corps as a member of the forces in occupied Japan. As a result of that service Sandy began his lifelong interest in Japanese art and culture, leading him to collect Japanese art and to cultivate many bonsai specimens. He returned to Japan in 1978 for an extended period as a visiting professor at the University of Osaka.

After leaving the Army in 1948 Sandy joined Albert Claude as a fellow at the Rockefeller Institute and spent the year examining the chromosomes of the salivary gland by electron microscopy. Formvar replicas were used (1949), because at that time there were no suitable techniques available for preparing thin sections. It was at the Rockefeller Institute that Sandy met George Palade, who had recently become a refugee from Romania, which had been occupied by the Russians and fallen under a Communist regime.

After a year, in 1949, he returned to Yale, where he was first appointed instructor and then assistant professor of anatomy. He continued his work on neurosecretion, and it was under his direction that Milton Brightman and Steven Wissig completed their doctoral theses on the relationship between neurosecretion and lactation in rats and on thyroid secretion also in rat.

By 1952 important technical advances had been made in preparing tissue for electron microscopy. As Sandy pointed out in one of his later publications (1992,1), "Palade had introduced his Veronal buffered osmium tetroxide for optimal fixation of tissues. Borysko, Swerdlow and colleagues had introduced a satisfactory method for embedding tissue in butyl methacrylate, and Harrison Latta had invented a way to break plate glass into useful knives for thin sectioning." So when George Palade invited Sandy to return to the Rockefeller Institute to work with him for six months and to learn the new techniques, Sandy enthusiastically accepted the offer. It was at this time that he began his definitive studies on the fine structure of the nervous system, and his first success in achieving good fixation of neurons was obtained by injecting osmic acid into the fourth ventricle of the rat, since here the motor cells of the abducens nucleus are close to the surface, as are the cells of the overlying cerebellum. The outcome was that in 1955 he and George Palade were able to publish a pioneering article in the first volume of the *Journal of Biophysical and Biochemical Cytology* (now the *Journal of Cell Biology*) on "The Fine Structure of Neurons." This article described the Nissl substance and the mitochondria of nerve cells, as well as long filaments that were subsequently recognized as neurofilaments. This study was soon followed by the one of which Sandy was most proud, the first description of the fine structure of synapses in the mammalian nervous system (1956). Sandy recounted (1992) how one Saturday early in August of 1953, when he was alone in the laboratory, he was using the electron microscope to examine thin sections of the abducens nucleus, and on the surfaces of dendrites and cell bodies he encountered clublike profiles that were filled with mitochondria and contained vesicles that were aggregated against the presynaptic membrane. Earlier that summer George

Pappas had shown Sandy electron micrographs showing the fine structure of a contractile vacuole in *Amoeba proteus* surrounded by clusters of small vesicles (30-40 nm diameter). Sandy remembered the close similarity between vesicles surrounding the *Amoeba* contractile vacuole and those in the presynaptic terminals on the soma of the motor neurons. Also the pre- and postsynaptic membranes were thickened and appeared denser, indicating a zone of intimate adherence, and most importantly these membranes were separated by a thin intercellular space, thus directly confirming Cajal's inference about the synaptic junctions between nerve cells. He later reminisced: "I became very excited, and having no one with whom to share this great news directly, I telephoned George Pappas at home to convey my exhilaration. Fortunately, he was there, or I would have burst. I was extraordinarily privileged to be the first one to see the synaptic junction, and I recognized my good fortune" (1992). Recollections of this period are available in a videotaped interview of Sandy Palay by Lawrence Kruger in the summer of 2001. The tape is deposited in the history archives of the Society for Neuroscience. The discovery of characteristic vesicles at the mammalian synapses was reported in a joint paper with Palade at a meeting of the American Association of Anatomists (1954). In the same year E. D. P. DeRobertis and H. S. Bennett also reported the finding of vesicles in nerve terminals of frog sympathetic ganglia and earthworm nerve cord at the meeting of the Federation of Societies for Experimental Biology meeting (Submicroscopic vesicular components of the synapse. *Fed. Proc.* 13[1954]:13:35).

Sandy returned to Yale, and in 1955 he was promoted to associate professor. While continuing to work on the brain fine structure, he began a study on intestinal villi and the pathway of fat absorption with his graduate student L. J.

Karlin, and that study was brought to completion a few years later (1959,1,2). Sandy stayed in New Haven only one year because his prominence as a neurocytologist gained him the position of chief of the Section on Neurocytology at the National Institutes of Health in Bethesda, Maryland. He was given an electron microscopy laboratory in the basement of Building 9. His new instrument turned out to have serious problems, but it was finally replaced with a later model that did function properly. His associates there included Jack Rosenbluth, Mary Grillo, Milton Brightman, David Wolf, Spencer Gordon, Jr., Sam McGee Russell, and the research assistant Catherine Crigler. Three years later, in 1960, he was promoted to chief of the Laboratory on Neuroanatomical Sciences at NIH. Sandy became interested in the tendency of astrocytes to swell after immersion fixation. Dispute with Sarah Luse and Ed Dempsy over the meaning of this artifact provided part of the impetus to develop perfusion fixation. Sandy had a strong intuition regarding what was true structure and what was artifact (myelin splits, swollen astrocytes, large extracellular spaces in the central nervous system), and this led him to develop and perfect the buffered osmic acid perfusion fixation method for rats, while continuing to work on synapses (1958,1), neuroglia (1958,2), and neurosecretion (1958,3). A major improvement occurred when the laboratory switched from methacrylate to Araldite embedding, which helped improve the appearance of compact myelin and resulted in a paper on the nerve cell bodies and their myelin sheaths in the eighth nerve ganglion of the goldfish (1961). Sandy's interest in myelin had also been spurred by a visit from Harry Webster, who brought spectacular electron micrographs of myelinated peripheral nerve fibers. There were engaging lunchtime meetings with Eric Kandel and W. Alden Spencer, who both had two-year appointments in the NIH neurophysiology laboratory. At this time in Bethesda Sandy bought a Mercedes convertible that he drove for many years.

In 1961 Sandy relinquished his position at NIH when Don Fawcett invited him to become the Bullard Professor of Neuroanatomy at Harvard Medical School. Sandy was a dominant figure in neurocytology, producing high-quality electron micrographs and lucid descriptions of structures that set standards for others to try to emulate. Soon after his arrival at Harvard he and his colleagues published an important article on the perfusion fixation with osmic acid they had developed at NIH (1962). Until then fixation of nervous tissues for electron microscopy was mostly achieved by immersing pieces of central nervous system in buffered osmic acid solutions. The resulting preservation was never very good. Using the perfusion technique with buffered osmium tetroxide, which produced black and often brittle brains, Sandy and his collaborators obtained superior preservation. This enabled them to begin to better analyze the morphological features of various components of the central and peripheral nervous systems, and accumulate a large portfolio of electron micrographs illustrating the fine structure and principles of organization of the central nervous system, especially the cerebellar cortex. Sandy was deeply impressed by how images from Golgi-impregnated material could be used to analyze electron micrographs and became very fond of the Golgi method, of which there were few practitioners in the United States. Visitors to Sandy's laboratory were regularly treated with a show of his impressive illustrations from the cerebellar cortex and emerged greatly inspired, and many returned for extended periods of collaboration.

Sandy was insistent on the value of artificial respiration of the animal prior to perfusion fixation with osmium tetroxide, as it had been shown that this improved the preservation of ultrastructure, especially that of the glial cells. Subsequently, as glutaraldehyde was introduced as a fixative and as aldehydes began to be purified, he and his trainees adopted the perfusion fixation of the nervous system by buffered aldehydes. Putting these techniques to good use, he and his wife, Victoria ("Vickie") Chan-Palay, completed detailed studies of the cerebellum. The results were published in a series of articles and culminated in the publication of the definitive description of the components of the cerebellum in their book *Cerebellar Cortex: Cytology and Organization* (1974). This book, which led to a better understanding of the neuronal circuits in the cerebellum, was cited among the Fifty Best Books of 1974 at the International Book Fair in Frankfurt.

As well as cooperating in science, Sandy and Vickie raised two caring daughters, Victoria (from Vickie's previous marriage) and Rebecca. They were a great comfort to Sandy in his waning years, and visits by his grandchildren, his daughter Victoria's two children, were always a particular joy to him.

By the late 1960s a wide range of structures in the central and peripheral nervous systems had been examined by electron microscopy, and it was becoming possible to make generalized descriptions of the various neuronal and neuroglial components of the nervous system. Sandy and Alan Peters talked about producing a book describing and illustrating the fine structural features of components of the nervous system when Alan Peters worked with Sandy on the lateral geniculate nucleus of the cat in 1963-64, but they concluded that the time was not yet ripe to undertake that project. The basic reason was that one important component of the nerve cell, the axon hillock and the axon initial segment that were the most plausible candidates for the spike initiation site, had not been recognized in electron micrographs. However, over the course of the next two years it became evident that the initial axon segment is consistently characterized by bundles of microtubules linked by cross-bridges and electron dense undercoating of the plasma membrane, and this led to another landmark publication in the *Journal of Cell Biology* (1968). Consequently in 1968 it was decided to go ahead and write the book, and Sandy and Alan Peters, who was then at Boston University, asked Harry Webster, who was at the Massachusetts General Hospital, to join them and to undertake the description of the peripheral nervous system. The hope was that such a book would serve as a guide to help others in the analysis of electron micrographs and promote the understanding of the principles of organization of the entire nervous system. The first edition of *The Fine Structure of the Nervous System* was published in 1970 as a rather thin book of some 200 pages, but by the third edition in 1991 it had grown to about 500 pages, with over 130 plates, many of which continue to be reproduced in various text books.

Sandy's great strength and charm was that he always had time for others and he was never prepared to accept second best. No doubt this latter aspect of his make-up led him to accept the position of editor in chief of the *Journal of Comparative Neurology*, when Max Cowan stepped down from that position in 1980. Sandy's attention to detail and his goal to strive for high standards of both text and illustrations resulted in the journal's receipt of increasing numbers of first-rate articles and ultimately becoming a weekly publication. Sandy examined each article submitted to the journal, and after reading the reviews he always wrote a carefully constructed letter to the corresponding author outlining which changes might be necessary to improve the article before publication. Even after retiring from Harvard in 1989 Sandy continued as editor in chief, running the journal from his home in Concord; after he stepped down as editor in chief in 1993 he continued to play a role as editor in chief emeritus. In 1991 he wrote an in-depth article on the founding of the journal (1991,2).

Sandy's ability to evaluate scientific work in the neurosciences put him in great demand as a member of editorial boards, and so in addition to the *Journal of Comparative Neurology* Sandy served on several other editorial boards, including the board of the *Journal of Neurocytology*. A number of his friends and colleagues contributed an issue of the *Journal of Neurocytology* in honor of his retirement from Harvard (October 1990). Sandy was also a member of the editorial boards of a number of other journals, including the *Journal of Cell Biology*, *Brain Research, Experimental Neurology, Anatomy and Embryology, Neuroscience*, and *Experimental Brain Research*.

Sandy's achievements in neuroanatomy were recognized by several important awards and honors, and among these we can cite the Lashley Award by the American Philosophical Society; his election to the American Academy of Arts and Sciences in 1963; to the National Academy of Sciences in 1977; and to the American Philosophical Society in 1997; his election as president of the American Association of Anatomists, an association that awarded him its Henry Gray Award for his contributions to anatomy; the Ralph Gerard Award for Contributions to Neuroscience by the Society for Neuroscience; and numerous invited lectureships. His retirement as Bullard Professor of Neuroanatomy from Harvard in 1989 by no means diminished Sandy's interest in science. As stated above, he continued for several years as the editor in chief of the *Journal of Comparative Neurology,* and in 1994 Boston College gave him an appointment as Distinguished Scholar in Residence in the Department of Biology. He enjoyed this appointment and often went to

Boston College, where he offered a graduate course in the history of neuroscience. Even when his health started to decline Sandy continued to teach this course, with the students coming to his home in Concord.

When one went to visit him at home, or later in the hospital, Sandy always had scientific journals beside his chair and was invariably ready to engage in a discussion of recent findings that he had read about and to put those findings in the context of earlier knowledge. Sandy had a deep and comprehensive understanding of the history of neuroscience, and he loved to peruse older publications. One regret is that he did not write more about this subject, for there are few with his depth of knowledge, insight, and love of the history of neuroscience (see 1987) and the close links between brain science and philosophy.

An aspect of Sandy's multifaceted personality that endeared him to many of his friends was his keen intellectual interest in the physical world, human nature, literature, music, and the fine arts. Sandy was also an accomplished pianist, and it was a source of great regret to him that toward the end of his life he was no longer able to play the classical pieces that he loved. Although Sandy was endowed with a reserved temperament, when visiting with him one felt in contact with a person of culture in the broadest sense of the word. In all probability this is the image that he would like us to remember. He will never be forgotten by those who knew him.

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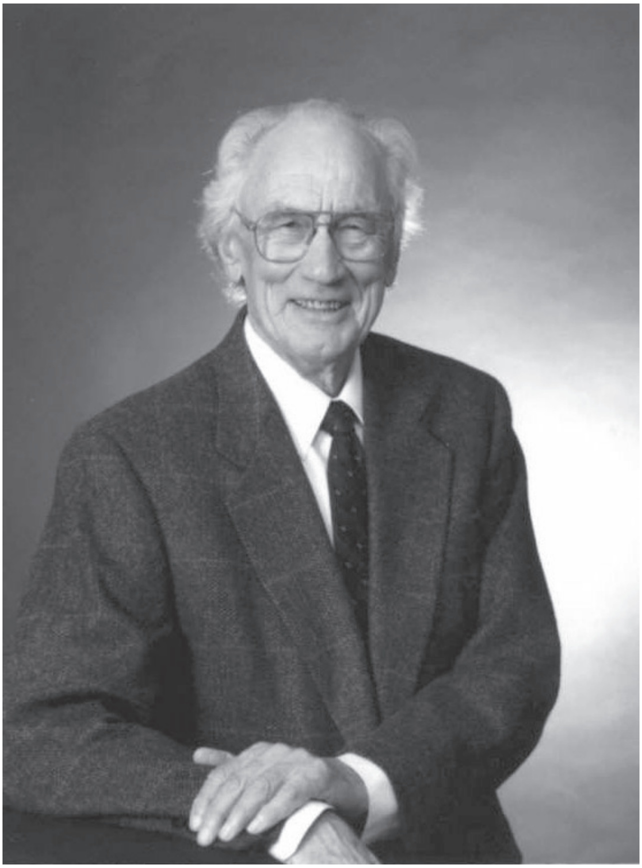

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KENNETH LEE PIKE

June 9, 1912–December 31, 2000

BY THOMAS N. HEADLAND

KENNETH L. PIKE, AGE 88, internationally recognized linguist, educator, and Christian thinker, died in Dallas, Texas, on December 31, 2000, after an illness of only five days.[1] Evelyn Griset Pike, his wife and closest friend since their wedding in 1938, and their oldest daughter, Judith, were at his side. Ken Pike was born in East Woodstock, Connecticut, on June 9, 1912, the seventh of eight children of a country doctor. He received his bachelor's degree in 1933 from Gordon College (then in Boston). In 1935 he joined the Summer Institute of Linguistics and served in Mexico, studying Amerindian languages. He received his Ph.D. in linguistics at the University of Michigan in 1942 under Charles Fries (Leonard Bloomfield was also on his dissertation committee) and later served for 30 years on the faculty at the University of Michigan.

Pike was the recipient of 10 honorary doctorates and professorships from universities around the world, including the University of Chicago, Université René Descartes, University of Lima, and Albert-Ludwigs University in Freiburg, Germany. His leadership included serving as president of the Linguistic Society of America, president of the Linguistic Association of Canada and the United States, and from 1942 to 1979 president of the Summer Institute of Linguistics (now SIL International). He was chair of the University of Michigan Linguistics Department from 1975 to 1977 and director of the English Language Institute at the University of Michigan at the same time. For a quarter of a century he divided his time between Michigan and SIL, as director of the SIL school at the University of Oklahoma and helping to establish other SIL schools around the world. He lectured in 42 countries and studied well over a hundred indigenous languages in the field, including languages in Australia, Bolivia, Cameroon, Cote d'Ivoire, Ecuador, Ghana, India, Indonesia, Mexico, Nepal, New Guinea, Nigeria, Peru, Philippines, Sudan, and Togo. He was elected to the National Academy of Sciences in 1985.

Ken Pike's contributions to the field of linguistics combined with his dedication to the minority peoples of the world brought him numerous honors. He was a recipient of the Presidential Medal of Merit from the Philippines and the Dean's Medal at Georgetown University. He was nominated for the Nobel Peace Prize 15 years in a row and for the Templeton Prize three times. At the time of his death he was a member of the National Academy of Sciences, the Linguistic Society of America, the American Anthropological Association, professor emeritus of the University of Michigan, and president emeritus of the SIL. At least 25 encyclopedias have published entries on him. He published 30 books, over 200 scholarly articles, another 90 articles for popular magazines, 8 poetry collections, and numerous other works—Scripture translations, individual poems, instruction workbooks, videos, and audio recordings. For a list of his publications up to 1987 see Brend (1987); for a complete list up to 2003 see Spanne and Wise (2003).[2]

Pike's last trip overseas was to Irian Jaya, Indonesia, in 1995, where he was the plenary speaker at the International Conference on New Guinea Languages at Cenderawasih

University. He was actively lecturing and writing until 1997 when his health required him to slow down. His last book publication was his five-volume set of poems (Pike, 1997a). He published three articles in 1998 (1998a,b,c), two in 1999 (1999a,b), and two posthumously (Pike, 2001; Peterson and Pike, 2002).

Pike's life can be seen in patterns of decades, each producing publications in its disciplines. During the 1940s his emphasis was on developing a science for the sounds of languages: phonetics and phonemics, tone and intonation (Beddor and Catford, 1999). In the 1950s he focused on anthropology and language in relation to culture, developing his holistic view. The 1960s involved mathematics, and the 1970s were devoted to grammatical analysis. During the 1980s Pike developed his areas of interest in philosophy, publishing his book *Talk, Thought, and Thing* (1993). *Current Anthropology* published "An Interview with Kenneth Pike" in 1994 (Kaye, 1994), in which Ken shared publicly for the first time some of his own personal experiences in academia. In the last year of his life he wrote a more personal account of his interactions with a number of famous scholars.[3] That account was published posthumously in 2001 (Pike, 2001). His website today is at <www.sil.org/klp>.

Pike became recognized as one of the United States' most distinguished scientists. He was first known in linguistics through his famous textbook *Phonetics* (1943), his development in the 1950s of his theory called *tagmemics*, and through his popular "monolingual demonstrations." Later he became recognized in anthropology through the growing popularity of his *emic/etic* concept. I review these below.

PIKE'S CONTRIBUTION TO LINGUISTICS

Pike's major theoretical contribution in linguistics was his development of tagmemics, an important theory in

American linguistics until the paradigm shift toward Noam Chomsky's transformational grammar theory in the 1960s. Pike's *magnum opus* on tagmemic theory was first published in three volumes in 1954, 1955, and 1960, and then in a second edition in 1967. For those not willing to work through that mammoth 762-page volume Pike later wrote a popularized version of just 146 pages that explains his theory at a level undergraduate students can handle. Subtitled *An Introduction to Tagmemics* (1982) and translated into Japanese, Korean, and Spanish, it became his most popular theoretical treatise. His most widely used book, though, and a true classic, is his *Phonetics* (1943). Published almost 60 years ago, it is still in print and used as a text in courses today.

Pike's practical contribution in linguistics was in his amazing ability to train so many students to learn, analyze, and publish data on unwritten minority languages. One of his major goals was to help colleagues with their linguistic challenges. To that end he established linguistic workshops around the world, in which he and his junior colleagues helped thousands of field researchers and Bible translators with difficult analytical challenges in aboriginal languages. When Pike first went to live with the Mixtec people in southern Mexico in 1935, he knew no Spanish, nor did the San Miguel Mixtecs. So he began learning their language monolingually, since there was no common language. This method eventually developed into his famous pedagogical monolingual approach for learning hitherto unknown tribal languages.

