Biographical Memoirs V.82

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Biographical Memoirs

VOLUME 82

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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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VOLUME 82

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John D. Axtell

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JOHN DAVID AXTELL

February 5, 1934–December 2, 2000

BY ARNEL R. HALLAUER

JOHN AXTELL WAS RECOGNIZED nationally and internationally for his research on sorghum (*Sorghum bicolor* L.). He had broad interests in the breeding and genetics of sorghum, but his research emphasized grain and forage quality and genetic improvement of sorghum germplasm. One of Axtell's major scientific findings was the discovery and identification of genetic factors responsible for reduced protein availability in grain sorghum. He led an interdisciplinary research team of biochemists, nutritionists, and geneticists who demonstrated that tannins had a negative effect on protein availability and hence on nutritional quality of grain sorghum.

Axtell also recognized the importance of increasing the content of the essential amino acid, lysine, in grain sorghum and the impact that increased lysine levels would have on improving the diets of humans who depend on grain sorghum as a staple food component. He pioneered a successful research program that identified natural and induced high lysine mutants in grain sorghums. Agronomic deficiencies associated with high lysine mutants of cereal crops have limited widespread cultivation of corn (*Zea mays* L.) and barley (*Hordeum vulgare* L.). But the high lysine sorghum

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cultivar from Ethiopia that was discovered by Axtell in a systematic survey of the world sorghum collection remains the only high lysine cereal crop under extensive cultivation. The high lysine cultivar is known locally as *Wotet Begunchie* (translated as *milk in my mouth*) and continues to be widely grown in the Wollo region of northwest Ethiopia.

Axtell most recently focused his research on the digestibility of sorghum proteins. He and his colleagues conducted extensive studies that clearly established that methods of food preparation had dramatic effects on the digestibility of sorghum proteins. Axtell and his colleagues developed a laboratory assay to determine the digestibility of sorghum-based foods without having to feed the sorghum to animals. Further, they demonstrated with use of the laboratory assay that the proteins of various cereal crops behave differently when cooked and that the cooking process was responsible for the decreased protein digestibility in sorghum. Further studies by Axtell and his colleagues clearly demonstrated that a traditional African fermented food product, *nasha*, significantly improved the digestibility and utilization of protein and energy from sorghum. These findings suggested that the traditional African societies that depend on sorghum as a staple have evolved food preparation methods that reduce some of the nutritional deficiencies associated with sorghum. These findings were corroborated from other studies conducted in Axtell's laboratory using other traditional African sorghum products, such as *kisra, asida, ugali,* and *marisa.* The research on digestibility ultimately led to the discovery and development of new, novel, highly digestible strains of sorghum. Germplasm developed from Axtell's research on digestibility has been widely distributed to the seed industry, national research programs, and international research centers.

Axtell also made significant contributions in the education and training of graduate students. He was a very effective

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teacher in the classroom and took great pride and joy in working with graduate students. Students studying and working with Axtell not only learned the essential technical research skills but they also were stimulated by his enthusiasm and dedication to research; they were inspired by his example. He served as major advisor of 39 graduate students who fulfilled the requirements for M.S. and Ph.D. degrees and served on advisory committees for more than 100 graduate students. His former graduate students currently occupy leadership positions in research and development in universities, in the seed industry, in international agricultural research centers, and in national research programs in developing countries. Axtell also had an international reputation as a counselor and teacher. Through his students he has made significant contributions to the quantity and quality of food available in the world.

John Axtell made valuable contributions to the agricultural sciences during his 34 years of tenure at Purdue University. The trademark of Axtell's university career was interdisciplinary research. He consistently and increasingly worked towards developing a team effort in addressing research problems in protein quality, tannin biochemistry, and forage quality of sorghum. Internationally his contributions in basic research findings, germplasm enhancement, graduate student education, and leadership in plant breeding and genetics are widely recognized. His greatest passion was for long-term institution building and for human capital development, tools that he believed were essential for improving the lives of the poor in developing countries.