Who would have guessed then that there were some four to five thousand such languages spoken around the world that were unidentified and unknown even to linguists in the 1940s? This holistic approach to language learning became Pike's trademark. He eventually taught thousands of his students how to learn such languages by using the method that he demonstrated countless times in his legendary monolingual demonstrations over the decades. (The method is explained by Pike [1999b] and described best by Makkai [1998].) Today those students have produced thousands of linguistic documents on 1,200 indigenous languages in 50 countries. (See <www.sil.org/acpub/biblio/> for a bibliography of 12,000 academic publications on minority languages and cultures by SIL field workers.) Most of those languages had never been studied before, most are spoken by just a few hundred to a few thousand people, and almost all fall under the category of what is called today *endangered languages,* defined as those likely to become extinct in the twenty-first century.

I was one of Pike's students. I personally met Ken in Mexico City in 1957, when I was an undergraduate student majoring in anthropology at the University of the Americas there. I asked him for an appointment, and he answered, "How 'bout now?" We talked for over an hour that night. That, and hearing him give a guest lecture the day before, changed the direction of my life. The following summer of 1958 I headed up to Norman, Oklahoma, where I took my first linguistic course under him at the University of Oklahoma. I attended two more summers under him at Norman (1960 and 1961), joined his organization (SIL), and left a few weeks later for the Philippines with my bride, Janet Headland.

In April 1962 Janet and I began living with the Casiguran Agta, a Negrito hunter-gatherer society of just 600 people living in eastern Luzon. After one year of our using Pike's techniques for learning an unwritten language, applying his monolingual approach with our Agta hosts, Pike came to the Philippines, in 1963, to conduct one of his linguistic workshops for SIL workers. For three months he met daily with several SIL field linguists at the SIL workshop center, including Janet and me and our two Agta "informants" (as we called them then). The two data papers we wrote at that workshop were eventually published in scholarly outlets (J. Headland, 1966; T. Headland, 1967). Pike coauthored them and had two of his senior research assistants submit them for publication. Our names, however, appeared as the sole authors in both essays. Even though Pike did much of the work, he didn't include his name as a coauthor on either essay. That was often his style. He was not even acknowledged in a footnote.

In later years, after Janet and I left the Agta in 1986 to move back to the United States, Ken and I became close friends. He and Evelyn and Janet and I got together often for social fun, often in their home or ours (just 10 minutes apart in Dallas). Ken and I began attending anthropology conferences together, speaking together at seminars, and coauthoring essays. Some people think of me as Pike's last student, since I was still learning from him up until he died. We had our last visit sipping tea together in his home, just four days before he passed away.

PIKE'S CONTRIBUTION TO ANTHROPOLOGY

Pike's major contribution in anthropology was his development of the *emic/etic* concept. First coined by Pike in 1954, the two terms are found in common usage in the vocabularies of most anthropologists today, and the distinction between emics and etics has proved very useful to them (see Franklin, 1997). In fact, most anthropologists today use insights about the different perceptions of reality of various cultural groups as the principal conceptual tool of their trade. The *emic/etic* distinction underlies a basic contribution to modern anthropology, a tool for understanding other cultures. Anthropologists make their living at least partly because of their unique ability to make the distinction between *emic* and *etic*.

The highlight of Pike's role in the American Anthropological Association came in 1988. At the AAA's annual meeting that year in Phoenix a public debate was scheduled between Pike and Marvin Harris on their differing uses of the *emic/etic* concept. The debate, which went on for four and a half hours with 600 anthropologists in the audience, was vigorous but cordial. It resulted in a book titled *Emics and Etics: The Insider/Outsider Debate* (Headland et al., 1990). One unforgettable, amusing incident occurred during this otherwise serious dialogue. During the discussion period a man in the audience asked Pike a question. In answering him Pike was describing an incident that happened to him in Russia, but he couldn't remember a name. He then looked out over the audience and suddenly said, "Evelyn, are you out there? Who was that man we had dinner with in Moscow?" Evelyn was sitting in the back of the auditorium. She stood up and said, "Ken, that was Dr. So-and-So." Pike said, "That's right." And he finished answering the question. I was the symposium moderator at this debate; when I went to the microphone to call on the next person, I first said to the audience, "Let me stop here, colleagues, to tell you who that was in the back of the room. That was Kenneth Pike's wife, Evelyn Pike, and they are here with us this week celebrating their golden wedding anniversary." Everyone started to applaud. Then Pike, without a moment's hesitation, stood up, leaned across the table, and blew his wife a kiss. The audience, perhaps restless after four hours of sitting, broke forth with cheering and whistles. It was an entertaining moment in a long and otherwise humorless panel that anthropologists still remember today.

PIKE'S CONTRIBUTION TO RELIGION

Throughout his career Pike was keenly interested in the religious aspect of his work, as seen in his relationship with Wycliffe Bible Translators. He, Angel Merecías, and Donald Stark completed the translation of the New Testament into the San Miguel Mixtec language in 1951. Pike was above all a Christian philosopher. He was a convinced theist who influenced thousands of people toward religion. He wrote numerous religious articles and books. Such books include *With Heart and Mind* (Pike, 1996 [1st ed., 1962]) and *Mark My Words* (Pike, 1971). In *With Heart and Mind* Ken defended scholarly and intellectual approaches to Christianity, maintaining that Christian faith and academic scholarship can be intimately integrated. As Hugh Steven wrote (1989, p. 16), "To understand and appreciate Pike, one must know he is both scholar and Christian; that his faith in Christ is at once full of energy, without pretense and rooted in Biblical depth."

PIKE WAS A MULE!

Ken Pike never had any internal conflict integrating his personal faith in God with his scholarship, nor his call to missions with his professorship at Michigan. But this was a problem for some academics who wondered whether Pike left his brains at the door when he went to church. Pike wrote his *Heart and Mind* volume to help those people understand that he did not. He recently wrote two shorter essays describing his dual calls to missions (Pike, 1997b) and to linguistics (Pike, 1998a). And his sister, Eunice Pike (1981), wrote a biography of Ken to explain his unique integration of faith and learning. Pike once told this story to help people understand his role as a Christian scholar:

In 1980 while Evelyn and I were lecturing in China, we were honored at a dinner at Beijing Foreign Studies University. I was seated next to a Chinese gentleman who had just returned from lecturing at Berkeley. When he learned who I was he said, "Ah yes, I heard about you while I was in the USA. But I also heard you are a missionary. So which are you, a missionary or a linguist?" I thought fast and told him I was a hybrid, a mule. His expression caused me to explain myself. Mules are the result of breeding between a horse, wanted for its speed, and a donkey, wanted for its strength and ability to walk over rocks in the road. When you want to combine the two qualities you have a mule. So sometimes I'm a horse and sometimes I'm a donkey, but I'm always a mule. I am both a missionary and a linguist. [Recorded by Ruth Carr and Ken Pike in 1988, and used here with Carr's permission.]

An example of how this played out in Pike's life can be seen in some of the letters he received over the decades from scholars who were influenced by his quiet faith in God. Here is an example, a letter from a Russian scholar who Pike befriended when he was a Fulbright scholar in Moscow in 1988, before the fall of the Soviet Union in 1991.

Dear Professor, Thank you for your paper. . . . It is a good contribution to the development of our mentality. . . . Many innovations are expected here [as a result of recent political upheavals]. . . . Thanks to the depolitization of higher educational establishments, there has appeared a possibility of abolishing party meetings, party bureau sittings, and so on. . . . My loss of belief in Stalin caused my sessation from the CPSU [Communist Party of the Soviet Union]. Paraphrasing the statement made by Pascal, "There is a God shaped vacuum in the heart of every person and it can never be filled by any creative thing but can be filled by God we may know through Christ." I must admit that the vacuum formed in my heart is open to Christ but it is not very easy for a former fanatic communist and atheist to make a decision. Your book *Ken Pike: Scholar and Christian* [Eunice Pike, 1981] is especially dear to me now as it depicts the ideal of the Christian gentleman in work and life. . . . So I am trying to study Christianity and wish I would ever dare to go to Shrebu [pseudonym] Church to be baptized. . . . I have to queue for hours to buy something eatable or salt cabbage to last through the winter. But man shall not live by bread alone. Yours sincerely. [Written to

Kenneth Pike, dated November 20, 1990, from Russia; the original letter was written in English, and is archived in the Pike Special Collection; the spelling here remains as in the original. Words added for clarification are in square brackets.]

And there are many other academicians today who are grateful for Pike's scholarly help in the past. Forty-six scholars came together after Pike's death to put together a large volume, a collection of their own essays, in honor of Pike's memory (Wise et al., 2003). It stands today as a tribute to Pike's life and scientific influence.

CASE 75-2: PIKE'S ORGANIZATION ACCUSED OF ETHNOCIDE

Not everybody liked Pike, however. He was criticized often enough in the academic world because of his tagmemic theory. But he was mainly controversial because of his religion and because he was the president of SIL, an organization whose primary aim is the translation of the Bible into preliterate indigenous languages. Pike wrote replies to the public charges against SIL and its sister organization Wycliffe Bible Translators. The first printed criticism of Pike came from David Stoll (1974), now an anthropology professor at Middlebury College, in the *Michigan Daily* when Stoll was just 23 years old. Pike replied in the same newspaper (Pike, 1974). In 1975 some members of the American Anthropological Association filed a formal charge of ethnocide (destroying indigenous peoples' cultures) against the SIL to the AAA's Committee on Ethics (the latter's Case 75-2). In May of that year the AAA wrote a letter to the SIL describing the complaint and inviting SIL to formally respond; Pike replied in a 15-page letter dated May 21. After spending a year investigating the charges, the AAA's Committee on Ethics submitted its report to the AAA Executive Board. The committee decided unanimously in favor of SIL against the complainants. In a letter dated September 20, 1976, to

Pike, AAA Executive Director Edward Lehman stated, "At its 85th meeting in May [1976], the [AAA] Executive Board accepted that [Committee on Ethics] recommendation, also by a unanimous vote." A more recent attack from anthropologists, this time accusing SIL of genocide, was published in *Anthropology Newsletter* in 1997 (Edelman, 1997). Pike replied also to that, and the AAA published it in a later issue of the newsletter (Headland and Pike, 1997). To accuse Pike's students of genocide was so extreme that even long-time SIL critic Stoll (1997) criticized Edelman's editorial.

* * *

Ken Pike was an extraordinary man. He loved life. He had a passion to challenge people to think. He wrote poetry. He laughed. He used his mind to solve linguistic puzzles and share the methods he discovered with others. He furthered science. He was a true scientist, scholar, philosopher, poet, pioneer, and author. He was a man who shared his life, knowledge, and love with countless people around the globe. He was a gentleman in the highest sense of the word, an elegant man who noticed and spoke with the most unpretentious person in a crowd; a shy child would catch Ken's eye, and he would engage the child in conversation.

In 1999 SIL began work on what has now become the *Kenneth L. Pike Special Collection*, a part of the *Language and Culture Archives* of SIL International, in Dallas, Texas. This archival storehouse today includes thousands of documents on or by Pike, his wife Evelyn Pike, and his sister Eunice Pike. His correspondence collected there spans almost 70 years. The collection is open to scholarly academic researchers.

Kenneth Pike is survived by his wife, Evelyn; three adult children, Judith Schram, Barbara Ibach, and Stephen Pike;

three grandchildren and two great-grandchildren; and one sister, Eunice V. Pike.

Pike's poem "The End" expresses the feelings of his students and colleagues.

The End

Regarding Daniel 12:9-13, and "the end of the days."

In tears, then joy!
Life in contrast
Sets the pace
Of learning
Good, through bad . . .

Both now and "then"
Hold to trust,
In God, in time
To light our stars,
Forever there.

(Pike, 1997a, vol. 2, p. 102)

NOTES

1. I am grateful to Karl Franklin, Evelyn Pike, and Calvin Hibbard (archivist of the Townsend Archives at SIL in Waxhaw, North Carolina) for helping me check facts and dates of the events reviewed in the present memoir. The author of this memoir wrote an earlier and shorter version of this memoir that was published in *American Anthropologist* (vol. 103, no. 2) in June 2001.

2. A database list of Pike's publications can also be found online at <www.sil.org/acpub/biblio/>.

3. In Pike's 2001 posthumous essay he reminisced about his dealings with America's early twentieth-century linguists, including Edward

Sapir (1884–1939), Leonard Bloomfield (1887–1949), Charles Fries (1887–1967), George Trager (1906–1992), Charles F. Voegelin (1906–1986), Bernard Bloch (1907–1965), Zellig Harris (1909–1992), Charles Hockett (1916–2000), Michael Halliday, (1925–), and Noam Chomsky (1928–), and with early American anthropologists Leslie White (1900–1975), Marvin Harris (1927–2001), and Dell Hymes (1927–).

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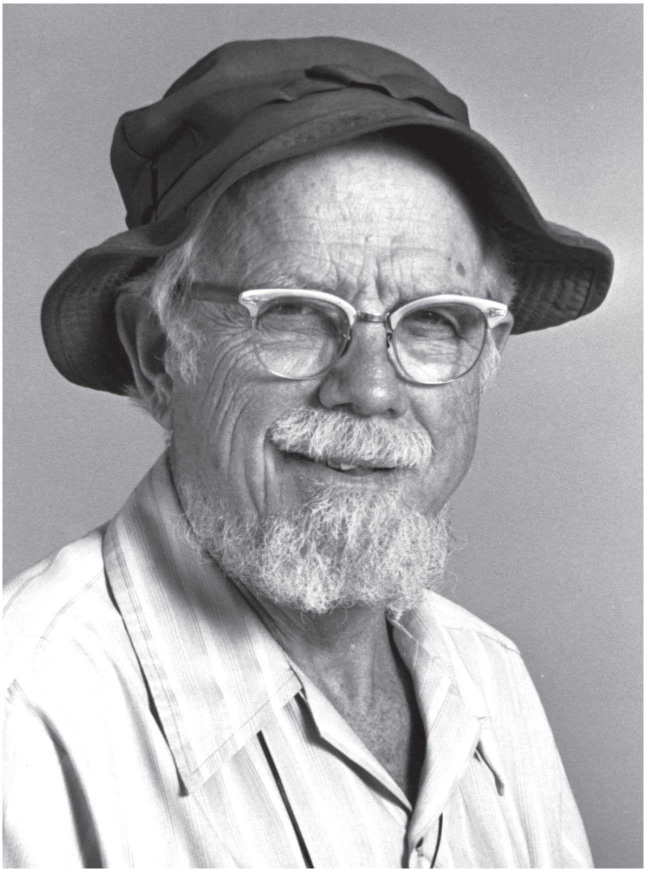

CHARLES MADERA RICK

April 30, 1915–May 5, 2002

BY STEVEN D. TANKSLEY AND GURDEV S. KHUSH

TO CALL CHARLES M. RICK the father of tomato genetics would not be an exaggeration. In the 1940s Rick began a series of studies that transformed tomato from a mere garden vegetable to a model organism. He was adept with the tools of reductionism (e.g., cytology, mutagenesis, biochemical genetics), yet he never succumbed to the reductionist's view of life. Rather, he synthesized all his observations and research into an integrated view of tomato biology, evolution, and biodiversity. Using this comprehensive approach he developed the biology not only of cultivated tomato but also the entire tomato genus, *Lycopersicon*. He was a naturalist and adventurer, once aptly described as Charles Darwin and Indiana Jones rolled into one. Rick traveled extensively throughout the Andean region of western South America collecting the wild relatives of tomato. The result was the development of an invaluable germplasm collection now housed at the C. M. Rick Genetics Resource Center at the University of California at Davis. This collection has become the cornerstone of tomato breeding and genetics and is the source of most of the major disease resistance genes that now characterize modern tomato cultivars. His legacy also includes many seminal papers on topics ranging from the evolution of mating systems to deletion mapping in tomato and a host of scientific protégés (offspring), including the two authors of this tribute, who were influenced by his evolutionary approach to plant genetics and infused with his enthusiasm for biological inquiry and the wonders of the natural world.

THE BEGINNINGS

Charles (or Charley as he was affectionately known) was born in 1915 in Reading, Pennsylvania. He grew up working in his father's peach orchards, and as an active member of the Boy Scouts he engaged in many outdoor activities. These early experiences were to shape Charley's life as a lover of nature, especially plants. Rick began his academic career at Pennsylvania State University, majoring in horticulture and received a bachelor's degree in 1937. It was at Penn State that Charley met and married Martha Overholts, who was to be his life partner and frequent companion on his many expeditions to the Andes and South America. From Penn State Charley matriculated at Harvard University and found a niche working under the advice of professors Karl Sax and M. M. East, pillars in the burgeoning field of cytogenetics and quantitative genetics. Surrounded by Harvard's Arnold Arboretum and embedded in the labs of Sax and East, Charley began to forge the marriage between natural variation and genetics—a theme that would more than anything else characterize the rest of his career.

TOMATO GENETICS—THE EARLY DAYS

Rick received his Ph.D. from Harvard in 1940, just as the United States was beginning to mobilize for war. Finding a limited demand for plant cytogeneticists at this time, Rick took an appointment in the Division of Truck Crops at the University of California at Davis, which had earlier served as the agricultural research station for the University of California at Berkeley. Rick later acknowledged that he was uncertain about the future of a basic plant geneticist in an agricultural setting. Nonetheless he busied himself in exploring a variety of topics including documenting the genetic changes induced by X ray in pollen of *Tradescantia* and the origin of polyembryony in asparagus. It wasn't until a stroll through a nearby tomato field with John MacGillivray, a fellow professor in the department, that Rick fell in love with tomatoes. During this walk MacGillivray intimated to Rick that while pollen irradiation might be of academic interest to some, of greater practical interest was why there were "bull" tomatoes and couldn't Rick "do something about them"? For those readers who are not so familiar with tomato lingo, "bull tomatoes" is not a derogatory term but rather refers to plants that are very large and typically barren of fruit. They occur at a relatively low but consistent frequency in tomato fields. Bull plants produce no useful fruit and because of their large size, they shade adjacent plants. How the skills of a cytogeneticist were relevant to unruly tomato plants was not at all clear to Rick, but he dutifully took on the project. To his surprise he discovered that a large proportion of these unusual plants were spontaneous triploids, hence explaining their reduced fertility. More importantly, these spontaneous triploids provided a ready source of trisomic plants (plants with a single extra chromosome). This eventually led to a comprehensive description of all 12 primary trisomics, which became the cornerstone of tomato cytogenetics and linkage mapping (see next section).

In 1948 Rick began a series of expeditions to South America to seek out the wild relatives of the cultivated tomato. He was a keen observer, and during these seed-collecting trips he took extensive notes on many aspects of population biology (including observations of pollination systems and insect vectors) and geographical and environmental features of the habitats in which the plants were found. He made careful preparations of herbarium specimens and kept notes on each plant in the wild populations from which seed was individually harvested and catalogued. As a result these collections became not only a source of novel germplasm for tomato breeders and geneticists but also provided the basis for subsequent systematic and population biology studies of species and races from the tomato genus, *Lycopersicon,* and its sister genus, *Solanum.* Working with these materials, Rick and colleagues were able to establish the systematic relationships of thousands of wild tomato populations and to group them into species clusters and geographical and compatibility races.

CYTOGENETICS TOUR DE FORCE

From the 1950s through the 1970s Rick was heavily involved with cytogenetic studies of the tomato genome. The beginning of this period can be traced to collaboration with D. W. Barton, a graduate student in the laboratory of S. W. Brown at the University of California at Berkeley. Brown had shown that all the individual chromosome bivalents of tomato could be identified at the pachytene stage of meiosis. He numbered the chromosomes in decreasing order of length. Working together, Rick and Barton applied pachytene analysis to identify the extra chromosome in each of the primary trisomics of tomato. Rick then used these trisomic stocks to associate mutant genes with specific chromosomes by virtue of their modified segregation ratios. In this way 10 of the 12 tomato chromosomes were associated with specific genetic linkage groups represented by mutants; however, a persistent puzzle was that no genes could be assigned to chromosomes 11 or 12.

It was in hopes of solving the chromosome 11 and 12 mystery that Rick invited one of us (G.S.K.) to join his group in the early 1960s for a six-month postdoctoral study. That short-term study eventually turned into a highly productive and very gratifying seven-year stay. In an effort to assign markers to chromosomes 11 and 12 we employed a pollen irradiation technique. Irradiated pollen from wild type plants was used to fertilize genetic stocks homozygous for mutant loci that had hitherto not been assigned to the other 10 chromosomes. Mutant progeny (ostensibly hemizygous for the mutant loci) were selected from these crosses and subjected to pachytene analysis for possible chromosomal deletions. When stocks containing the *a-hl* mutant loci were examined in this manner, we were able to definitively assign them to chromosome 11 by virtue of their association with detectable deletions on this chromosome. (It turned out that the original chromosome 11 trisomic stock was actually a tertiary trisomic involving chromosomes 7 and 10, which explained the original difficulty in assigning genes to this chromosome.) In a similar way we eventually assigned two markers (*alb* and *fd)* to chromosome 12, completing the tomato genetic-linkage map.

These pollen irradiation experiments became a primary source of new stocks for Rick's burgeoning cytogenetics endeavors. Amongst the progenies sired by the irradiated pollen were plant lines with a variety of novel phenotypes. Cytological examination of these stocks revealed three classes of mutant that proved especially interesting: (1) tertiary monosomics in which the short arms of two chromosomes are missing and the long arms of those same chromosomes are fused at the centromere; (2) monoisodisomics, which carry a normal chromosome inherited from female parent but an isochromosome from the male parent; and (3) arm deficiencies, which lack an entire short arm of one chromosome. These stocks in turn gave rise to other cytogenetics stocks (e.g., telotrisomics, secondary trisomics, and tertiary trisomics). Together these lines proved extremely useful for genetic studies. It was during this intense cytogenetic period of Rick's career that he and his students and postdocs used these stocks to pinpoint the physical position of 129 loci, to orient each linkage group with respect to chromosomal orientation, and to localize all of the centromeres of the tomato genome. As a result of these studies tomato became the first dicot in which the genetic linkage map was integrated with the pachytene physical map and thus a model organism for genetics. This integrated genetic-physical map of the tomato genome still guides geneticists and molecular biologists today.

EVOLUTIONARY AND POPULATION GENETIC STUDIES

Rick had a lifelong interest in plant evolution and systematics that was nurtured by the intellectual environment of UC Davis, which boasted many prominent evolutionists. Upon return from his first collecting expedition to South America, Rick embarked on a series of studies aimed at unraveling the systematics of wild tomato species, mapping out the distribution of genetic variation, and determining the biological and geographic factors responsible for these attributes. Rick was both comprehensive and multi-pronged in his approach, using geographic, morphological, cytogenetic, sexual cross-compatibility, and in latter years molecular data to draw inferences. He recognized the importance of mating systems in determining the structure and extent of genetic variation in both wild and cultivated populations of tomato. While a number of wild tomatoes are obligate outcrossers owing to gametophytic self-incompatibility, others, including the cultivated tomato, are facultative outcrossers. Rick discovered that the dominant characteristic determining the degree of cross-fertilization (outcrossing) in these species is the degree to which the stigma surface is exerted beyond the anther cone. He further showed that this method of outcrossing was because of a co-evolution of floral structure and insect vectors found in the native habitats of the wild species.

Rick was in his late fifties when molecular and biochemical genetics were born. Nonetheless he was quick to incorporate molecular techniques in both his evolutionary and genetics research. He used isozymes to quantify and study genetic variation both within and between populations of each wild tomato species. The set of treatises produced during this period (1970s and early 1980s) is now the definitive source of taxonomy and genetic variation for species in the tomato genus. He and his students, including one of the authors of this memoir (S.D.T.), determined the inheritance of and genetically mapped virtually all the isozyme determinates used in these systematics studies. This resulted in the first molecular linkage map and was the predecessor of the DNA linkage maps that are so commonly used in plant genetic, molecular, and breeding studies today. Rick was also one of the first people to recognize the potential of molecular breeding. During one of the systematic studies of genetic variation in cultivated tomato accessions Rick discovered highly significant linkage disequilibrium between a rare isozyme allele and resistance to nematodes. He went on to show that the nematode resistance gene was tightly linked to the isozyme gene and that breeders had introduced the rare allele together with the resistance gene from a wild species. He correctly reasoned that the rare isozyme allele would provide a faster and more accurate screen for resistance than nematode inoculations. This particular assay is still in use by many tomato breeders and was the first example of marker-assisted breeding in plants, which is now the foundation of most crop improvements.

WILD SPECIES AND PLANT BREEDING

While Rick conducted basic studies on the systematics and evolution of the tomato genus, he always had an eye toward the agricultural applications of such research. He was a strong proponent of the use of wild germplasm as a source of novel genes for crop plant improvement. This was somewhat of a radical view as, even today, many plant breeders view wild germplasm as a last resort. As early as 1953 Rick showed that crosses between wild species and their cultivated relatives could reveal novel genetic variation of potential use in agriculture. He promoted the use of geographic and environmental factors as predictors of which wild accessions might contain useful traits. Working with accessions of *L. cheesmanii,* a species endemic to the Galapagos Islands, he discovered a non-abscising pedicel trait and showed that the underlying gene could be transferred to cultivated tomatoes. The result was tomatoes from which the pedicels and calyxes could be easily removed for mechanical harvesting. This trait is now widely used by modern tomato breeders. The list of useful genes that have been transferred from the wild species accessions that Rick collected, studied, maintained, and freely distributed would be much too long to include in this memoir; their impact on tomato breeding has been immeasurable.

Rick also introduced a technique that has had an incredible influence on plant genetics. In the 1970s he began a set of experiments in which he would exchange single chromosomes from a wild species into a cultivated tomato line by backcrossing. This work was made possible by the genetic linkage map that Rick and his colleagues had created. These "introgression lines" could then be used to study the effects of defined segments of wild-species DNA in otherwise isogenic backgrounds. While this work was limited by the resolution of genetic maps available at that time, the concept has proven to be powerful and has been exploited to enormous effect in tomato genetics and molecular biology—most notably by D. Zamir at the Hebrew University in Israel, whose studies are now a model for both plant and animals.

MENTOR, FRIEND, AND STORYTELLER

Charley Rick was advisor to more than 40 students and postdocs, most of whom have gone on to notable careers at major universities and institutions in the United States and several foreign countries. His style of mentoring was largely by example. His enthusiasm for and dedication to his research were contagious. His days started early, by 7:00 a.m. each day, with a bike trip to the office, where he could still be heard typing into the still of the night. Weekends were no exception, excluding time out for gardening, hiking, or weekend trips to his cabin at Bodega Bay. But for Charley this was not work, it was a way of life and a love for the tomato and all that it could reveal.

Rick had an encyclopedic memory and could recall details of almost everything he had read or experienced. A nature walk with Charley would open up your senses and awareness to everything around you, making you wonder how you could have ever missed those things that he so readily saw. He was also a fantastic storyteller: the Mark Twain of plant genetics. Fueled by a lifetime of expeditions and adventures, a wry wit, and an eloquence that is rare among scientists, Charley could keep an audience spellbound. Regardless of the topic he had a knack for taking the ordinary and making it extraordinary and could find the humor in almost every passing moment in life. He was an icon on the UC Davis campus, easily identified by his trademark khaki fishing hat that seldom left his head regardless of whether he was pollinating tomatoes or addressing a group of dignitaries.

Charley was a friend to so many people in so many walks of life. Though his relationship with each person was undoubtedly different, he had a way of making everyone feel special and directly connected to his energy and vitality. He was an avid supporter of the National Academy of Sciences, faithful in attending the annual meeting even when traveling became a physical difficulty, and he gave generously to the Academy to assure its future viability. While Charley's passing was a great loss to the Academy and the plant genetics community, a bit of his spirit is carried by all who had the good fortune of meeting this extraordinary individual.

Charles Rick is survived by his son, John, an eminent archaeologist and a professor at Stanford University; his daughter, Susan Baldi, a professor in both the Life Science and Health Science departments at Santa Rosa Junior College; two grandsons; one granddaughter; and one great-grandson.

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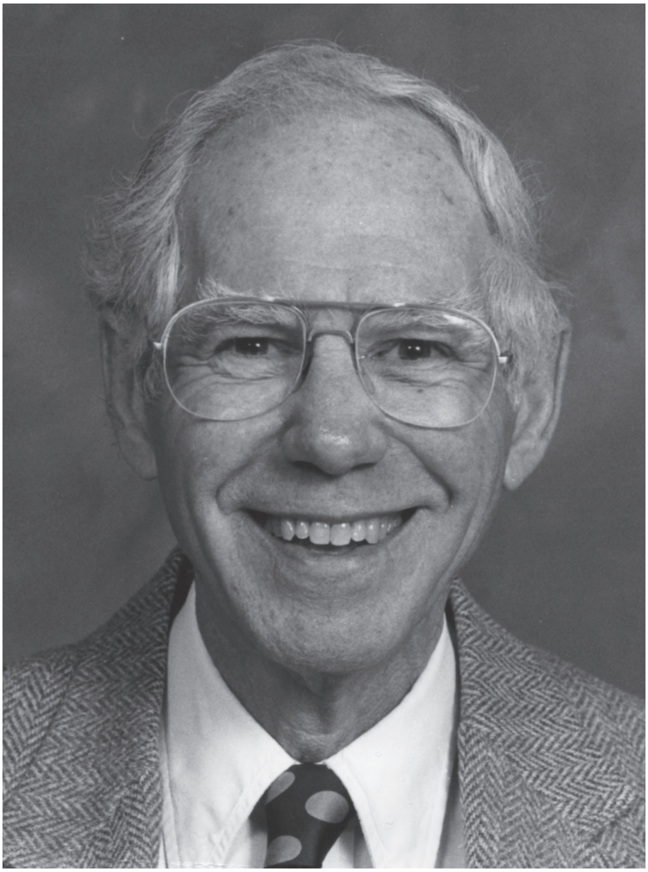

Courtesy of the Fermi Institute

Robert G. Sachs

ROBERT GREEN SACHS

May 4, 1916–April 14, 1999

BY KAMESHWAR C. WALI

On Sachs's eightieth birthday on May 4, 1996, Carolyn Sachs said, "Bob has three loves in his life: family, sailing, and physics." She went on to say, "However much I wanted, I never had him declare his order of preference, because I was afraid of the outcome." Indeed for the world of physics at large, although Sachs was known as an avid sailor and a loving patriarch of a large family, the highlight of his life was his loyalty and commitment to physics. Over the course of his scientific career, which spanned six decades, he made fundamental contributions to a wide- ranging area of basic research, including atomic, nuclear, and particle physics. A conscientious and meticulous teacher, Sachs guided the research of several graduates from the University of Wisconsin and the University of Chicago who have made their own mark in research.

To speak in broad terms Sachs believed strongly that "it is experiment what defines physics," not pure thought or pure mathematics however elegant and beautiful these may be. Secondly, the underpinning of a physical model or a physical theory should be the most general, well-established principles to derive constraints and construct models to test against experiments and provide new tests for further measurements. He (his students as well) avoided models based on extensive numerology and numerical analysis, which ultimately did no more than testing the general principles. Sachs characterized his approach as "phenomenological theory," and over the years with this methodology he and his students worked on groups of problems, each group spanning a period of years but overlapping to some extent either in time or content or both. There have been three such major groups of research activity: (1) electromagnetic interactions of nuclei and their constituents, the nucleons (neutrons and protons); (2) resonances and unstable particles; and (3) time reversal and CP violation.

Although Sachs's primary interest was teaching and research, when needed and called upon, he offered himself in service to the physics community. He was recognized for his contributions to questions relating to national and international energy policies, for his services to high-energy physics panels, and for his successful efforts in creating the Division of Particles and Fields. He served as associate laboratory director at Argonne National Laboratory (1964-68) and was in charge of its high-energy experimental research program at the new and then the most powerful 12 GeV accelerator, the Zero Gradient Synchrotron (ZGS). He later served as the director of the laboratory (1973-79). He also put in two terms as the director of the Enrico Fermi Institute (1968-73 and 1983-86).

Bob Sachs was born on May 4, 1916, in Hagerstown, Maryland, but his family moved to Baltimore, Maryland, in 1921, when Bob was five years old. Bob's grandfather came originally from Russia. His family name was Schabershovsky. When he married one of the seven daughters (and no sons) of a man with the family name of Sachs, he was adopted by his wife's family and acquired the name Sachs. Bob's father, Harry Maurice Sachs, was the oldest in a family of seven siblings from his father's second marriage after the death of his first wife. Bob's mother, Anna Green, was the daughter of an upper-middle-class Jewish family in Richmond, Virginia. Anna's father was an Austrian by birth and her mother was a Lithuanian.

Anna married Harry Sachs when she was only 16. Her father was strongly opposed to the marriage because she was too young to marry and, secondly, Harry, who came from a small town, did not measure up to his expectations for a son-in-law. Anna married without his knowledge, but with complicity of her mother, which led to her parents' divorce. She remained estranged from her father for the rest of her life, but remained close to her mother. In Baltimore Harry Sachs worked in the city printer's office during the day and took night classes and eventually got a law degree from the University of Maryland. Anna was an accomplished singer, who sang professionally and during services at the temple Oheb Shalom to supplement the family income. These were difficult days for the Sachs family as Bob was growing up.

Bob Sachs had his elementary education at John Eager Howard School number 61. After completing his high school education at the City College in Baltimore he entered Johns Hopkins University in 1933. By then he had realized that physics was his true calling and opted for a straight six-year Ph.D. degree course, bypassing the usual bachelor's and master's degrees. He began his graduate research as a student of James Franck and Maria Göpert-Mayer. Officially his thesis supervisor was Göpert-Mayer, but Edward Teller at the nearby George Washington University was his true mentor. At Teller's suggestion Sachs worked on the neutron-proton interaction potential in deuteron, which led to his first publication (1938). The values of the parameters of the Yukawa potential that he determined by solving the deuteron problem numerically and fitting them to the data proved to be extremely useful in nuclear physics of the time. They were used extensively until the advances in meson theory and the modification of the potential in light of new experiments.

After his graduation, during the years 1939-41, Sachs continued his association with Edward Teller as a research fellow at George Washington University. He worked closely with him in research on a great variety of physics, including atomic, molecular, and solid-state physics, as well as nuclear physics. Among several papers during that period there were two that had great impact. One of them established a very general result for the ratio of the frequencies of the transverse and longitudinal polar waves in polar crystals in the long wavelength limit (1940). It is known as the Lyddane-Sachs-Teller theorem in solid-state physics literature. The other had to do with the general problem of scattering of slow neutrons by molecular gases (1941). At the time, the results were of interest mainly in determining neutron cross-sections from measurements on molecular gases, but later they played a vital role in the study and analysis of neutron dynamics in gas-cooled reactors that became a part of the Atomic Energy Commission program.

In 1941 Sachs had spent just four months (March to June) as an instructor at Purdue University, when he received a call from Robert Oppenheimer to be "his" postdoctoral fellow at the University of California, Berkeley. He left Purdue, but just as he started research on a problem in meson theory his pure research career came to an abrupt end; following the Pearl Harbor tragedy and America's entry into World War II he became engaged mostly in classified applied research, the results of which could not be published in regular journals. First, he was called back to Purdue to serve as a house theorist to work on a project on crystal rectifiers. In 1943 he moved to Aberdeen Proving Ground in Maryland to become the chief of the air blast section of the terminal ballistics branch of the Ballistic Research Laboratory (BRL).

The project at Purdue concerned the development of improved detectors for microwave radar. The prevailing art of the manufacture of such detectors was very labor intensive and entailed high cost. The needed contact between the point of a metal wire and a silicon crystal in the detector had to be made manually by tickling the crystal with the wire. Sachs's assignment, under the direction of Karl Lark-Horovitz, was to make detectors that circumvented this problem and performed the way the known simple theory predicted. Although the effort did not lead to the solution of the practical problem in designing better detectors, it did lead to an understanding of why the ideal theory does not work and became an important contribution to the practical use of semiconductors. There were two types of Si and Ge semiconductors known as N type and P type. The theory of impurities explained why some of these semiconductors are of a specific type, but Sachs and Lark-Horovitz realized how specific impurities could be added to highly purified Si and Ge to make on demand either an N- or P-type semiconductor. Subsequently this discovery became extremely important in the production of transistors.

At BRL Sachs led a group that analyzed the tests of bombs and explosives. Aside from managing the group Sachs provided the interpretation of the results in terms of effectiveness in actual warfare, proposed new tests, did operations analysis on bombing, and carried out research on shock waves and blasts. During the course of this work Sachs and his group needed to know the effect of ambient pressure and temperature in order to analyze the results of their tests and for the evaluation of blast effects at high altitudes.

Sachs solved this problem in a very simple way by using scaling methods (1944).[1] The BRL was a laboratory of the Ordinance Department, which had no access to the work of the Manhattan Project at Los Alamos. The contrary was not true. It turned out that Sachs's solution was of great importance to the Manhattan Project. It had defied John Kirkwood, a distinguished theoretical chemist working at Los Alamos, who was trying to find the solution by detailed dynamical methods.

After the explosion of atomic bombs over Hiroshima and Nagasaki, the Ordinance Department requested Sachs to undertake a study of the comparative effectiveness in terms of both cost and strategies. This resulted in an unpublished report titled "Atomic Explosives for Defensive and Offensive Purposes" (1945). It was a far-reaching analysis that remained classified until 1964. An extract from this report appeared in the *Bulletin of Atomic Scientists.*[2]

At the beginning of 1946 Sachs joined the University of Chicago's Metallurgical Laboratory to work on the physics of a gas-cooled BeO, ceramic-moderated reactor for electric power generation, proposed by Farrington Daniels and hence known as Daniels Pile. When the laboratory became Argonne National Laboratory on July 1, 1946, and was transferred to the Atomic Energy Commission, Sachs was appointed director of the Theoretical Physics Division. He had multiple responsibilities, including recruitment of theoretical physicists, research on various designs of power reactors, and basic research in nuclear physics. He also commuted between Chicago and Oak Ridge, Tennessee, to give introductory lectures in the first nuclear engineering program set up in Oak Ridge. The program included a group of young engineers selected by interested industrial companies from among their employees as promising candidates to become the first industrial nuclear power engineers. Indeed many among this group became the principal leaders of the then developing industrial programs in nuclear engineering.[3] He was also successful in bringing some prominent theorists such as his former thesis advisor, Maria Göpert-Mayer, David Inglis, and Morton Hammermesh to Argonne.

Sachs's contributions to applied physics during this period included calculations of the critical sizes and temperature effects for reactor cores as well as investigations of various control methods. Most of this work and the work during the war years were presented in reports that were classified for a long time. Only a part of that work appeared subsequently in regular publications and found applications in basic research. For example, Sachs's work on relativistic shock waves during the war was subsequently cleared for publication (1946) and had significant impact on theoretical work in astrophysics.[4] Likewise, the paper with Fermi and Sturm (1947) on thermal neutron scattering, which arose from applied work on reactors, illustrated for the first time the power neutron diffraction for studying the properties of condensed matter. In nuclear physics it became instrumental in determining the nature of effective potentials in neutron scattering.

In 1947 Sachs joined the faculty of the physics department of the University of Wisconsin at Madison, marking the true beginning of his academic career in teaching and research. Wisconsin, which was a major center of experimental nuclear physics at the time, welcomed Sachs, who began to teach basic theory courses, including a course in nuclear theory to both experimental and theory students. Soon students graduating from Wisconsin found themselves to be leaders in Atomic Energy Commission projects and in nuclear engineering programs at Argonne National Laboratory as well as in academic positions throughout the country. The course on nuclear theory led to his writing a textbook on the subject, *Nuclear Theory* (Addison-Wesley, Cambridge, Mass., 1953).