PERSONAL HISTORY

John David Axtell was born in Minneapolis, Minnesota, on February 5, 1934. He became interested in science and enrolled at the University of Minnesota for his undergraduate

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studies. He earned his B.S. degree in agronomy and plant genetics in 1957. He continued his education at the University of Minnesota and earned his M.S. degree in plant genetics in 1965. Because of his interest in genetics, he enrolled for further graduate studies in plant genetics at the University of Wisconsin, Madison, where he completed the requirements for a Ph.D. degree in plant genetics in 1967. Later in 1967 he accepted a faculty position in the Department of Agronomy at Purdue University. Axtell's contributions in teaching, research, and mentoring of graduate students were recognized by his peers and colleagues. He progressed rapidly in the academic ranks from assistant professor (1967-72), to associate professor (1972-75), to professor (1975-82), and to Lynn Distinguished Professor of Agronomy (1982-2000). His professional career of 34 years was at Purdue University, and he became one of the most recognized faculty members for his contributions to the Department of Agronomy, College of Agriculture, and Purdue University. Axtell was always available and willing to assist and counsel faculty members and graduate students. His genuine warmth and friendliness contributed to his status as a valued colleague and a caring mentor, both for colleagues and students.

John Axtell is survived by his wife of 43 years, Susan D. Kent, in West Lafayette, Indiana; son John, Jr., of Ridgewood, New Jersey; daughter Catherine of Albuquerque, New Mexico; and daughter Laura of Minneapolis. His genuine warmth, caring, generosity, and enthusiasm for life were also extended to his family. Axtell's family has pleasant memories of the many recreational activities and journeys that he planned for them. The children recall that he made routine activities special and special occasions magical; he made every day a celebration of life.

Axtell's warmth for others was reflected when he would greet people with special gifts and a smile, because he took

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so much pleasure in giving. His children recall that his greatest gift, however, was to teach them the many important lessons by example. Throughout his life he demonstrated compassion and thoughtfulness in his actions toward others; he always remembered the importance of small acts of kindness and was grateful for the things others did for him. Through his actions he taught his children to work hard and to do their best, to strive to make a difference in the lives of others, to respect all people, and to celebrate the diversity in humankind.

It was an unexpected and severe loss to his family, colleagues, and peers when John Axtell suddenly died on December 2, 2000, at St. Elizabeth Medical Center, Lafayette, Indiana. He was very active in teaching, research, and participation in professional activities until the time of his death.

PROFESSIONAL HISTORY

John Axtell's professional career of 34 years was at Purdue University. During his tenure there he made a broad range of contributions to the agricultural sciences. Although his primary responsibilities were teaching and research, he also made significant international contributions in basic research, germplasm development, and scientific leadership in plant breeding and genetics. During his professional career John Axtell's research emphasized the genetics and improvement of sorghum, both as a staple in human diets and as feed for animals.

A major scientific finding of Axtell's was the discovery and identification of factors responsible for the reduced protein availability or digestibility in grain sorghum. The presence of tannins, located primarily in the testa layer, conferred to the sorghum grain resistance to both bird damage and weathering. For this reason sorghum varieties high in tannin content are commonly grown and utilized in

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some areas of the world. Axtell and his students demonstrated for the first time that tannins had a negative effect on protein availability and hence on nutritional quality of sorghum. The significance of this discovery was two-fold. First, it resulted in a shift in emphasis of sorghum breeding programs around the world towards lower tannin sorghums. As a result, almost all sorghum varieties and hybrids developed and released for human consumption in the last 10 to 15 years have been of lower tannin types. Second, it attracted scientists in biochemistry, genetics, and food science to undertake research on the various aspects of tannin and phenolics in sorghum and other food products.

Recognizing the importance of increasing the lysine content of sorghum and the impact this would have on improving the diets of people who depend on sorghum as a staple diet, Axtell pioneered a successful research program that led to the identification of natural and induced high lysine mutants in sorghum. While agronomic problems associated with the high lysine cereal mutants have limited the widespread cultivation of high lysine corn and barley, the high lysine sorghum cultivar from Ethiopia, discovered by Axtell in a systematic survey of the world sorghum collection, remains as the only high lysine cereal under widespread utilization. This cultivar, known locally as *Wotet Begunchie* continues to be widely grown in the Wollo region of northwest Ethiopia. The high lysine sorghums are readily recognized by farmers in the region as special purpose varieties, and they maintain them by saving seed from their own fields.