At the time Sachs entered the field of nuclear physics, nuclear theory was concerned with the nature and origin of strong interactions of nuclear forces. The interpretation of the underlying nuclear structure, however, based on experiments that involved only strong interactions, had become a difficult task. Sachs's proposal was to use electromagnetic probes since the strength of the electromagnetic interaction measured by the fine structure constant was approximately 0.01 of that of the strong interactions. Consequently at relatively low energies they were expected to cause little disturbance to the underlying nuclear system. Moreover, their dynamical effects could be isolated and calculated using perturbation techniques.

Sachs's first seminal paper in nuclear physics was published before he joined the faculty at the University of Wisconsin. The paper, "Magnetic Moments of Light Nuclei" (1946), established what became know as the mirror theorem for the magnetic moments of mirror nuclei (pairs of nuclei such that neutrons and protons in one are replaced by protons and neutrons in the other). It was based on the assumption that magnetic moments of nucleons in a nucleus are additive and it led to a correlation between the observed magnetic moments and the internal angular momentum structure of light nuclei. The mirror theorem, with suitable modifications to include relativistic corrections and its application to several experimental situations, led to several important papers during 1946-48. It soon became evident, however, that the mirror theorem, combined with the assumption that the nucleon moments are additive, failed to give reasonable agreement with experiments. There was mounting evidence that there was a contribution to the total magnetic moment in addition to the sum of the spin and orbital moments of individual nucleons inside the nucleus.

Sachs recognized that the additional contribution could come from exchange currents due to the exchange of charged pi mesons between the nucleons. This was long before pi mesons were established experimentally. Persuaded by Fermi not to concentrate on a specific model, he developed a general phenomenological theory of exchange currents in nuclei (1948). This was based on a gauge invariant form of the Majorana exchange potential between nucleons. It led in subsequent years to a study of general consequences of gauge invariance. For instance, in the paper on the radiative transitions of a nonrelativistic system of particles (1951), Sachs (with Norman Austern) was able to give a general proof of the Siegert theorem[5] for electric multipoles of all orders. Among other important results Austern and Sachs presented the formal construction of magnetic multipole moments, including the exchange current effects from any gauge invariant Hamiltonian. They demonstrated that the Rayleigh cross-section for the scattering of long wavelength radiation from a neutral system of charged particles was a direct consequence of gauge invariance.[6] Further study of this formulation led to general expressions for nuclear transition amplitudes for various phenomenological forms of exchange and velocity-dependent interactions (1951, 1952) and an understanding of various electromagnetic properties of nuclear systems in light of then available data.

While primarily a nuclear theorist, Sachs foresaw high-energy physics as the new frontier field, the physics of the future in the early 1950s. He persuaded the department and the administration to hire faculty in this relatively new area and succeeded by the middle 1950s in making Wisconsin a center for high-energy experimental and theoretical research.[7] In his own research Sachs turned his attention from nuclear structure to the structure of nucleons, the presumed elementary constituents of all nuclei. He visualized the nucleon as consisting of a bare nucleon surrounded by a cloud of pi mesons. Because the strong interaction between the pi-meson cloud and the bare nucleon prevented the application of techniques based on perturbation theory, a new approach was necessary. Sachs introduced the general methods of his phenomenological approach to investigate the electromagnetic properties of the physical nucleon (1952), in particular the magnetic moments of the neutron and proton and the (static) neutron-electron interaction. His investigations suggested that the pion cloud consisted of highly correlated pairs and led to a model that could, in terms of the electromagnetic energy, account for the neutron-proton mass difference (1954). The later discovery of the rho meson in nucleon-nucleon collisions confirmed this feature of the nucleon structure.

This was the time in the 1950s when the pioneering experiments of Robert Hofstadter on electron-neutron scattering raised fundamental questions regarding the physical interpretation of the electromagnetic form factors and their physical interpretation in terms of charge and magnetic moment distribution inside the nucleon. The conventional Dirac and Pauli form factors had led to a paradoxical result in the interpretation of the measured electron-neutron interaction.[8] In a paper that had strong impact, Sachs, along with F. J. Ernst and K. C. Wali, derived certain combinations of the Dirac and Pauli form factors as the truly physically meaningful expressions, in the sense that they were the Fourier transforms of the spatial charge and magnetization distributions inside the nucleon (1960). This not only removed the paradox but also led subsequently to new insights as regards the high-momentum transfer behavior of the form factors (1962).

In the late 1940s and early 1950s, when the world of elementary particles was ushered into a new era with the discovery of a host of new elementary particles, Sachs, like many other theorists, turned his attention to the study of their strange properties. He proposed a classification scheme for elementary particles based on a new additive quantum number called "attribute" (1955). This paralleled the Gell-Mann-Nishijima classification scheme for hadrons based on "strangeness," but went beyond it since it included leptons as well.[9] With Treiman he wrote a classic paper on $K-\bar{K}$ interference (1956) that was to become a forerunner for a number of theoretical and experimental discoveries in K-meson physics. He developed a phenomenological theory based on S-matrix approach to describe their anomalous decay properties (1961), which incidentally proved to be a powerful tool to describe unstable particles in relativistic field theories.

The discrete symmetries—the charge conjugation (C), space inversion or parity (P), and time inversion (T)—have presented a great challenge to particle theory. The invariance of interactions under their combined operation, known as the CPT theorem, is regarded as one of the sacred principles of theoretical physics, since it is based on some very general principles, such as locality, Lorentz invariance, and causality. The discovery of parity violation in weak interactions in 1957 naturally raised the question regarding the invariance under the other two discrete operations. Sachs became interested in the subject when some preliminary experiments indicated a violation of the $\Delta S = \Delta Q$ rule in the semi-leptonic decays of neutral K-mesons. Along with Treiman, Sachs analyzed the data, compared it with theory, and showed that the results implied the violation of CP invariance (1962). They suggested several experiments to test the conservation of CP in neutral K decays.[10] Then onward, the study of the discrete symmetries—C, P, and T—their conservation and their violation became an important part of Sachs's research. Thus when CP violation was discovered in 1964, it was implicitly assumed that there should be a compensating T violation in order to preserve CPT invariance. Sachs felt strongly that it was extremely important to test CPT invariance independently, just because it was based on such general principles. Any violation, however small, implied the failure of some fundamental principle that goes into its proof. He proposed experiments to test the validity of CPT (1963) and tests of T violation (1990) independent of its expected violation connected with CP violation (assuming CPT invariance). Along with B. G. Kenny he examined the role of nonhermitian interactions in the proposed tests of T violation (1973, 1986). The monograph *The Physics of Time Reversal* (University of Chicago Press, 1987) covers all aspects of time-reversal invariance beginning with classical physics, extended to the quantum world of atomic and nuclear systems and finally to quantum field theories of elementary particles.

In 1963 Sachs's teaching and research came to a temporary standstill when he moved to Chicago to be the associate laboratory director for high-energy physics at Argonne National Laboratory (ANL). On December 4, 1963, during the gala dedication dinner of the Zero Gradient Synchrotron (ZGS), Albert Crew, the director of ANL, announced Sachs's appointment. As the associate director, whose responsibilities he assumed on February 1, 1964, Sachs was responsible for directing the operation of the ZGS and supervising the physics program at the new accelerator. He faced a serious challenge since the construction of the ZGS at Argonne had a history of controversy between the Atomic Energy Commission (AEC) and the Midwest universities. For much of the Midwest university community Argonne was not a natural site for an accelerator for basic research in high-energy physics. It had no record of high-energy physics activity. It was a multipurpose government laboratory, operated by the University of Chicago, with programs oriented mainly toward applied research.

Physicists in the Midwest universities wanted an accelerator of their own in the Midwest, patterned after the Brookhaven National Laboratory in the east, which had been created and operated by the Associated Universities, Inc. (AUI). They had formed a Midwestern Universities Research Association (MURA) and had proposed the construction of a high-intensity, 12.5 GeV machine (MURA accelerator), to be built in Madison, Wisconsin. In spite of its support from the Ramsey panel,[11] President Lyndon Johnson rejected the project based "strictly on competitive economic considerations." But to lessen the blow of rejection the President also directed Glen T. Seaborg, the AEC chair, "to take all possible steps to make possible an increase in the participation of the academic institutions in the Midwest in the work of the Argonne National Laboratory."[12] Johnson went on to say that he fully supported the centers of scientific strength in the Midwest. He felt certain that with the right cooperation between the government and the universities, a great deal could be done to build at Argonne the nucleus of one of the finest research centers in the world.

As a result the contract for Argonne and the University of Chicago was replaced by a tripartite contract between a consortium of Midwest universities (called Argonne Universities Association), the University of Chicago, and the AEC. Earlier Roger Hilderbrand, who was Sachs's predecessor as associate laboratory director, and E. E. Goldwasser, acting on behalf of the potential users, had established a ZGS Users' Group. However, the users, who were still harboring feelings of disappointment at the rejection of the MURA accelerator, were not particularly friendly to Argonne. The fact that Sachs came from Wisconsin with a strong association with the user community and had played an active role in MURA deliberations did not make much difference. Recalling those initial days of his appointment, Sachs said, "I found a certain chill come over our relationship the minute I took the ANL job. I had no friends left!"[13] But in a short time much of this bad feeling faded away. The Users' Group, as conceived by Hilderbrand and Goldwasser and as implemented by Sachs, was to become a model for organizing high-energy physics research programs worldwide.

According to Sachs he was free of strong prejudice because as a theorist he had the advantage of knowing so little about matters to be decided. At the same time he suffered from the disadvantage of not knowing enough about experiments to trust his own judgement.[14] To overcome the latter and in search of the needed guidance and insightful input from the experimental side, he appointed Thomas H. Fields as the director of the High Energy Physics Division. Together, focusing on young people and encouraging them to start their own experiments, they built a highly productive research effort locally at ZGS.[15] Sachs also gave strong support for the particle detector projects that were already set in motion by his predecessor with groups at Midwestern Universities. These included, for instance, the design and construction of a 7^0-separated beam, a 30-inch hydrogen bubble chamber, and a 40-inch heavy liquid bubble chamber. These detectors, as well as a number of new electronic particle detector systems, enabled the ZGS university-based users to make an ongoing series of notable physics contributions in topical areas such as hadron spectroscopy, exploration of quark model effects, and weak interactions.

President Johnson, in keeping with his promised support for the full development of research facilities in the Midwest, put in a special grant in his fiscal year 1966 budget for upgrading the ZGS. There were two competing proposals: a new injector consisting of a 200-MeV linear accelerator and a 12-foot hydrogen bubble chamber with an additional external proton beam experimental area. It was Sachs's responsibility to decide between the two. He opted for the second choice with a superconducting magnet for the bubble chamber. It was a much too risky venture as it was an entirely new generation of chambers. A superconducting magnet of the required size, which would be by far the largest in the world, had never been built. Nonetheless Sachs took the bold step, and under the leadership of Gale Pewitt, the bubble chamber and the magnet were designed and built successfully. It was indeed a great triumph in the design, construction, and operation of large chambers. One of the immediate results was that it enabled the study of GeV neutrino interactions with free nucleon targets.

Sachs also encouraged special projects whose scientific goals were outside high-energy physics research but could make innovative use of the skills and facilities at the ZGS complex. Some of these projects played crucial roles at ANL after the ZGS accelerator was shut down in 1979. For example, the intense pulsed neutron (spallation) source at ANL is based on pioneering experiments done at the ZGS. For many years thereafter this neutron source facility has made use of the ZGS booster accelerator and the second proton area.

During the years as associate director Sachs was also the regional secretary of the American Physical Society for the central states. By 1968 the ZGS program was running smoothly. The Users' Group had extended beyond the Midwest to include all high-energy and particle physicists in the United States. Through this influential group Sachs had at hand a means independent of government to make management decisions and decide on priorities concerning the future of high-energy physics. To institutionalize such capabilities he proposed and succeeded in reorganizing the American Physical Society, with divisions representing the major subdisciplines of physics under the control of the membership. The Division of Particles and Fields was his creation.

With ZGS running smoothly Sachs resigned in 1968 as the associate laboratory director to return to full-time research, teaching, and other academic responsibilities at the University of Chicago. But this dream proved to be very short lived. In October of that year George Beadle, president of the university, persuaded him to be the director of the Enrico Fermi Institute (EFI). Sachs undertook immediately the task of revitalizing the institute. He perceived that the tradition of excellence established during the Fermi years was slipping away. While the EFI had an excellent group of senior faculty, it needed new faculty, not only to join ongoing programs but also to initiate the programs of the future. It required a different style of research, different from the individualistic traditions among the seniors. With the advent of the Fermi National Laboratory Sachs was able to set successfully in motion the recruitment of new and young faculty both in theory and experiment. Although this was a full-time effort, Sachs taught courses for about one quarter out of each year and worked with several graduate students. He also served as the chair of the panel on elementary particle physics,[16] a part of the Physics Survey Committee of the National Academy of Sciences chaired by D. Allan Bromley. The report, *Physics in Perspective*, the outcome of a two-year effort, described the status and prospects of each subfield of physics; it included projections of budgetary needs and set priorities.

In 1973 just as he was preparing again to devote his full-time energies to research and teaching, Sachs was called on to return to Argonne National Laboratory as its director. The laboratory was in deep trouble owing to severe budget cuts for fiscal years 1973 and 1974. The morale was low, as the cuts meant the loss of jobs for several hundred employees. The ZGS was 10 years old; its end was in sight with newer and ever more powerful machines on the horizon. Argonne's largest program, the Liquid Metal Fast Breeder Reactor (LMFBR) was also faltering. Milton Shaw, the AEC's director of reactor development, who often clashed with Argonne's academically oriented reactor designers, saw Argonne as incapable of proceeding with the program along the lines he perceived, namely, toward a commercial breeder reactor.

Fortunately for Sachs, within three months of his taking over the directorship (ironically on April Fool's Day in 1973), Dixy Lee Ray was appointed the chair of the AEC. Milton Shaw resigned. The Nixon Administration, convinced of the looming energy crisis, launched a new plan under which the AEC and its national laboratories would form the core of a new energy research agency responsible for developing energy technologies and applications ranging from fossil fuels and nuclear reactors to solar energy and energy conservation. Nixon, in addition to providing the AEC an additional $100 million on energy research in fiscal year 1974, asked Dixy Lee Ray to develop a five-year national energy plan for the expenditure of $10 billion.

Sachs was called on to serve on the Senior Management Committee to advise the chair of the AEC on the new energy initiative. The energy crisis and the new energy plan meant for Argonne potential recovery from the budget crunch of 1973. Sachs immediately started to expand the laboratory into new areas of energy research. He found out that there were many individual members who, aware of the situation, had been looking into the laboratory's broader capabilities in energy research. The laboratory had set up panels and task forces to look at energy options other than the nuclear option. One such panel was the solar energy panel that was struggling with the problem of tracking the Sun, using focusing devices to obtain useful solar energy. From his days as associate laboratory director for high-energy physics Sachs recalled the work of Roland Winston, a ZGS user from the University of Chicago. Winston had invented a unique nonimaging (nonfocusing) light collector for use in a Cerenkov counter in high-energy physics research. Sachs encouraged him to look into the possibility of using it to collect sunlight for solar energy systems. Winston and his collaborators soon established that nonimaging concentrators can be designed to focus sunlight efficiently throughout most of the day without ever moving. No tracking was necessary. It opened up a whole new possibility of large-scale deployment of nontracking solar concentrators.

Within a few years the laboratory recovered from its major troubles. The AEC became the Energy Research and Development Administration, followed by the establishment of the Department of Energy. Sachs felt he had accomplished his task and that it was time to return to the university to pursue his main interest, namely, research and teaching. He did so in 1979, but had to serve another term (1983-86) as the director of the Enrico Fermi Institute before he could devote himself full-time to this pursuit.

The fundamental questions concerning the discrete symmetries C, P, and T were to remain the main focus of his research during the last two decades of his life. In spite of the great success of the Standard Model of strong and electroweak interactions, it is recognized that it is not a fundamental theory. Of its many shortcomings the lack of understanding of the origin and the strength of CP violation in weak interactions stands out as one of the great mysteries of theoretical physics. With the advent of quantum chromodynamics (QCD) a new problem arose concerning what is called "strong CP violation." Sachs devoted himself almost exclusively to an understanding of some fundamental questions concerning this problem, which at the root depends on the definition of QCD vacuum (1994). His last two published papers (1994, 1997) delve deeply into the problem. His subsequent detailed exploration relating the origin of strong CP violation to the early Universe has remained unpublished.

Bob Sachs was devoted to his family just as much as to his academic pursuits and responsibilities. He is survived by his wife, Carolyn L. Sachs; five children (Judith Crow, Portola Valley, California; Joel Sachs, Arlington, Massachusetts; Rebecca Norris, Maynard, Massachusetts; Jennifer Sachs, New York City; and Jeff Sachs, Basking Ridge, New Jersey); three stepchildren (Jacqueline Wolf, West Newton, Massachusetts; Kate Wolf, Lincoln, Massachusetts; and Thomas Wolf, Brookline, Massachusetts); and fourteen grandchildren. Judith and Joel were his adopted children from his marriage to Jean Shudofsky (nee Jean Woolf) on December 17, 1950. Jean Sachs died on January 27, 1968, after a prolonged battle with cancer beginning around 1960. Affectionate and charming, Jean made the Sachs home a warm and friendly place for all Bob's students and associates.[17] After her death Franklin Levin, a former student, introduced Sachs to Levin's "beautiful and very intelligent" sister Carolyn Wolf, whose husband had died in a tragic plane crash a year before. They married on August 21, 1968.

The extended family came together often—for birthdays, holidays, and sometimes to sail together. "As the patriarch of a large brood," Rebecca Norris says, her father "spoke willingly and enthusiastically at family occasions such as weddings, anniversaries, and bar and bat mitzvahs. He looked the role—tall and slim with his bushy eyebrows and hair a little wild—and his speeches always had the right touch. They were interesting, warm, funny, impeccably delivered, and perhaps most important of all, brief. He was the quintessential grandfather."

To his colleagues, students, and associates Sachs was caring, loyal, and responsible. He was known to hold strong opinions, but was always forthright and a man of unquestionable integrity. He was tireless in working for institutional developments. At both Wisconsin and Chicago he enjoyed great respect from deans, presidents, and provosts. He made sure that the teaching faculty was the backbone of a university.

Sachs stood for fairness and internationalism in physics. Throughout his scientific career, along with his own research efforts, Sachs brought forth opportunities for others, particularly for the young. He worked closely with his students and his devotion to them was legendary. "I first met Bob Sachs in 1959 at CERN sitting at a lakeshore café," says R. F. Sawyer. " He offered me a post-doc job and said it is pretty much like this in Madison. It is a good place to do physics. His enthusiasm and energy had made the department in Madison a fine place to do particle physics." Sachs was responsible for Roland Winston's outstanding career by directing him from high-energy physics to solar energy research.

The summer institutes Sachs initiated at the University of Wisconsin during the years 1960-64 was a prime example of his concern for the physics community at large. It was a time when even the best of the younger physicists in U.S. universities had to interrupt their research during the summer and seek employment to supplement their income. That was an era when few university faculty members had summer support covering their salaries. Sachs's proposal to mitigate the situation by forming a summer institute for well-qualified theorists was welcomed by the National Science Foundation. With the necessary support for travel and salaries for the participants the summer institutes became unique in fostering free exchange of ideas among a rare combination of senior and distinguished and young, upcoming researchers not only from United States but also from all over the world.[18]

Sachs was a Guggenheim fellow (1959-60); received honorary doctor of science degrees from Purdue University (1967), University of Illinois, Circle Campus (1977), and Elmhurst College (1987); was elected to membership in the National Academy of Sciences (1971); and served as the chairman of the Academy's Class I (Physical and Mathematical Sciences, 1980-83) and chairman of its Physics Section (1977-80).

I WOULD LIKE TO THANK Carolyn Sachs and the Sachs family for all their help in completing this project. I am particularly indebted to Rebecca Norris; Judith Crow; and Jeff, Jennifer, and Frances Sachs for providing me with Bob Sachs's family history. I have also had extensive help from many of Sachs's colleagues and associates, most particularly (in alphabetical order) Malcom Derric, Thomas Fields, William F. Fry, Roger Hilderbrand, Wendell G. Holladay, J. M. Nevitt, Gale Pewitt, Jonathan L. Rosner, Raymond F. Sawyer, Roland Winston, and Lincoln Wolfenstein.

NOTES

1. This paper is still the primary source on the subject for people working on weapons program at Livermore. Although it was available to the public, Sachs was unsuccessful in getting it released for publication in a scientific journal.
2. Power of prediction—an example. *Bull. At. Sci.* XX(1964):20-21.

3. A partial list includes the names of John Simpson, Harry Stevens, Robert Dietrich, and Captain Hyman Rickover, who later became Admiral Rickover. Sachs shared lodgings with Rickover in Oak Ridge and became his private tutor on the subject of nuclear energy.

4. The subject of this paper became quite important in astrophysics after the war. In a footnote of this paper Sachs calls attention to the possibility of thermonuclear detonations.

5. In electromagnetic radiative transitions in nuclear systems, theoretical justification for replacing the current density operator by charge density operator is referred to as Siegert's theorem (A. J. F. Siegert. *Phys. Rev.* 52[1927]:787).

6. These results found an important extension to semi-relativistic systems, such as pion-nucleon systems with finite source interactions by Richard Capps, a student of Sachs at the time (*Phys. Rev.* 99[1955]:926).

7. The appointment of William F. Fry in 1951 was the beginning of high-energy experimental physics program. It was followed by the appointments of William. D Walker in 1954 and Myron L. Good in 1960. By the middle 1950s the program blossomed into an active center of fundamental discoveries concerning the behavior of the new particles. Theoretical high-energy physics also acquired a boost with K. M. Watson, K. Simon, and later H. W. Lewis and R. F. Sawyer on the faculty.

8. The static electron-neutron interaction could be understood qualitatively in terms of the charge distribution in the pion cloud surrounding the neutron. However, using meson theory, L. Foldy had shown that the observed interaction could be accounted for by the Pauli form factor or the (anomalous) magnetic moment of the neutron instead of the expected Dirac form factor (*Phys. Rev.* 83[1951]:688).

9. However, the predictions based on this classification were found to conflict with experiments and consequently the classification scheme had to be abandoned.

10. It turned out that the preliminary results indicating the violation of $\Delta S = \Delta Q$ rule were not sustained with further improved statistics in measurements. Hence the theory of CP violation that was proposed in this paper turned out to be wrong. True CP violation in K-mesons was discovered in 1964.

11. The panel headed by Norman Ramsey, who was appointed by

the President's Science Advisory Committee to study and report the U.S. priorities in high-energy physics research. The other members of the panel were E. L. (Ned) Goldwasser, J. H. Williams, F. Seitz, P. H. Abelson, O. Chamberlain, M. Gell-Mann, T. D. Lee, W. Panofsky, and E. M. Purcell (J. M. Holl. *Argonne National Laboratory, 1946-96*, p. 216. Urbana: University of Illinois Press, 1997).

12. L. Greenbaum. *A Special Interest,* p. 160. Ann Arbor: University of Michigan Press, 1971.

13. Concluding remarks by R. G. Sachs in the proceedings of a one-day symposium on the 30th anniversary of the ZGS startup, ed. M. Derric, unpublished.

14. R. G. Sachs. P. 38 in *History of the ZGS (Argonne 1979),* ed. J. S. Day, A. D. Krisch, L. G. Ratner: New York: American Institute of Physics, 1980.

15. For instance, A. Yokosawa's successful design of a polarized target intended to study the spin dependence of the scattering of protons from polarized nuclei.

16. National Academy of Sciences. Report of the Elementary Particle Physics Panel to the Physics Survey Committee with J. D. Bjorken, J. W. Cronin, L. Hand, D. H. Miller and W. J. Willis, Physics Perspective II, A, 1-159. Washington, D.C.: National Academy of Sciences, 1972.

17. I came to Madison in 1955 to do my Ph.D., leaving behind my family, my pregnant wife, and two little girls. Jean Sachs's warmth and affection meant a great deal to me. Jennifer Sachs and Rebecca Norris (then Jenny and Rebbi) made up for the girls I had left behind.

18. The long list included, among others, Julian Schwinger, Abdus Salam, John Ward, L. Michel, C. N. Yang, and T.D. Lee. Jeffrey Goldstone did his fundamental work on spontaneous symmetry breaking during the first summer institute. Benjamin Lee and Jonathan Rosner were still students when they got an opportunity to participate in these institutes, which undoubtedly had great influence on their future careers.

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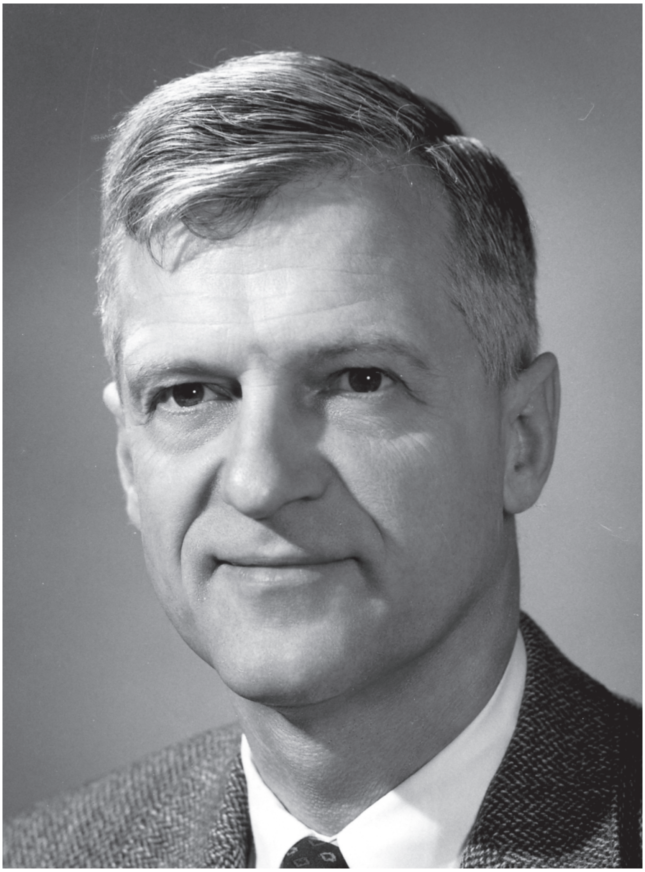

Ricco-Mazzuchi Photography, Berkeley, California

S. L. Washburn

SHERWOOD LARNED WASHBURN

November 26, 1911–April 16, 2000

BY F. CLARK HOWELL

For close on to four decades Sherwood Washburn sought to encourage, urge, insist, and cajole practitioners of biological anthropology—particularly the core of human evolutionary studies—to shift away from outdated methodologies, abandon outmoded or questionable precepts, adopt modern perspectives of an emergent evolutionary biology, and practice analytical, comparative, and experimental methods relevant to elucidation of the nature and roots of the human condition. His epiphany emerged progressively and sweepingly across prevalent biological and social science and wrenched but did not utterly revolutionize scientific praxis in biological anthropology as he sought and overtly intended it should. The expansion and elaboration of a number of attendant natural sciences relevant, indeed critical, to such study did play the requisite roles and thus exemplified the transformation essential to paradigm shift. Nonetheless Washburn played a unique and manifestly invaluable role in the refashioning—even restructuring—of human evolutionary studies as they emerged, diversified, and ultimately flourished in the later twentieth century.

Sherwood Larned Washburn (always known as "Sherry") was born on November 26, 1911, and grew up in Cambridge, Massachusetts. His father, a minister and one-time professor of church history, was dean of the Episcopal Theological School in Cambridge. Washburn always had an advantaged upbringing and youth, private school education in Cambridge and at Groton (1926-31), and ready admittance to Harvard, attended by his elder brother (Henry Bradford, a junior) and following on the Washburn male elders. He graduated summa cum laude (B.A., 1935), continued on to graduate studies, and subsequently received the doctorate in anthropology in 1940. His undergraduate honor's thesis was supervised by the mammalogist Glover M. Allen, who a few years later would produce the first exhaustive checklist of African mammals. Always short and wiry, Washburn at times lamented his lack of participation in sports (or performance of "feats of physical prowess"), although he performed superiorly in his weight class at college wrestling and had played soccer at Groton, suffering successive broken wrists as a consequence, which forever constrained his writing, driving, and dissecting proficiencies.

As boy and youth he pursued varied interests in natural history, both in mammalogy and in ornithology, including enhancing the Groton Museum with discarded stuffed mounts and keeping of captive raptors, both hawks and great horned owls. As a youngster he had familiarity with and entrée to Harvard's Museum of Comparative Zoology (MCZ), its exhibits and collections, where the director was a family friend and staff was encouraging, and where he happily worked over secondary school vacations. Upon entering Harvard's graduate school his projected major was zoology, possibly ultimately even medical school. An introductory general anthropology course, taught there by a close family friend and stimulating lecturer, Alfred Tozzer, served to reveal its cross-disciplinary roots and to capture permanently his interest in its breadth of scope and scientific relevance for the human condition. He never looked back thereafter.

Following completion of his initial graduate year, 1935-36, an opportunity was afforded him to participate as an assistant in a zoological collecting expedition to southern Asia. This Asiatic Primate Expedition was formulated, promoted, and largely funded by Harold J. Coolidge, zoologist and conservationist, and included primate morphologist Adolph Hans Schultz (1891-1976) of Johns Hopkins University, comparative psychologist Clarence Ray Carpenter (1905-75) then at Columbia University, and MCZ technicians. The experience was to have a profound effect on Washburn's perspective on and approach to evolutionary problems not unlike experiences of naturalists a century earlier. A university (Sheldon) traveling fellowship enabled him a year's freedom to indulge himself in the necessary endeavors of human and then comparative anatomy, having just finished a year's immersion in the later and in vertebrate paleontology with A. S. Romer at Harvard. An intensive summer course at the University of Michigan afforded in-depth experience of human gross anatomy, much facilitated by an obliging laboratory assistant, W. T. Dempster, who then and subsequently was interested in human locomotion and was a proponent of joint functional mechanics. This experience was followed by an autumn term at the University of Oxford in the United Kingdom and an opportunity to gain from the masterly lectures of Wilfred Le Gros Clark (1895-1971), a major contributor to systemic ("pattern") approaches to primate and human morphology, particularly the central nervous system and tissue systems of the human body.

A stint of collecting in Ceylon (now Sri Lanka), then over four months in northern Siam (now Thailand) focused on the collection of lar gibbons and in observations of their naturalistic behavior, was followed by several months of further collection in Sabah in eastern Malaysia devoted to collection of diverse colobine monkey species, several macaque species, and even a few orangutans. Here was a splendid opportunity to acquire from freshly obtained material first-hand experience of structure and morphology within and between lesser and greater apes and both arboreal and more terrestrial cercopithecoid monkeys. This experience afforded insights fundamental to Washburn's perceptions of primate evolution as it had in years past in some similar circumstances equally to Arthur Keith and to Wilfred Le Gros Clark. Washburn was to continue preparation work on these extensive collections upon return to Harvard, often assisted by Gabriel Lasker, where he also had an opportunity to teach a course on primates upon an offer to do so by his major advisor, E. A. Hooton (1887-1954).

Washburn accepted a position in anatomy at Columbia University's College of Physicians and Surgeons in 1939 and remained there for eight years. He had married Henrietta Pease, daughter of an academic in the classics and college president, in 1938. His doctorate at Harvard was awarded the following year, the first there in anthropology to be focused on nonhuman primates, in this instance metrical appraisal of adult skeletal proportions among Asian macaques and langurs. At Columbia, Samuel R. Detwiler (1890-1957) headed the department and was himself a major practitioner of developmental biology. This environment encouraged the young instructor to practice and ultimately to urge others to implement experimental procedures to garner further insights and comprehension of body structure and links to adaptation, and thus to attack the nature of evolutionary transformations among primates, human and nonhuman.

A series of papers between 1941 and 1948 dealt with matters of both research procedure and with efforts to elucidate morphology through experimental approaches. Some of these and associated efforts directed to the elucidation of development and growth of bone were to be pursued by his own students subsequently. Other figures with whom he became well acquainted at this time in New York and who played influential roles in his intellectual development included Theodosius Dobzhansky (1900-75) of Columbia University; William King Gregory (1876-1970) and George Gaylord Simpson (1902-84), both at the American Museum of Natural History; and particularly Paul Fejos of the nascent Viking Fund, founded in 1941. Fejos (1897-1963), of Hungarian background, a doctor of medicine, ex-cavalry officer of the Great War, and former cinema and ethnographic film director, became both a fast friend and major supporter of matters anthropological, when the field was still limited in practitioners and in scope. In 1946 under the auspices of Columbia University's summer school and with the underwriting of the Viking Fund (to become the Wenner-Gren Foundation for Anthropological Research in 1951) and the strong support of its director of research, the first of a succession of summer seminars in physical anthropology were held in New York City. Washburn was then secretary of the American Association of Physical Anthropologists; with former Harvard classmate and close friend Gabriel W. Lasker (1912-2002) (anatomy, Wayne State University) as supporter, twice-weekly discussion gatherings and demonstrations were held over a six-week period. At Washburn's encouragement Lasker consequently inaugurated a series of attendant annuals, titled *Yearbook of Physical Anthropology*, offering reportage on such workshop gatherings and re-publication in lithographic form of significant and relevant recently published contributions to the field. The summer workshops continued through 1951. (The yearbook was to continue for 22 volumes, until 1979, whereupon it became an important and welcome annual supplement to the association's official serial *American Journal of Physical Anthropology*.)

This quotation, excerpted from the overview of their initial summer seminar, drafted by Lasker's subsequent wife, Bernice Kaplan, encapsulates the mood and underlying intent behind the organization of this seminal gathering.

For the most part the emphasis was on the advisability of redefinition of problems and reorientation of the methodology with which to approach the several facies of the field in general. It was believed that the older methods of approach to the overall problems, while yielding valuable information, had now reached a point where redundancy rather than additional insight was resulting from their use. The need for an intensive investigation of a specific problem chosen for study was frequently brought out as probably more fruitful than the approach through surveys of the physical characteristics of the people of this or that geographic region. Although historically all sciences, physical anthropology included, had begun with description and then proceeded to analysis, the opinion was advanced and supported that it would now be better first to analyze problems, to define what was being looked for and why, and then to organize the technical procedures to the closer understanding of what is to be analyzed—moving from this to those larger, more inclusive and more meaningful descriptions which can best be done when the analytic work has been completed.

In 1947 Washburn joined the anthropology faculty at the University of Chicago, where he was to remain over the ensuing 11 years. He succeeded Wilton Marion Krogman (1903-87), who held a doctorate in anthropology from Chicago with a thesis devoted to an analysis of craniofacial growth in anthropoid apes, the first such done within that department. Krogman held a joint appointment in anthropology and anatomy at Chicago, as he had previously at Case Western Reserve. In spite of a thwarted promise of a joint appointment in anatomy (cum laboratory facilities) Washburn continued and accelerated his efforts at rejuvenation and redirection in human evolutionary studies through the practice of biological anthropology. (Subsequently when Washburn served as editor of the association's professional journal [1955-57], he sought among the association membership repeatedly, and failingly, to change its title to Human Evolution; ultimately two other serials were to bear that name.) Facilities of a laboratory nature were scarcely even minimal at Chicago; nonetheless laboratory-oriented work was pursued and doctoral dissertations on cranial growth and development, aspects of cranial bone structure and adaptation, on chimpanzee growth, on brachiation and its adaptive correlates, among others, were forthcoming. Several graduate students were holdovers from Washburn's predecessor, and others—including myself—were newly demobbed veterans at war's end.

The first of three journeys to sub-Saharan Africa by Washburn occurred over some months in the spring of 1948. All these field trips were supported by the Wenner-Gren Foundation and encouraged by its director, Dr. Paul Fejos. The first included visits to South Africa and an opportunity to see and examine australopithecine fossils in Pretoria and in Johannesburg and to meet with those who were charged with their retrieval, preparation, and study. A following stay in Uganda, hosted at Makerere College in Kampala, enabled the observation and particularly the collection of a large series of cercopithecine monkeys of several taxa on which Washburn was able to make substantial quantitative assessments of muscle weights, proportions, and morphology in the course of their preparation. This activity followed directly his similar activity in Southeast Asia a decade earlier. This and later journeys led to reciprocal visits to the United States, particularly Chicago, by Africa-based scientists and others from elsewhere for as much as an academic term. Among them were Robert Broom (1866-1951), Raymond Dart (1893-1988), Alexander Galloway (1901-65), John Robinson (1923-2001), Ronald Singer, Phillip Tobias, and Kenneth Oakley (1911-81), all of whom were assisted in their travel by the foresaid foundation and often were associated with our departmental seminars and made courses and other visits at institutions elsewhere.

In June 1950 a major international symposium of the Cold Spring Harbor Biological Laboratory was devoted to the "origin and evolution of man." The program was developed by Washburn and Theodosius Dobzhansky, and its 37 principal participants (among 129 people who registered) were drawn from cultural and biological anthropology, primatology, genetics, evolutionary and paleobiology, and medicine. Three of the major contributors to the "Evolutionary Synthesis" were present as were a number of outstanding scientists from abroad. This assembled mix of scientists, the focus and scope of the symposium, and near immediate publication of the resultant volume in the laboratory series unquestionably had a major effect on its consequent impact on an emergent biological anthropology, perhaps even more so than the summer seminars (which were largely restricted to practitioners within that field). Similarly in 1952 the Wenner-Gren Foundation's conference "Anthropology Today," held in New York City, was both critical examination and stocktaking, coupled with an envisioning of developments to come and worthy directions to pursue. Washburn played a significant role there in its planning, along with colleagues Sol Tax and Alfred Kroeber (University of California, Berkeley). In a contribution there ("The Strategy of Physical Anthropology") and another a year previously at the New York Academy of Sciences ("The New Physical Anthropology") Washburn sought to set the course for the ultimate emergence of a full-fledged program of human evolutionary studies. Such was not to emerge fully for another several decades, but its components were to grow and even to diversify in the interim.

Washburn served as president of American Association of Physical Anthropologists (1951-52) and as department chair at Chicago (1952-55) in those years. The venue of the Third Pan-African Congress on Prehistory in Livingstone, Northern Rhodesia (now Zambia), in 1955 occasioned for him a second and summer visit to central Africa. He sought there to assemble a collection of baboon skeletons, which was duly accomplished, as the animals were then and there considered vermin in agricultural settings. Moreover, it also led to substantial time spent by him in naturalistic observations near Victoria Falls in the Hwange (formerly Wankie) Game Reserve (Zimbabwe) on baboon troops already somewhat conditioned to human presence due to the touristic circumstance. Washburn gained an appreciation of diet, feeding behavior, sleeping practices, predator avoidance, social structure, dominance, and local environmental adaptation such that the hook was already well set for his near future shift toward concerns with the social behavior of primates in natural habitats, as an especially important comparative, if analogical, window into events in human evolution. He was able to pursue this perspective further in the course of two multidisciplinary symposium gatherings in 1955 and 1956, which eventuated in the volume *Behavior and Evolution* (1958) in which many aspects of evolutionary biology were explored with reference to adaptation and behavior.

This perspective was elaborated in the later part of 1959 during a third visit to Africa, in this instance Kenya, where a graduate student, Irven DeVore, had initiated a field study of olive baboon (*Papio anubis*) troops in Nairobi National Park. There and later at Amboseli National Park, near the Tanzanian border, Washburn was enabled to observe primates and associations with other large mammals in natural settings and to develop further perspectives on adaptations of mammal communities in African savanna environments. His wife, Henrietta, and son Stanley accompanied him on this trip; during this period her first symptoms of Parkinsonism were evidenced, to which she succumbed many years later. Washburn had spent an earlier year, 1956-57, at the Center for Advanced Study in the Behavioral Sciences in Palo Alto, California, at which time his African field experiences of animal behavior were refined and expanded both in cross-disciplinary seminar contexts and in due course with his developing collegial relationship with David Hamburg, a psychiatrist with wide-ranging interests in stress, coping behavior, human adaptation, and ultimately evolutionary biology. Hamburg, after medical school and military service, had served at the National Institute of Mental Health and then joined the faculty of Stanford's School of Medicine; he subsequently presided over the Institute of Medicine from 1975 to 1980, founded a new division focused on health policy and education between the Kennedy School of Government and the Harvard Medical School; he ultimately became president of the Carnegie Corporation and served as well as president of the American Association for the Advancement of Science in 1984-85.