In recent years Axtell focused his research on the digestibility of sorghum proteins. A study by scientists at Johns Hopkins University in the early 1980s, which involved feeding malnourished Peruvian children, suggested that the protein digestibility of sorghum gruel was significantly lower than that of wheat (*Triticum* sp.), rice (*Oryza sativa*), or

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corn. This study attracted the attention of various individuals, including leaders of donor agencies, who questioned the merit and ethics of supporting sorghum research for utilization by humans. In response Axtell and his colleagues conducted studies that clearly established that methods of food preparation could have dramatic effects on the digestibility of sorghum proteins. Axtell and his colleagues developed a technique for determining the digestibility value of sorghum-based foods in vitro. Using their in vitro assay, they demonstrated that the proteins of various cereals behave differently when cooked and that the cooking process was responsible for the decreased protein digestibility in sorghum. Furthermore, Axtell and his colleagues, using in vitro assay, rat balance tests, and human feeding of malnourished infants, clearly demonstrated that a traditional African fermented food product, *nasha*, significantly improved the digestibility and utilization of protein and energy from sorghum. This suggested that traditional African societies that depend on sorghum as a staple have evolved food preparation methods that alleviate some nutritional problems associated with sorghum. This has been corroborated from additional studies conducted in Axtell's laboratory using such traditional African sorghum products such as *kisra, asida, ugali,* and *marisa*. Thus, Axtell planned and executed scientific studies that helped clarify an issue that could have had serious ramifications for sorghum research, both in the United States and the less developed countries.

Axtell also made worldwide contributions in germplasm development. Varieties and populations of sorghum developed by him and his colleagues at Purdue University have been widely distributed to national research programs and to international research centers. These varieties have either been selected directly or utilized as parents in intercrossing programs by various breeders. A number of these varieties

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have been released for wide cultivation in India, Ethiopia, Colombia, and Kenya. An outstanding example is a Purdue sorghum variety P-954063 introduced to ICRISAT, which remains as the most widely used parental line in their program. P-954063 also has been the base population of a variety selected and released in Ethiopia under the name *kobomash*.

PUBLIC SERVICE

Research and teaching were Axtell's primary responsibilities, but he was an active participant and leader in local and international affairs. Locally he chaired numerous departmental committees; School of Agriculture committees, including three dean search committees; and several university committees. He provided counsel to department heads, deans, and leaders of national and international research programs and made valuable contributions to crop science and agricultural sciences in general during the past 30 years. He was a member of the Board on Agriculture and Renewable Resources of the National Academy of Sciences (1978-81); member of scientific review of Future Strategy of International Rice Research Institute at Los Banos, Philippines (1988); chairman of the Symposium on Strategies for Improving Crop Quality: The Next Challenge; International Crop Science Congress, Ames, Iowa (1992); member of the Committee on Plant Sciences, Commission on Life Sciences, National Research Council, Washington, D.C. (1993); member of the Scientific Council to the Gene Expression Center with the Board on Agriculture, National Research Council, Washington, D.C. (1991-93); member of the McKnight Foundation Oversight Committee of the Collaborative Crop Research Committee, Minneapolis, Minnesota (1993-97); and member of the National Research Council's Committee on Science, Technology, and Health Aspects of the Foreign Policy Agenda of the United States (1998).

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John Axtell was elected to membership in the National Academy of Sciences in 1982. He was a member who became a strong advocate and an effective spokesman for the Agricultural Sciences Section (62) and was elected chairman of that section for 1987-90. Because of his leadership of Section 62, he was elected chairman of Class VI (Applied Biology and Agricultural Sciences) of the National Academy of Sciences for 1994-97. John Axtell always attended the annual meetings of the National Academy of Sciences, and he was recognized as a leader in his section and class. He was a strong supporter of the importance of the agricultural sciences and their impact on providing adequate quantities of good nutritional food for the United States.

FOREIGN SERVICE

John Axtell's opinions were widely sought by peers and students in international agriculture development because of his excellent knowledge and understanding of the challenges and opportunities of international development. Most of Axtell's sorghum research was directed toward improving the nutritional value of sorghum, particularly where sorghum is a staple in the human diet. Consequently, Axtell, colleagues, and students worked closely with the peoples of Africa to improve sorghum quality and production. His willingness to participate and assist with international research programs will be a great loss. Axtell was an active participant and a strong supporter of the activities of the International Sorghum and Millet Consortium (INTSORMIL): he was a member of the Title XII Technical Committee (1979-82); chairman of the Title XII Technical Committee (1981); coordinator of activities in Niger (1983-95); chairman of the Ecogeographic Zone Council (1983-85 and 1993-94); principal investigator (1979-2000); and was instrumental in conceptual development of INTSORMIL. Axtell's advice and