Washburn joined the faculty of anthropology at the University of California, Berkeley, in 1958 and served just over two decades until retirement in 1979. Thus, his overall career in anthropological academia was to span 32 years. At Chicago Washburn had elaborated his perspectives on a concern with human evolutionary studies within the emerging concepts of a modern evolutionary synthesis. A major yearlong survey course there involved joint teaching across the range of biological, prehistoric, and archeological aspects of the human evolutionary career. Undergraduate enrollments were minor and the audience therefore minimal within the prevalent Robert Maynard Hutchins college structure. At Berkeley a major attraction in a larger (and growing) departmental framework was the opportunity to confront a larger, and undergraduate, body of over a thousand in the course structure of a year, in a course of one's own design, offer a few chosen graduate seminars or labs as wont, and to seek to expand and refine the scope of primate and human evolutionary studies within a very much larger university context. A live-animal facility suitable for the maintenance and observation of primates was also developed jointly with a member of the department of psychology. It was to expand ultimately.

Washburn's success in undergraduate education proved unparalleled, as reflected in greatly enlarged enrollment, forthcoming financial support of teaching assistance, and consistent appreciation of his charismatic lectures. A pool of new doctorates emerged from an expanded cadre of graduate students. In 1958-61 he quickly established a program on the evolution of human behavior with extramural financial assistance of the Ford Foundation; subsequently there was a longtime program on primate behavior underwritten by the National Institutes of Health. A fellowship in the campus Miller Institute for Basic Research in Science afforded released time for this undertaking. The anthropological faculty was progressively diversified and strengthened with appointments to represent various aspects of biological anthropology, prehistory, and paleoanthropology, very much at his counsel and urging. Washburn served briefly (1961-63) and grudgingly as department chair, resigning after disputations on proposed faculty appointments.

Washburn participated from the beginning in the Wenner-Gren Foundation's overseas activities at their splendid castle Burg Wartenstein, situated in southern Austria. He first chaired a conference on "The Social Life of Early Man" in 1959 (published 1961), the conception of which originated largely with others, and whose format and composition he only partially controlled. Nonetheless its 19 participants constituted a significant cohort of international scientists and a good mix of scientific disciplines. He succeeded vastly better at another significant gathering, in 1962, for another conference, "Classification and Human Evolution" (published 1963), that proved to constitute a landmark in the development of modern primate evolutionary studies within the time of the emergence of molecular biology.

At this time, in 1960, Washburn received the Viking Fund Medal of the Wenner-Gren Foundation (among the last awarded), was elected a fellow of the American Academy of Arts and Sciences in 1961 and a member of the National Academy of Sciences in 1963. He served as president of the American Anthropological Association, and at its 1962 annual meeting his insightful and definitive presidential address on the meaning and meaninglessness of the concept of race and of racial categorization among humans elicited both praise and shock. Early in his career, during the war years, he had spoken out in an educational journal on the use and misuse of race concepts. This was not, however, either his interest or area of expertise, although his grasp of the problem was surely substantial enough. Moreover, he had long ago overcome the still frequent affliction of typological thinking and use of stereotypes. In this instance he returned to the subject, at the insistence of the association's board and on the occasion of a major trade publication, *The Origin of Races* (Knopf, 1962) by Carleton S. Coon (1904-81), which presented an ill-conceived and poorly founded perspective on human evolutionary processes, events, and trajectories. His address was thoughtful, direct, and courageous at a time of national unease, controversy, and a resurgence of active segregationists; it constituted another landmark event at a moment too critical to be missed. Washburn noted and demonstrated that "racism is based on a profound misunderstanding of culture, of learning, and of the biology of human species" and "is equally a relic supported by no phase of modern science." Mark Twain long before had nailed it "a fiction of law and custom." On another occasion Washburn received that association's Distinguished Service Award.

Washburn and David Hamburg encouraged formation of a major study group devoted to an extensive survey of current primate field studies convened at the Center for Advanced Study in the Behavioral Sciences in 1962-63. This nine-month effort, in which 20 investigators were to participate, resulted in the first major volume, *Primate Behavior: Field Studies of Monkeys and Apes* (I. DeVore, ed., 1965), to represent this field in an evolutionary, inclusive, and widely accessible perspective. Its impact on anthropology, ethology, and primatology was timely and momentous. Similarly Washburn played an important role in a wide-ranging study dedicated to *The Teaching of Anthropology* (published 1963), a major effort directed to the current status and future direction of the burgeoning field and its growing, and fissioning subdisciplines. In a conference in 1969 on coping and adaptation (published, 1974), Washburn coauthored with David Hamburg, as was the case on several other occasions as well, an important paper on social adaptation among nonhuman primates. Here as elsewhere the effort was directed to place aspects of the human condition and its disaffections in a comparative and phylogenetic perspective. In 1965 Washburn received in London the Ciba Foundation Annual Lectureship Medal on the occasion of a conference there dedicated to aggression. Two years later he received the Huxley Medal of the Royal Anthropological

Institute (London) and in 1968 he was an invited lecturer on "The Study of Human Evolution" for the Condon Lectures at the University of Oregon.

Washburn played a central role in planning and urging development of national primate research centers throughout the country. His national and international impact on the growth of primatology was formally recognized at the fourth International Primatological Congress in Portland in 1972. This expansive meeting was dedicated to Washburn, as was a hefty issue of the *American Journal of Physical Anthropology* (vol. 38, no. 2, 1973). In 1975 the University of California honored him with the title of University Professor, a prized and indeed rare recognition. He retired as emeritus University Professor in 1979 and was awarded the Berkeley Citation for meritorious service. In subsequent years he was to receive a Walker Prize of the Boston Museum of Science, an honorary D.Sc. from Witwatersrand University, Johannesburg, and the Charles Robert Darwin Award of the American Association of Physical Anthropologists.

This retrospective concerning Sherwood Washburn reflects in great measure my own experience and association with him over a period of some 50 years. We met momentarily when he first visited the University of Chicago in the spring of 1947 before his acceptance of a faculty position in anthropology later that same year. I was a newcomer to college after naval service in World War II and was infused with a renewed hope to study biological anthropology as a profession with a focus on the study of human evolution. Washburn was to become my principal advisor, although I worked closely with others in prehistoric archeology (R. J. Braidwood, 1908-2003), paleontology (E. C. Olson, 1910-93), and various natural sciences. My initial instructorship in anatomy, at Washington University, after the doctorate in 1953, was cut short by a request to return to the anthropology faculty at Chicago. We served together there for three years. In 1970 we again came to be associated on the anthropology faculty at the University of California, Berkeley, when I accepted a proffered position, recently open consequent to the death of Theodore D. McCown (1908-69).

Washburn made a lasting and singularly important impact on biological anthropology throughout his career. This is of course reflected to an extent in the recognition and honors afforded him, but most particularly it stands by the changes and transformations effected as a consequence of his presence and the positions he espoused. He was neither shy nor deferential; the postures he assumed and the stances and beliefs he opposed were always serious, even pressing matters and thus warranted outright proselytism on his part. He was surprisingly skeptical and iconoclastic, given his background. In my experience he was open to suggestions, welcomed information pertinent to his position(s) and interest(s), but scarcely gave quarter against open opposition to views he adamantly upheld. Washburn was a mine of ideas, often showered in profusion like sparks from an anvil, often times surprisingly innovative and fruitful, always provocative but sometimes off the mark. On an occasion early in our acquaintance I recall he carried a copy of Richard M. Weaver's newly published *Ideas Have Consequences* (Chicago, 1948), and which Washburn then extolled at great length, adding that "our own science" sorely needed "house cleaning" and such new "envisionings." After that moment of revelation I had a real grasp of his own perspectives and intentions; it enabled a relationship of near parity despite our 14-year age difference. As our birthdays were only a day apart, for some years we shared in their celebration.

Washburn's initial focus was directed at overall body structure and transformation of its components within the course of primate evolution, and particularly that of higher anthropoids and mankind. Of course he recognized the necessity to approach and delineate scientific problems feasible of solution given available knowledge and technological capacities. Hence, it was incumbent to define evolutionary complexes (rather than mere isolated traits); to compare and contrast variations in such complexes and their adaptive correlatives; to elucidate underlying biology, including functional mechanics, and genetics of such complexes; to recognize conditions propitious for selection of such adaptations; and thus to improve capacity and quality of phylogenetic inferences (reconstructions) through better theory, better methods, and scientific responsibility. I do not recall him to be unduly preoccupied with all the niceties of scientific method; some history of science may have afforded grist for his mill, whereas the (formal) philosophy of science largely did not. Gabriel Lasker, in his autobiography, *Happenings and Hearsay* (1999), considered that transformations attendant on redirections toward "new physical anthropology" resulted in a "true paradigm shift within the discipline." Washburn was ultimately to direct his (and others') attention to many other concerns, including naturalistic behavior of primates, cultural learning through education, citizenship, values, and society, all espoused in an evolutionary framework, and as well early on the value and implications of work in molecular genetics. He deprecated the emergence and called out the evils and limitations of sociobiology.

Washburn's approach was both analytical and reductionist. He dismissed trivia, minutiae, (much) given truth or received wisdom. In all these respects I gained vastly from knowing him. Over his career his interests broadened increasingly, exploring and ultimately engulfing other areas of relevance as he sought to comprehend and to explicate the origin and nature of the human condition within a naturalistic framework. As a consequence his encouragement of many aspects of the practice of what was indeed to become a new physical anthropology was meaningful and repeatedly fruitful. This is to say his success rate was high, in spite of the fact that he hardly ever had a personal research record in depth in any particular topic, problem set, or technology. He neither established laboratory facilities nor pursued a rigorously defined research agenda after leaving Columbia. At those times institutional structure was definitely a limiting factor toward such resources. His focus was programmatic across a general concern, fortunately having at best fuzzy boundaries; in fact his mode of thought (or play) was to ignore, to transgress such traditional limits and to usurp or to engulf the useful and the relevant, regardless of disciplinary and historical priority. He was openly and frankly iconoclastic in such respects and thus pan-disciplinary in vision. Such are the features that Washburn brought so effectively and individually across social and natural sciences, and as have a number of the many students he mentored. He is remembered for all his efforts directed toward the growth and realization of a field of scientific study still nascent upon his appearance on the scene.

For this memoir I have relied largely on my own acquaintance with Washburn for some 50 years. I have also profited from obituaries by R. H. Tuttle (*American Anthropologist* 102[4]:865-69, 2002), J. Marks (*Evolutionary Anthropology* 9[6]:225-26, 2000), and A. L. Zihlman (*American Journal of Physical Anthropology* 116:181-83, 2001). Washburn's full bibliography appears in the volume *The New Physical Anthropology: Science, Humanism and Critical Reflection* (S. C. Strum, D. G. Lindburg, and D. Hamburg, eds., pp. 277-85. Upper Saddle River, N.J.: Prentice-Hall, 1999) and it contains appreciations of him and his work by some former students, as well as reprintings of some of his own

most influential papers. His activities and intellectual influences are also explored at length by Donna Haraway in a chapter titled "Remodeling the Human Way of Life: Sherwood Washburn and the New Physical Anthropology, 1950-1980" in *Bones, Bodies, Behavior: Essays on Biological Anthropology* (George Stocking, ed. Madison: University of Wisconsin Press, 1988). Two further resources were "S. L. Washburn. Evolution of a Teacher" in *Annual Review of Anthropology* (12:1-24, 1983), and "An Interview with Sherwood Washburn" by Irven DeVore (*Current Anthropology* 33(4):411-23, 1992). In his own autobiography (*Happenings and Hearsay: Experiences of a Biological Anthropologist.* Detroit, Mich.: Savoyard Books, 1999), Gabriel Ward Lasker, a classmate and candid admirer of Washburn, has useful thoughts about his colleague and the state of the discipline in those earlier days.

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Victor F. Weisskopf

VICTOR FREDERICK WEISSKOPF

September 19, 1908–April 22, 2002

BY J. DAVID JACKSON AND KURT GOTTFRIED

VICTOR FREDERICK WEISSKOPF was a major figure in the golden age of quantum mechanics, who made seminal contributions to the quantum theory of radiative transitions, the self-energy of the electron, the electrodynamic properties of the vacuum, and to the theory of nuclear reactions. In the broader arena through his writings and actions he was an effective advocate for international cooperation in science and human affairs. In 1981 he shared the Wolf Prize for physics with Freeman Dyson and Gerhard 't Hooft for "development and application of the quantum theory of fields." In 1991 he was awarded the Public Welfare Medal of the National Academy of Sciences "for a half-century of unflagging effort to humanize the goals of science, acquaint the world with the beneficial potential of nuclear technologies, and to safeguard it from the devastation of nuclear war." As a member of the Pontifical Academy of Sciences he was instrumental in persuading the Pope to speak on the dangers of nuclear weapons.

Weisskopf was born in Vienna, Austria, on September 19, 1908. In his nineties and increasingly frail, he died at home in Newton, Massachusetts, on April 22, 2002. Growing up in Vienna in a well-to-do Jewish family, he had a happy and carefree childhood despite the Great War. In his teens he attended a gymnasium and for two years the University of Vienna. He showed an early interest and ability in science. In 1928, upon the recommendation of Hans Thirring, professor of theoretical physics in Vienna, he moved at age 20 to Göttingen to continue his studies under Max Born. His first important paper, written with Eugene Wigner, was on the quantum theory of the breadth of spectral lines. After completing his Ph.D. thesis in the spring of 1931 he went to Leipzig to work under Werner Heisenberg and then in the spring term of 1932 under Erwin Schrödinger in Berlin. For the academic year 1932-33 he received a Rockefeller Fellowship to work in Copenhagen with Niels Bohr and in Cambridge with Paul Dirac.

In the fall of 1933 Weisskopf came to Zürich for two and a half years as Wolfgang Pauli's assistant. While there he published two important papers. The first was on the self-energy of the electron in the framework of Dirac's hole theory, in which he showed that the self-energy diverged only logarithmically with decreasing size of the electron's charge distribution, in contrast to the linear divergence of classical theory and the quadratic divergence of the one-particle Dirac theory. The second paper, coauthored with Pauli, concerned the quantum field theory of charged *scalar* particles (not the spin 1/2 particles of Dirac). They showed that antiparticles were not unique to the Dirac theory but occurred in general and that electrodynamic processes involving the scalar particles were closely similar to those involving spin 1/2 electrons.

In April 1936 Weisskopf accepted a fellowship at Bohr's institute in Copenhagen. While there he completed an impressive analysis of the properties of the vacuum in the presence of electromagnetic fields, clarifying earlier work, giving physical arguments for the removal of certain infinities, and presciently enunciating the concept of charge renormalization. With the increasing persecution of Jews in Nazi Germany and the prospect of war, he and his wife decided to look for ways to escape Western Europe. To enhance his chances Weisskopf began to work in the increasingly important field of nuclear physics, which occupied many at Bohr's institute, and to publish in English. Although he had job offers from the Soviet Union, after a visit in late 1936 he and his wife decided that he would consider them only as a last resort. With Bohr's help he was offered a lectureship at the University of Rochester beginning in the fall of 1937.

He was on the Rochester faculty for five and one-half years. During that time he continued research in nuclear physics but also on the electrodynamics of the electron. He returned to the self-energy problem and in 1939 established a result little appreciated at the time or now: that in the *n*th order of perturbation theory the self-energy diverges only as the *n*th power of a logarithm.

In early 1943 Weisskopf was invited to Los Alamos, where he soon became Hans Bethe's deputy in the theoretical physics group. Already famous for his physical intuition, he was much sought after by the experimenters to provide estimates of little known or little understood nuclear processes. He served for a time as mayor of Los Alamos, evidence of his humanity and social responsibility.

In early 1946 he joined the physics faculty at the Massachusetts Institute of Technology, where with one substantial break he remained until retirement in 1974. His researches at MIT focused on nuclear reactions, with one major paper with J. Bruce French on a complete calculation of the leading radiative correction to atomic energy levels (the Lamb shift). The nuclear physics work was done mainly in collaboration with Herman Feshbach, sometimes augmented by students and postdocs. These papers are marked by their clarity, the simplicity of the assumptions, and their close connection to experiment. Together with John Blatt he authored an influential text on theoretical nuclear physics, published in 1952. That same year he was elected a member of the National Academy of Sciences.

In 1961 at the apex of his career as an academic researcher Weisskopf was invited to be director general of CERN, the European center for high-energy physics near Geneva, Switzerland. For CERN it was an inspired choice. Weisskopf provided intellectual leadership and a vision of the laboratory as an international research center second to none. He successfully promoted construction of the first proton-proton collider, the intersecting storage rings, and saw to the eventual building of a 300-GeV accelerator. He set CERN on its path to be a preeminent, some would say the preeminent, research center in high-energy physics today.

At the end of his five-year term he returned to MIT, where he was named Institute Professor and chaired the Physics Department for six years (1967-73). During these years he pursued occasional research, but devoted increasing fractions of his time to writing, invited lectures, and public service. He published popular expositions of science, collections of essays on science, science in public affairs and scientific personalities, and his autobiography. After retirement he continued writing and giving informal talks to explain the wonders of science to the lay public.

From near the end of the Second World War Weisskopf was active in discussion of the promise of nuclear energy and the dangers of nuclear weapons. He was among the founders of the Federation of Atomic Scientists, a member of the Emergency Committee of Atomic Scientists chaired by Albert Einstein, and a participant in the early Pugwash meetings.

Weisskopf served as president of the American Physical Society in 1960. He was elected to numerous learned societies in addition to the National Academy of Sciences and received many honorary degrees. He received several prizes and medals, including the Max Planck Medal in 1956, the National Medal of Science in 1980, and the already noted Wolf Prize in 1981 and the Public Welfare Medal in 1991.

EARLY YEARS

Viki (the nickname by which he was universally known) Weisskopf was the second of three children born into a comfortably middle-class Jewish family. His father, Emil, originally from Czechoslovakia, was a successful lawyer; his mother, Martha, was from an upper-middle-class nonobservant Jewish Viennese family. The family saw to it that young Victor and his siblings were exposed to the rich cultural offerings of Vienna—concerts, the opera, theater—with summers at Altaussee. He studied the piano and developed a lifelong love of music. In his late teens he even considered seriously becoming a professional musician. He attended a progressive elementary school and then a gymnasium. There his interest in science, especially astronomy and physics, flourished.

While at their summer home in Altaussee southeast of Salzburg in August 1923, Viki and friend George Winter spent several hours on top of an 1,800-m peak, where they observed a total of 98 shooting stars of the annual Perseid shower, which they classified as to color and appearance. The results of their investigation were published in *Astronomische Nachrichten* (1924). It is not many of us who can claim a first research publication at age 15 and a half!

At the gymnasium participation in youth groups was the norm; Viki joined the young socialists and in 1926 took part in performances of political satire in Vienna cabarets. For two years he attended the University of Vienna, where he found inspiration in Hans Thirring's lectures in classical theoretical physics. Thirring, sensing Weisskopf's exceptional abilities and knowing that Vienna was not in the forefront in modern physics, recommended that he transfer to Göttingen for further studies. Göttingen at that time was the Mecca of theoretical physics, where Heisenberg, Born, and Jordan had invented quantum mechanics in 1925-26.

GÖTTINGEN

At Göttingen Viki became a doctoral student of Max Born, the professor of theoretical physics. In his autobiography[1] Born remembers the young man.

> Another member of my group of research students was Victor Weisskopf, who came from Vienna. He was at first very timid, and several times came near to giving up theoretical physics when he made a blunder in his reasoning. But I encouraged him and succeeded in keeping him on his path.

Viki's insecurity over his potential for mistakes came to the surface from time to time throughout his career, as we note below.