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counsel also were provided to other international research programs: AID representative at CIAT/AID board meeting at Cali, Colombia (1982); preparation of a draft project paper on "Bases of Plant Resistance to Insect Attacks," a U.S. AID research project for the International Center for Insect Physiology and Ecology (ICIPE), Kenya (1982); AID representative to the Asia Ag Officers Conference on Rainfed Agriculture (ICRISAT) (1983); advisor on the Joint Committee on Agricultural Research and Development and the Board for International Food and Agricultural Development (BIFAD) (1984-85); director for the Purdue/AID Sorghum Project (1972-94); U.S. scientific liaison officer to the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) at Hyderabad, India (1984-87); technical advisory panel member for the Southern Africa (SADCC) Sorghum/ Millet Improvement Program (1985-93); and member of the AID Academy for Educational Development, Communication for Technology Transfer in Agriculture (CTTA) to discuss the potential of Swaziland serving as a CTTA diffusion site (1985-87). John Axtell's presence and contributions to INTSORMIL and other international organizations were very important to the international community. One individual stated that John Axtell's death was "a global disaster for sorghum and millet's global family."

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HONORS AND AWARDS

1974 Special fellowship, National Institute of General Medical Sciences
1975 Sigma Xi Research Award, Sigma Xi
Certificate of Appreciation, U. S. Agency for International Development
1975 Alexander von Humboldt Award
1976 Crop Science Research Award, Crop Science Society of America
1977 Fellow, American Society of Agronomy
1982 Lynn Distinguished Professor, Purdue University
1984 International Award for Distinguished Service to Agriculture, Gamma Sigma Delta
1985 Fellow, Crop Science Society of America
1998 Purdue's Pride in International Programs. Purdue Profiles: John Axtell
INTSORMIL Team Award, Purdue University

Sources of information used to summarize the distinguished career of John D. Axtell included John D. Axtell, *Crop Science Society of America News,* February 2001, p. 33; "Memorial Resolution for John D. Axtell" by Gebisa Ejeta, March 9, 2001; and John D. Axtell's resume in the Department of Agronomy, Purdue University.

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Maxime Bôcher

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MAXIME BÔCHER

August 28, 1867–September 12, 1918

BY WILLIAM F. OSGOOD

MAXIME BÔCHER WAS BORN in Boston, August 28, 1867, and died at his home in Cambridge, September 12, 1918. His father, Ferdinand Bôcher, was the first professor of modern languages at the Massachusetts Institute of Technology. Shortly after Mr. Charles W. Eliot, at that time professor of analytical chemistry and metallurgy in the same institution, became President of Harvard University, Professor Bôcher was called to Cambridge (in 1872) and for three decades was one of the leading teachers in the faculty of Harvard College. He was an enthusiastic collector of books. His library, which was divided after his death, formed the nucleus of the library of the French Department and yielded, furthermore, a welcome accession to the library of the Cercle Français; but its most important part, the valuable Moliére and Montaigne collections, passed intact to the library of Harvard College. It was through the generosity of Mr. James Hazen Hyde, who bought the whole library, that such a disposition of the books became possible.

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As a college teacher Ferdinand Bôcher is remembered by many men for whom college life in their student days offered varied attractions, as one who helped them to see and enjoy the beauty of language and literature.

Maxime's mother was Caroline Little, of Boston. She was of Pilgrim ancestry, being a descendant of Thomas Little, who joined the Plymouth Colony in its early days and in 1633 married Anne Warren, the daughter of Richard Warren, who came in the Mayflower.

Thus Bôcher's boyhood was passed in a home in which much that is best in the spirit and thought of France was united with the traditions and intellectual life of New England. He attended various schools, both public and private, in Boston and Cambridge; but it was to the influence of his parents that the awakening of his interest in science was due.

He graduated at the Cambridge Latin School in 1883 and took the bachelor's degree at Harvard in 1888. Then followed three years of study at Göttingen, where he received the degree of doctor of philosophy in 1891, and at the same time the prize offered in mathematics by the philosophical faculty of the university. From 1891 till his death he was a member of the Department of Mathematics in Harvard University. He married Miss Marie Niemann, of Göttingen, in 1891. His wife and three children, Helen, Esther, and Frederick, survive him.