Born's duties and poor health left Viki largely on his own. After learning quantum mechanics from Gerhard Herzberg, Viki embarked on research on the interaction of radiation with matter, a broad subject of central importance and one to which Weisskopf would make major contributions. He attacked his first unsolved problem: the natural width of spectral lines in emission of radiation by atoms. He was able to make progress on a two-level quantum system but not beyond. He sought help from Eugene Wigner, who was then in Berlin but who came back to Göttingen for regular visits. Wigner became Viki's mentor and the collaboration led to two papers. The first, most important paper (1930) treats the exponential decay of excited atomic states and the natural breadth of the associated spectral lines for all types of transitions. In contrast to the semiclassical result where an intense line was necessarily broad and a weak line narrow, the quantum theory accommodates the occasional puzzling broad but weak line.

His doctoral thesis (1931) described the application of the theory to resonance fluorescence, the absorption and re-emission of light by atoms. The thesis has had considerable impact in atomic spectroscopy over the years.[2]

POSTDOCTORAL YEARS

In the years 1931-37 Viki had the most remarkable of postdoc careers, first with Heisenberg in Leipzig, then Schrödinger in Berlin, Bohr in Copenhagen, Pauli in Zürich, and then Bohr again. This history speaks not only to how Viki's talents and promise were judged by the leaders of theoretical physics but also to the scarcity of long-term positions during the Great Depression. The job shortage was greatly compounded for Viki by the exclusion of Jews from numerous academic posts in Germany after the Nazis came to power in 1933. Rockefeller fellowships and Bohr's hospitality in Copenhagen played central roles for temporary opportunity and sustenance for many.

As a new Ph.D. Viki went first to Leipzig, funded by his family, to work with Heisenberg. At Christmas 1931 Schrödinger invited him to Berlin for the spring term to be his *Assistent* in Fritz London's temporary absence. As he told one of us (K.G.) late in life, Schrödinger would sometimes telephone shortly before he was to lecture and ask Viki to substitute, which Viki so many decades later acknowledged with boyish embarrassment as having been occasioned by the professor's assignations. Schrödinger showed another side by arranging a one-year Rockefeller Fellowship for Viki to begin in the fall of 1932. For the long summer period Viki and his current girl friend went to Kharkov in the Soviet Union, where Lev Landau was. This was the first of several trips to the Soviet Union, trips that both opened his eyes to the evils of the regime and made friendships with Soviet scientists that were useful in different ways later in his life.

During his postdoc period Viki developed close friendships with many young colleagues who were rapidly becoming prominent physicists, especially Patrick Blackett, Felix Bloch, Hendrik Casimir, Rudolf Peierls, and George Placzek, as well as Max Delbrück, who left quantum field theory to become a great pioneer in molecular biology. And in 1932 on his second day in Copenhagen Viki met Ellen Tvede, who was soon to be his wife and constant companion until her death in 1989. By this time Viki seemed to have lost the shyness remarked on by Born. In his reminiscences[3] Hendrik Casimir notes that in Copenhagen Viki participated in the entertainments at Bohr Institute conferences by "provid[ing] both poetry and song" and by taking the role of the Dalai Lama.

Three central problems of quantum electrodynamics (QED) were the focus of Viki's research during his postdoctoral period: the role of antiparticles, the self-energy of the electron, and the properties of the vacuum in QED. The puzzling negative energy solutions of Dirac's amazingly successful relativistic wave equation were in 1932 proposed by Dirac to correspond to antiparticles of the same mass as the electron but of opposite charge, an interpretation that was highly controversial. Nevertheless, such particles were discovered in cosmic-ray experiments later that year, but that these objects were indeed Dirac's antiparticles was not clear for some time. Meanwhile the description of the self-energy of the electron using Dirac's electrons alone (positive energy solutions only) led to a badly divergent result.

Understanding of both these problems was greatly advanced in two papers written during Viki's two and a half years as Pauli's *Assistent* in Zürich (fall 1933-spring 1936). At Pauli's suggestion Viki computed the electron's self-energy in perturbation theory, including both electrons and positrons. In doing this calculation Viki made a sign error, which was quickly pointed out by Wendell Furry; when this was taken into account, the result was a self-energy that diverged logarithmically as the electron's radius *a* tended to zero (1934).[4] This was an astonishing result: Classical electrodynamics was long known to produce a linear divergence, and the one-particle version of QED, already mentioned, yielded a quadratically divergent self-energy. The "soft" logarithmic divergence of QED with electrons and positrons was a first indication that QED might be made a tractable theory.

Viki was very discouraged by his error in the self-energy calculation, which exacerbated his lack of mathematical self-confidence. "[I] told Pauli that I wanted to give up physics, that I would never survive this blemish on my professional record." (1991, p. 80). Pauli, like Born before, urged him not to take it too hard, that it would not end his career. And so it proved.

The second Zürich paper, written with Pauli, dealt with the quantization of the charged scalar field (1934).[4] At the time, this work was viewed as a purely theoretical exercise for no "elementary'' spin zero particle was known. It was, however, an exercise that taught an important lesson, because it demonstrated that antiparticles are not a peculiarity of Dirac's theory for spin 1/2 fermions but are also an inevitable feature of a quantum field theory for charged bosons. Furthermore, the Pauli-Weisskopf paper demonstrated that a marriage of relativity and quantum mechanics does not require spin 1/2 as many had incorrectly inferred from Dirac's theory, and that scalar QED gave results for physical processes similar to spin 1/2 QED. And with the advent of Hideki Yukawa's meson theory of nuclear forces, the scalar field theory became more than a setting-up exercise for theoreticians.

COPENHAGEN AGAIN

Viki had been very productive during his first year in Zurich, perhaps in part because he was alone. Ellen and he had agreed to test their love with a one-year separation. The test proved successful, and they were married in Copenhagen on September 4, 1934. During the remainder of his time in Zurich he continued to work on topics in electrodynamics, beginning with an investigation of the properties of the vacuum in QED. This line of study reached full fruition in Copenhagen in 1936, where he held a fellowship with Bohr (April 1936-September 1937). At the Bohr Institute nuclear physics was beginning to be emphasized in addition to basic problems in quantum mechanics and field theory. Of the stimulating atmosphere at the institute Viki says, "Influenced by this remarkable group, I wrote two of my best papers during that time." (1991, p. 95).

The first of these is a classic paper on the polarization of the vacuum caused by the virtual electron-positron pairs under the influence of a uniform electromagnetic field of arbitrary strength (1936).[4] Although this topic had already been studied, the earlier work was very formal and marked by ambiguities. Viki largely cleared these up in this investigation, the technically most sophisticated of his career. Especially noteworthy was his prescient recognition of charge renormalization, in which he exploited the analogy with a charge placed in a polarizable medium to conclude that vacuum polarization produces an unobservable (though infinite) constant factor multiplying all charges and electromagnetic field operators.

It had become clear to Viki by 1936 that an Austrian Jew had better leave Europe for the United States. This was far easier said than done, however, because there were hardly any positions available in the United States, or elsewhere for that matter, and many highly qualified physicists who anticipated a Nazi onslaught were competing for them. To further his chances Viki decided that he should publish something on the new rage (nuclear physics) and do so in English. He carried this off brilliantly with the second of his Copenhagen papers—an original application of statistical mechanics to the evaporation of neutrons from nuclei—and published his results in *Physical Review* (1937).

Viki's first opportunities to leave were a professorship in Kiev and a senior research position in Moscow. In late 1936 he and Ellen visited the Soviet Union and promptly realized that the political climate had deteriorated drastically since Viki's earlier visit to Russia. He would therefore only consider a position there if nothing else were available. In those years Bohr went regularly to England and America to "sell'' the refugees at his institute. In 1937 Bohr convinced the University of Rochester to offer Viki a poorly paid instructorship, which he accepted.

ROCHESTER AND LOS ALAMOS

In the fall of 1937 Viki began a new phase of his career with a new country, a new language, and a largely new field. In his five and a half years at Rochester, nuclear physics became a major focus of his research, with studies of Coulomb excitation[5] and radiative transitions (1941) being especially noteworthy. He continued to work on QED and published a remarkable paper addressing the self-energy problem in greater detail, but more significantly proving that the self-energy diverges logarithmically to all orders in perturbation theory (1939). This paper strengthened his result of 1934.

Fission was discovered in Berlin in December 1938, and on September 1, 1939, Germany invaded Poland. Nuclear physics was suddenly transformed from an esoteric intellectual pursuit into a potentially decisive factor in the war. Not being a citizen—indeed, an enemy alien until he and Ellen became U.S. citizens in 1943—Viki did not participate in secret war-related work until early 1943, when Robert Oppenheimer asked him to come to Los Alamos, where he remained until late in 1945.

No data were then available for many of the processes involved in producing a nuclear explosion or the ensuing effects. The bomb project had to rely on theorists for guidance on many fronts. Viki became a prominent member of the theoretical physics division: His office was known as "the seat of the oracle" in recognition of his ability to quickly devise qualitative solutions to physics problems. Hans Bethe, the division head, eventually needed support in running his expanding team and appointed Viki as his deputy. Because he had been in charge of the calculations about the effects of the bomb, Viki was one of the few theorists to witness the Trinity test at Alamogordo.

MIT AND QED AGAIN

In early 1946 Viki joined the faculty at MIT and during that fall began teaching and research again. With his student Bruce French he revisited the electron self-energy problem to explore an earlier suggestion of Hendrik Kramers that one might make sense of the higher-order radiative corrections in electrodynamics in spite of the infinity in the self-energy of the electron. Kramers had pointed out that what is actually observable is the energy difference between free and bound states of electrons. Viki's demonstration that the divergence is only logarithmic made it plausible that *differences* would be finite and meaningful. French and Weisskopf had not completed their calculation when, in June 1947, Willis Lamb announced the results of his microwave experiments on hydrogen, showing a tiny disagreement with existing theory, with a very small energy difference between two levels supposedly degenerate. Many theorists pounced on this result, which became known as the "Lamb shift." Bethe quickly showed in a nonrelativistic calculation with a cutoff that Kramer's idea led to a level shift close to that measured by Lamb. His work depended, however, on the plausible but unproven assumption that the logarithmic divergences at high energy exactly canceled.

By early 1948 French and Weisskopf completed the first consistent calculation of the Lamb shift, but Viki would not publish because they had a very small disagreement with the independent calculations of Richard Feynman and Julian Schwinger, who agreed with each other. Viki could not believe that his work with French was correct. Surely the two young geniuses who were using their new and much more powerful techniques had not made the same mistake. But they had! The upshot was that French and Weisskopf published their year-old result (1949) only after a paper by Kroll and Lamb appeared with essentially the same calculation.[6]

The Kroll-Lamb theory paper contains a succinct statement about Viki's place in the firmament of theoretical physics: "[Our] calculation," they wrote, "[is] based on the 1927-34 formulation of quantum electrodynamics due to Dirac, Heisenberg, Pauli, and Weisskopf." Despite or perhaps because of such praise, in his autobiography Viki indulges in self-criticism. He laments that he had not had the insight to pursue more diligently his 1936 work in which he realized that just the charge and mass of the electron were affected by the short-distance (high-frequency) divergences, that he and French did not work hard or fast enough to have made a *prediction* of the Lamb shift before its experimental observation, and that his fear of publishing a wrong result caused them to miss being the first to publish the correct result. His wistful “I might even have shared the Nobel Prize with Lamb” (1991, p. 169) sums up his view of what might have been. Such regrets aside, Weisskopf stands among the leaders of twentieth-century theoretical physics and a key player in the development of quantum electrodynamics and field theory in the 1930s and 1940s, as was recognized by the Wolf Prize in 1981.

NUCLEAR PHYSICS

Viki often derided his own technical abilities in theoretical physics. He once said he was contributing his “don’t know how” to a collaborative effort. But he was justifiably proud of his remarkable ability to arrive at results by intuition, by exploiting basic principles and making educated guesses. In this regard he would express his gratitude to Paul Ehrenfest, who had been a charismatic visitor to Göttingen when Viki was a student. But as is true of other masters of the intuitive argument, Viki acquired his magical ability by having devoted his youth to technically difficult calculations. As with the great pianist who can improvise so effortlessly, an enormous amount of hard, tedious work lies behind the magic.

Nothing illustrates better his talent of focusing on the essential physics with simple intuitive descriptions than his work in nuclear physics, his primary interest in the postwar period. In this research he found the perfect collaborator in Herman Feshbach. Together and with students and postdocs they published a series of papers on nuclear reactions that are noteworthy for the clarity, plausibility, and simplicity of their assumptions (1947, 1949). In addition to his own research he produced his *magnum opus*, the treatise on nuclear physics written in collaboration with John Blatt (1952). This became the bible for several generations of nuclear theorists.

The most influential paper of this period was with Feshbach and Charles Porter (1954). It describes the total and elastic scattering cross section of neutrons (averaged over individual resonances) through a seemingly incompatible blend of the single-particle shell model and Bohr's concept of the compound nucleus. A vast amount of data is described remarkably well with this approach. In a little known paper Francis Friedman and Viki explored the compatibility of the single-particle and compound nucleus pictures with a masterful mixture of qualitative and quantitative arguments (1955).

DIRECTOR GENERAL OF CERN

By the late 1950s Viki was at the apex of his research career. He was elected president of the American Physical Society for 1960. In 1961 his appointment as director general of the European Organization for Nuclear Research (CERN) near Geneva suddenly transformed Viki from an academic research scientist heading a handful of colleagues and students into the chief executive of a young, large, and burgeoning multinational enterprise. Having seen Oppenheimer succeed in a similar if much more dramatic metamorphosis, Viki saw fit to tell the CERN Council in his "job interview" that not only did he have no administrative experience but "I consider this my strength." Apparently it was, for he proved to be an inspiring and imaginative leader of the laboratory, and a skillful diplomat in the complex political setting in which CERN is governed and funded.

When Viki took over as director general, CERN had a successful research program in nuclear physics with its synchrocyclotron, but only the beginnings of a high-energy physics program. Envisioning CERN as a world-class high-energy physics laboratory, Viki in his first address to the CERN Council at the end of 1961 outlined plans for the innovative intersecting storage rings (ISR) to be fed by the new 20-GeV proton synchrotron, and also spoke of a future 300-GeV accelerator. During his tenure as director general the number of staff and participants more than doubled to 2,500. The laboratory began to make major discoveries. The proposals for the ISR and a 300-GeV machine led to the formation of an internal committee to assess priorities and costs, which grew by 1966 into the European Committee on Future Accelerators. By the end of Viki's term in December 1965 the ISR had become a reality, as the CERN Council funded its construction and also R&D for a 300-GeV accelerator.

At CERN Viki is also remembered for his nighttime visits to the experimental halls and his down-to-earth seminars on particle theory for experimental physicists. A significant factor in Viki's decision to seek the CERN position had been his desire to learn particle physics—a rather extravagant measure for satisfying so modest a wish, but quite typical of him. This motive charged his many educational activities at CERN with an inspiring enthusiasm.

Viki's controversial championing of the ISR, the world's first proton-proton collider, and also of the 300-GeV machine set the tone and spirit of European high-energy physics as a serious competitor to the United States, until then the dominant player in the field, with long-lasting significance for physics everywhere. After returning from Europe

Viki recommended the formation of the influential High Energy Physics Advisory Panel to the Atomic Energy Commission (later the Department of Energy), and was the first HEPAP chair.

RETURN TO MIT

On returning to MIT Viki served as department chair for six years and engaged in intermittent research on particle physics with junior colleagues. Numerous honors came his way—the Max Planck Medal (1956), MIT Institute Professor (1966), Pontifical Academy of Sciences (1975), Ordens Pour le mérite für Wissenschaften und Künste (1978), the National Medal of Science (1980), the Wolf Prize (1981), the Enrico Fermi Award (1988), the Public Welfare Medal of the National Academy of Sciences (1991), as well as many honorary degrees and foreign memberships to prestigious academies. He served for four years as president of the American Academy of Arts and Sciences during a crucial period of consolidation. Along the way he authored a number of books and collection of essays, including his autobiography and coauthored (with one of the present authors, K.G.) a two-volume work *Concepts of Particle Physics* (1984, 1986) that had its origins in popular lectures to summer students at CERN. He reached mandatory retirement in 1974 and became professor emeritus.

CONCERNED SCIENTIST

The threat to humanity posed by nuclear weapons was a preoccupation of Viki's ever since he participated in the discussions initiated by Niels Bohr at Los Alamos before the Trinity test. After the war Viki was among the Manhattan Project scientists who organized what became the Federation of American Scientists. At that time he was also a member of the small committee chaired by Einstein that sought to inform the public about the bomb. In the 1950s Viki participated in the first Pugwash meetings between Western and Soviet nuclear scientists, and continued thereafter to reach out to influential Soviet scientists in pursuit of nuclear arms control. His friendships from the 1930s were of great advantage. Viki joined the Union of Concerned Scientists when it was founded in the MIT physics department, which he then chaired, and he later became a member of its Board of Directors. After his election in 1975 to the Pontifical Academy of Sciences Viki played a central role in convincing Pope John Paul II to speak out repeatedly against the nuclear arms race.

Viki worked incessantly for control and reduction of nuclear weapons and for international cooperation in science. At CERN he encouraged the reciprocal participation of CERN and Soviet-block physicists in each other's high-energy physics programs. He believed deeply in the role of science and scientists in making the world a more peaceful and safer place.

> Science is a truly human concern; its concepts and language are the same for all human beings. It transcends any cultural and political boundaries. Scientists understand each other immediately when they talk about their scientific problems; it is therefore easier for them to speak to each other on political or cultural questions and problems about which they may have divergent opinions. The scientific community serves as a bridge across boundaries, as a spearhead of international understanding (1989, pp. 7-8).

TEACHING AND STUDENTS

Not only was Viki a research scientist, administrator, humanist and internationalist, he was also a wonderful teacher and mentor to aspiring physicists. His deep understanding of fundamental principles and his intuition as to what was essential made his formal lecture courses inspirational, despite his well-known cavalier attitude about pedagogic precision (4π was often approximated by unity, for example). During his summers at CERN after being director general he initiated immensely popular introductory lectures on particle physics for the summer students, also attended by many CERN staff. The authors and others continued the tradition; a two-volume work (1984, 1986), already mentioned, was an outgrowth.

Teaching also included direction of research, of course. Among students at Rochester formally under Viki's supervision were Esther M. Conwell (M.S., 1944) and Ernest D. Courant (M.S., 1942; Ph.D., 1943); Robert Dicke and John Marshall, Jr., acknowledge their indebtedness to him in their theses. At MIT Viki had 21 Ph.D. students, among them the present authors, a Nobel laureate, and MIT faculty colleagues.[7]

MUSIC

Viki's love affair with the piano and classical music persisted through his whole life. In his autobiography half of the final chapter ("Mozart, Quantum Mechanics, and a Better World") is devoted to discussion of his loves in music, both as a listener and a participant. His musical companions recognized him as an enthusiastic pianist with a deep appreciation for Beethoven, Mozart, Schubert, and other classical composers. That music was an integral part of his being is illustrated by a story told by Maurice Jacob of Viki at a public scientific lecture in Paris.[8] Trouble with the sound system caused a few minutes delay. As he waited to begin Viki noticed a grand piano in the corner of the stage, went over to it, sat down, and began to play. The audience was enchanted.

CODA

A sketch of Viki that is confined to his career, even though it was so complex and productive, would only paint the palest shadow of the man. What was really unique about Viki was his vibrant personality. He conveyed an infectious happiness—as he put it, "I have lived a happy life in a dreadful century!"—for had he not seen Hitler and Stalin in action and witnessed the first engineered nuclear explosion? He maintained this happy disposition even under difficult circumstances, as when he spent his early months as CERN director in a Boston hospital suspended in an orthopedic contraption following an auto accident in Geneva.

Viki's almost tangible happiness was not just a signature of his own personality but was in large measure due to his good fortune in having found two wonderful women to accompany him through life: Ellen Tvede, his wife for 55 years until her death in 1989, and Duscha Scott, his second wife, who gave him joy and vital support in his final decade. Ellen and Viki had two children after coming to the United States: Thomas E. Weisskopf, professor of economics at the University of Michigan, Ann Arbor, and Karen Worth, senior scientist, Center for Science Education at Education Development Center, Inc., Newton, Massachusetts.

Victor Weisskopf combined in himself two traits that are often in conflict and rarely coexist so harmoniously: On one side the sentimental and the romantic, on the other the rigorous intellectual discipline and judgment. As he liked to say, his favorite occupations were Mozart and quantum mechanics. He called his popular and wide-ranging exposition of science *Knowledge and Wonder* (1962). In giving talks to lay audiences about cosmology he often played the part of Haydn's *Creation* that accompanies the words "And there was light." Viki exemplified the vitality and imagination that produced one of history's great intellectual revolutions, and gave it a human face.

NOTES

We acknowledge our debt to Viki's autobiography for many factual details. On technical and other matters we have lifted unashamedly (with official or tacit permission) from our own writings about Viki in other venues.[9] We thank the departments of physics at the Massachusetts Institute of Technology and the University or Rochester for assistance concerning Viki's students and other matters.

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Gordon R. Willey

GORDON RANDOLPH WILLEY

March 7, 1913–April 28, 2002

BY EVON Z. VOGT, JR.

GORDON RANDOLPH WILLEY, who died of heart failure at his home in Cambridge, Massachusetts, on April 28, 2002, in his eighty-ninth year, was universally recognized as one of the premier American anthropologists of the twentieth century. His pivotal contributions to science during his long productive life included excavations of archaeological sites in the southeastern United States, Peru, Panama, Belize, Guatemala, and Honduras; detailed ceramic studies that provided data on cultural change in prehistoric societies through time and through space; syntheses of common cultural themes in Mesoamerica and South America; and especially his creation of the field of "settlement pattern studies," an extraordinary theoretical and methodological advance that he pioneered in the Viru Valley of Peru in a single season, in 1946 (Fash, in press).

Gordon Willey was born in Chariton, Iowa, on March 7, 1913, the only child of Frank and Agnes (Wilson) Willey. His father was a pharmacist in the small town of Chariton. This fact later became significant for Willey when he learned that the father of Professor Alfred M. Tozzer, his predecessor at Harvard, had also been a pharmacist of middle-class status in the town of Lynn, Massachusetts. It was only after

Professor Tozzer married Margaret Castle from one of the wealthy and famous "five families" of Hawaii that he became a member of the elite in Cambridge.

At the age of 12 Willey moved to Long Beach, California, with his parents. At Woodrow Wilson High School he excelled in academics and in track. He set several school records, including the 60- and 220-yard dashes. Upon graduation from high school the University of Arizona recruited him.

After reading William Prescott's *Conquest of Mexico and Peru* he had decided he wanted to study archaeology. His Latin American history teacher persuaded Willey that he should study under Professor Byron Cummings, the renowned field archaeologist and Southwestern expert at the University of Arizona. Cummings served as dean of the Faculty of Sciences, Arts, and Letters and was an athletic booster. Willey fondly remembers his arrival in Tucson in the autumn of 1931, when he was greeted by the University of Arizona band, which had marched to the station to meet his train. He later recalled this experience as one of the high points of his early career.

At Arizona Willey took many courses with Byron Cummings (he especially enjoyed his courses on Mexico) and with Charles Fairbanks, who taught the courses on dendrochronology (the study of tree rings to date archaeological sites). Cummings took Willey along on field expeditions in the Southwest and to dig at the site of Kinishba in east-central Arizona. After completing his A.B. in anthropology in 1935, he continued on at Arizona, obtaining his M.A. the following year. During this year in graduate school Willey earned extra money by serving as the freshman track coach.

Willey applied to several schools with leading doctoral programs, including ironically Harvard, but he was denied admittance. When Dean Cummings secured a Laboratory of Anthropology Field Fellowship for him with Arthur Kelly in Macon, Georgia, Willey eagerly accepted and spent the summer of 1936 excavating at the Stubbs mound and learning about the sites of the region, their ceramics, chronology, and other aspects of material culture. He also observed the finer administrative points of running this major excavation funded by the Works Progress Administration. His first publication (1937) was a preliminary effort to establish a dendrochronology for the Southeast. This reflected his work with Fairbanks at Arizona as well as being a harbinger of a career devoted to the mastery of "time-space systematics" (Fash, in press).

It was also in Macon that Willey met, courted, and married in 1938 the charming Katharine Winston Whaley who became his lifelong mate through 63 years of happy marriage until she died in 2001.

Willey continued his WPA work in Louisiana. His first monograph, published in 1940, was the product of collaboration with James Ford and was titled *Crooks Site: A Marksville Period Burial Mound in LaSalle Parish, Louisiana*. The collaboration with Ford also led to an innovative article "An Interpretation of the Prehistory of the Eastern United States" (1941) that became a classic in American archaeology. I recall reading and admiring this article as a graduate student in anthropology at the University of Chicago.

With the continuing support of Dean Cummings and Willey's hard work in the field, Willey was finally admitted in 1939 to the doctoral program at Columbia University. His new teacher and mentor at Columbia was William Duncan Strong, who was noted for introducing his students to the great figures in the field and setting up meetings for discussions of the burning issues of the day.

Willey excavated in Florida in 1940 and published on the area (1949), but he shifted his geographic focus from the southeastern United States to South America when Duncan Strong invited him to go to Peru in 1941. As a fledgling archaeologist Willey had no intention of pursuing Latin American studies, but in 1940 the energetic Peruvian scholar Julio C. Tello convened a meeting at the American Museum of Natural History in New York to promote Andean research. At this meeting Duncan Strong was persuaded to undertake research in Peru, and he invited Willey to go along.

Strong and Willey excavated at Ancon and Supe, and later at the famous Inca oracle site of Pachacamac. From September 1941 to March 1942 Willey and James Corbett excavated at Chancay, Puerto de Supe, and Ancon. Willey's Ph.D. dissertation was based on the research at Chancay and was titled *Excavations in the Chancay Valley, Peru*, *1943* (Moseley, 2003).

Upon completing his Ph.D. in 1942 Willey served a year as an instructor of anthropology at Columbia. The following year he landed a post at the Bureau of American Ethnology at the Smithsonian Institution, where he worked under the direction of Julian Steward from 1943 to 1950. Much of his time was devoted to working on the monumental *Handbook of South American Indians,* edited by Julian Steward. Willey edited many of the articles as they were received and he wrote many himself.

Willey's next field experience occurred in the Viru Valley in Peru in 1946 as a member of the project team consisting of archaeologists Duncan Strong, Clifford Evans, James Ford, Junius Bird, Donald Collier, and Willey, along with geographer F. W. McBryde and the social anthropologist Alan Holmberg.

While most of the archaeologists continued to engage in the useful data gathering they had followed before, Willey was convinced by Julian Steward to withdraw from the "stratigraphic race" being run by the others and to undertake a "settlement pattern survey." As Willey later wrote: "I would be doing more for the project, myself, and archaeology, he argued, if I attempted to say something about the forms, settings, and spatial relationships of the sites themselves and what all this might imply about the societies that constructed and lived in them" (1974, p. 153). Willey conducted the settlement survey, which consisted of making maps from aerial photographs, checking these maps in the field with compass and chain measurements, and recording details of the setting and forms of architecture.

At the time of the Viru Valley survey Willey did not appreciate the potential of the settlement pattern survey either as a new viewpoint for archaeologists or as an integrating force for the various other studies that were being conducted in the valley. Willey later reported that

> in that 1946 field season, as I walked over the stony and seemingly endless remains of Viru's prehistoric settlements, I felt I had been misled by Steward and dealt a marginal hand by my colleagues. The latter were getting tangible pottery sequences to delight the heart of any self-respecting archaeologist while I was chasing some kind of wraith called "settlement patterns" that had been dreamed up by a social anthropologist (1974, p. 154).

As it turned out Willey's *Prehistoric Settlement Patterns in the Viru Valley, Peru* (1953) became a classic. It not only recorded the settlement patterns for the whole valley in meticulous detail but it also served as the fulcrum for integrating all the information gathered in the valley. It is clear that this was a monograph that would set new guidelines and standards for future archaeologists and archaeological research programs (Vogt and Leventhal, 1983, p. xvi).

Gordon Willey pursued the settlement pattern approach in archaeological field research shortly after he was appointed to the Bowditch professorship at Harvard in 1950. In 1948 he had been excavating in Panama to investigate the interesting question of pre-Columbian relationships between Peru and Mesoamerica and he had planned to move north gradually, reaching the Maya region from the south. But Professor Alfred Tozzer insisted in 1952 that Willey move his research immediately to the Maya area in accord with the wishes of C. P. Bowditch, who had bequeathed the funds to Harvard for the professorship that bore his name.

Willey followed Tozzer's advice and began his Maya field studies in British Honduras (later Belize) in 1953. He selected an area in the Belize River valley, an area whose river banks had recently been cleared of tropical forest, and promptly set out to plan the Belize Valley settlement pattern project that was "to change the course of Maya archaeology forever" (Fash, in press). It was the first archaeological program to be supported by the National Science Foundation and it produced new insights into Classic Maya settlements and lifeways (Ashmore, 2003). Almost all the Maya archaeological research carried out previously had concentrated on the centers of the ancient sites with their pyramids, temples, and palaces and had ignored the outlying settlements where most of the Maya lived and grew their crops of maize, beans, and squash (1956, 1965). As Fash explains,

> The Belize River Valley work was to leave its mark on Maya archaeology, in a way that very few projects in that part of the New World have, before or since. The settlement pattern focus proved extremely productive, showing that there were densely populated areas even away from the major centers, and leading to examinations of the structure of ancient Maya society (Fash, in press).

The settlement pattern focus had the additional scientific advantage of relating to ethnographic studies of the structure of contemporary Mayan communities and especially to the analyses of cultural continuities and changes in Mayan culture from the time of the Classic Maya to the present.

Shortly after Willey's arrival at Harvard, I discovered that he and I shared a deep interest in settlement patterns. Because my A.B. at the University of Chicago was in the Department of Geography, I was trained to think of the dispersal of human settlements on the landscape as a productive way of understanding the relationship of culture to the natural environment. Gordon Willey's publications on the Belize Valley project stimulated me to pay close attention to settlement patterns when I began my long-range study of Tzotzil-Maya communities in the highlands of Chiapas.[1] Willey and I also taught courses together (especially "Peoples and Cultures of North and South America") and offered a joint seminar on "The Maya" for many years.

In the spring of 1960 Gordon Willey and I decided to exchange field visits in order to learn more about each other's research operations. Gordon was excavating in Guatemala and I was making an ethnographic study of the Zinacantan in Chiapas. In March I drove our Land Rover to Guatemala City, where I met Gordon. The following day we flew in a small plane to Sayaxche in the Peten and from there traveled by motorboat down the Rio Pasion to the famous Maya site of Altar de Sacrifios, located near the junction of the Rio Pasion and the large Rio Usumacinta. Gordon had been excavating Altar de Sacrificios for a number of years, and I spent a week with him at his wonderful field camp that had been set up by the efficient and congenial field director A. Ledyard Smith.

Each day we visited the archaeological site where a large crew of Guatemalans was excavating this Classic Maya center. Work stopped in the hot late afternoon, and we would go for a swim or take one of the boats and go fishing. At sundown Ledyard would have one of his men cut a heart of palm for hors d'oeuvres. Then Ledyard would break out a bottle of S. S. Pierce whiskey. He always alternated between scotch and bourbon. When the bottle was empty, it would be time for dinner at the camp. We would then fall into our cots, carefully covered with a mosquito net, for a deep sleep, broken only by the cries of howler monkeys in the tropical night.

It was a wonderful week. I was impressed with the quality of the meticulous archaeological work and by the well-organized daily schedule. However, I found the heat of the tropical selva enervating, and was again glad I was doing my ethnological research in the cool highlands.

After we flew back to Guatemala City, Gordon and I drove to San Cristobal Las Casas for a visit at my field station in the highlands of Chiapas before continuing our joint travels back to Boston. The mayor of San Cristobal, whom I knew well, kindly invited us to stay in his house, so we had comfortable accommodations.

During the visit in Chiapas I drove Gordon to the Tzotzil-Maya-speaking Chamula Center, Zinacantan Center, and the Zinacanteco hamlet of Paste, where I had a field house, to observe our research operations. I recall that Gordon, who was used to the systematic daily schedules of an archaeological field camp, was rather uncomfortable with the constant changes and demands of ethnological research in which one has to adapt to, for example, a sudden evening request (just at dinner time) of an informant for a ride to Zinacantan Center to locate a shaman to perform a curing ceremony for a sick child. Organized daily schedules are simply not possible as they are in archaeological work. Gordon himself recognized this in a recent comment: "I would have made a poor ethnologist. I have often thought archaeologists and ethnologists have gone on their different career ways because of their different temperaments. . . . Nevertheless, ethnologists and archaeologists can learn from each other."[2]

Willey's later excavations in the Maya region at Altar de Sacrificios (1958-64) and Seibal (1964-68) in the Rio Pasion River valley of Guatemala and in Honduras at Copan (1975-77) resulted in dozens of monographs on subjects ranging from ceramics and artifacts to architecture, epigraphy, and settlement patterns (Fash, in press).

Willey also had a deep interest in archaeological systematics. This interest was best expressed in his highly influential 1958 book with his Peabody Museum colleague Philip Phillips, *Method and Theory in American Archaeology*. This path-breaking volume was "must" reading for a generation of American archaeologists and was recently republished. In the book Willey and Phillips arranged all of New World culture history in time and space in a masterful overview of the field. The book presented a view of American archaeology from the mainstream; it was a rebuttal to the sharp criticisms that had been leveled at Maya studies by Clyde Kluckhohn and later by his student, Walter Taylor, in *A Study of Archeology* (1948) (Leventhal, 2003).

Willey's diverse interests in New World prehistory and his willingness to take on the big issues of the day propelled him into the forefront of American archaeology. One example was his presidential address on "The Early Great Art Styles and the Rise of Pre-Columbian Civilizations" presented at the American Anthropological Association meetings and published in 1962. This search for causality in the ideological realm was a far-sighted attempt at plotting a new course. Marcus (2003) recently reviewed Willey's evaluation of the similarities of Mesoamerican Olmec and Andean Chavin art styles. She concluded that Willey's central question—What role did Olmec and Chavin art play in the rise of civilization?—should be rephrased as: Was the rise of first-generation states stimulated in some way by the prior appearance of widespread art styles among neighboring chiefdoms? Marcus suggests that much of chiefly art communicated and extolled the attributes that chiefs wanted, such as bravery and success in battle, and close genealogical ties to heroic ancestors and supernatural forces. Sometimes that art could reach the level of greatness. In the case of the Early Horizon art in Mexico and Peru it was eventually followed by state formation. But in Panama, chiefdom followed chiefdom until the arrival of the Spaniards (Marcus, 2003).

Another example was his 1976 article "Mesoamerican Civilization and the Idea of Transcendence," published in the British journal *Antiquity*. In this article Willey opines, "I cannot be satisfied to believe that we have all of the worthwhile answers about human cultural behavior in the data of subsistence, demography, war, trade, or the processes of social class differentiation" (p. 213).

With his masterful ability to synthesize disparate information Willey proceeded to consider the significance of the Mesoamerican mytho-historical figure of Topiltzin Quetzalcoatl in light of the concept of ideological transcendence. In this effort he sought to place Mesoamerican religion on a par with Old World civilizations with transcendent movements such as Judaism and Buddhism, among others. He boldly proposed that the antiwar, antihuman sacrifice ethic embraced by Topiltzin Quetzalcoatl transcended the aggressive political character espoused by most Mesoamerican states in the wake of the collapse of Teotihuacan and suggested that transcendent movements appeared during times of civilization crises (McAnany, 2003).

Willey's Maya work was also the inspiration for three highly influential advanced seminars at the School of American Research in Santa Fe: *The Classic Maya Collapse* (Culbert, 1973), *The Origins of the Maya Civilization* (Adams, 1979), and *Lowland Maya Settlement Patterns* (Ashmore, 1991). Willey wrote and coauthored the summary statement for all of these and also wrote the introductory chapter for the School of American Research volume on *Late Maya Civilization* (Sabloff and Andrews, 1986).

Willey served as the president of the American Anthropological Association in 1960-62 and of the Society of American Archaeology in 1967-68. He was a fellow of the American Academy of Arts and Sciences (elected 1952), the Royal Anthropological Institute of Great Britain and Ireland, the Society of Antiquaries, a member of the Sociedad Mexicana de Antropologia, the National Academy of Sciences (elected 1960), the American Philosophical Society (elected in 1984), and a corresponding member of the British Academy.

When he was elected to the American Academy of Arts and Sciences, he was told by Professor Tozzer, who nominated him, "I got you into the American Academy, but there are two higher ranking academies in the United States, the National Academy of Sciences and the American Philosophical Society, and it's up to you to get yourself into those academies."

Willey's awards included the A. V. Kidder Medal for Archaeology, the Order of the Quetzal from the government of Guatemala, the Gold Medal for Distinguished Archaeological Achievement from the Archaeological Institute of America, the Viking Fund Medal for Archaeology, the Huxley Medal from the Royal Anthropological Institute, the Distinguished Service Award from the Society for American Archaeology, the Walker Prize from the Boston Museum of Science, the Lucy Wharton Drexel Medal for Archaeology from the University Museum of the University of Pennsylvania, and the Gold Medal from the London Society of Antiquaries. He was awarded honorary doctorates from the University of Arizona, the University of New Mexico, and the University of Cambridge (U.K.), where he was a visiting lecturer in 1962-63 and an overseas fellow at Churchill College in 1968-69.

Willey was also repeatedly invited to attend international symposia at the Wenner-Gren Foundation for Anthropological Research's Burg Wartenstein near Gloggnitz, Austria. Two of these symposia were focused on Maya studies, which I organized and chaired: "The Cultural Development of the Maya" in September 1962, which included 10 Maya specialists (1964), and "Prehistoric Settlement Patterns: Retrospect and Prospect" in August 1980 with essays in honor of Gordon R. Willey who served as the discussant (1983).

After his scientific writing was complete, he turned to writing fiction, including a mystery novel, *Selena,* based on his field experience in Florida, as well as to numerous award-winning plays and musicals he wrote for the Tavern Club of Boston, where he served as president from 1973 to 1975.

Gordon Willey had a great capacity for friendship and for collaboration with colleagues old and young. He was always modest about his own achievements and made certain that proper credit was given to all who worked and published with him over his long lifetime. Understandably he became a mentor for dozens of students and younger colleagues in anthropology.

But Gordon Willey's impact in the long run will come from his impressive publications, articles in scientific journals, technical monographs on every site he ever excavated, and more general books, such as his *A History of American Archaeology* with J. A. Sabloff (1976, 1980, 1993) and his masterful two-volume *An Introduction to American Archaeology* (1966, 1971).

His colleagues in anthropology and other sciences throughout the world will long and warmly remember Gordon Willey and his outstanding scholarly contributions.

Gordon Willey is survived by two daughters, Alexandra Guralnick and Winston Adler, his sons-in-law, Peter Guralnick and Jeffrey Adler, and five grandchildren.

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NOTES

1. See E. Z. Vogt. Some aspects of Zinacantan settlement patterns and ceremonial organization. *Estudios de Cultura Maya* 1(1961):131-45; Some implications of Zinacantan social structure for the study of the ancient Maya. *Actas y Memorias del 35th Congreso Internacional de Americanistas* 1(1964):307-19; *Zinacantan: A Maya Community in the Highlands of Chiapas,* 1969 (Cambridge: Harvard University Press); *Tortillas for the Gods,* 1976 (Cambridge: Harvard University Press); *Fieldwork Among the Maya,* 1994 (Albuquerque: University of New Mexico Press).

2. G. R. Willey. Vogt at Harvard. In E*thnographic Encounters in Southern Mesoamerica: Essays in Honor of Evon Z. Vogt, Jr.*, eds. V. R. Bricker and G. H. Gossen, pp. 21-32. Austin: University of Texas Press, 1989.

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