His college course was a broad one. Outside of his main field of mathematics and the neighboring field of physics he took a course in Latin and two courses in chemistry, and courses in philosophy under Professor Palmer, in zoology under Professor Mark, and in physical geography and meteorology under Professor Davis; and it is interesting to note that in his senior year, beside his work in mathematics, he elected Professor Norton's course in Roman and mediæval art, a course in music with Professor Paine, and an advanced

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course in geology with Professor Shaler and Professor Davis, and Professor (then Mr.) Wolff. In his senior year he also competed for a Bowdoin Prize, and the committee awarded him a second prize for an essay on "The meteorological labors of Dove, Redfield, and Espy." At graduation he received the bachelor's degree summa cum laude, with highest honors in mathematics, his thesis being "On three systems of parabolic coordinates." A travelling fellowship was granted him, and it was twice renewed.

Bôcher's education was not confined to the courses he took. He was a reader and a thinker, and he was interested in many of the general questions of the day. But generalities did not satisfy him; he demanded of himself that he know precisely the essential facts. His critical powers were early cultivated, and he was endowed with good judgment. In debate, he was able to marshal his facts with rapidity, to arrange them strategically, and to make his point with clearness. In rebuttal, he was an expert.

I recall an incident which occurred at a meeting of the M. P. Club[1] in the early nineties, and which shows the characteristics last mentioned. Professor Woods had given an interesting talk on surfaces which are applicable to one another, and had illustrated his subject with models from the Brill collection in the mathematical library of the Institute. One of the members of the Club was a physician, whose interest in mathematics had been kindled by Benjamin Peirce, and who, though not a profound mathematician, nevertheless delighted to read mathematics, much as our ancestors read their Horace. He asked a question which was based on his doubt whether parallel lines, in any logically necessary interpretation of the words, "meet at infinity." Now, there was also present a learned professor from another institution, and it pleased him to answer the doctor from a mighty height. But, in his answer, he was thinking only of projective

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geometry, and his arrogance made Bôcher indignant. "That is not true in the geometry of inversion," the latter replied. "That is not geometry," was the professor's scornful rejoinder. "It is what Klein calls geometry," came back quick as a flash. "Oh, Klein is not a geometer." This was the professor's last shot. In two brief statements of facts the youthful Bôcher had put his opponent into the position of asserting that the man who wrote the "Vergleichende Betrachtungen über neuere geometrische Forschungen" was not a geometer!

Above all, Bôcher was sincere. He like to argue and to defend a position; but when the game was over, it was the truth which had been brought out that pleased him most.

He distrusted popular conclusions, even when the public was a learned one. It was facts, not views, that he sought, and his own intellect was the final arbiter. The following incident is characteristic of his type of mind. When his last sickness was developing, he needed a physician, and the well-known doctors were away in the war. He made inquiries one day regarding a young practitioner of rising fame, with whom Professor Birkoff had recently had some experience. The latter said in closing, "I must add, however, that Dr. —— is pessimistic. He is given to taking a gloomy view of the condition of his patients." "I do not care whether he is pessimistic or not," was Bôcher's reply, "if the diagnosis is correct."

The later years of his life were not happy ones. Even as far back as the winter of 1913–1914 his strength was frequently inadequate for the daily needs. He never complained; in fact, he was unwilling to talk about himself even for a moment. But for one whose demands on himself were such as Bôcher's it must have been a severe trial not to achieve the full measure of results of which the mind was capable and for which it longed to work.

He was a Puritan, and with the virtues he had also the

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faults of the Puritan. There was no place in his world for human weakness, even though the individual had done his best. A reverence for human beings because of their struggles to attain higher things was lacking in his make-up; he respected only results. And so, to many a man who came into personal relations with him in his profession, he seemed cold and unsympathetic. What the stranger, however, too often failed to observe was that Bôcher applied the same stern standards to himself. Why should others expect to fare better?

In order to understand the mathematical work of Bôcher it is well to consider at the outset the state of the science as he found it. The nineteenth century was an era of intense mathematical activity, not in one land alone, but among all the peoples which were leaders in scientific thought. If it was not reserved for mathematicians to make formal discoveries coordinate in importance with those which formed the crown of the discoverers and early developers of the calculus, it is none the less true that mathematical imagination never played more freely, not only in geometry and algebra, but also in analysis and mathematical physics.

But mathematics was no longer in its infancy. In the great age just preceding the French revolution, a mathematician could know, at least in its essential parts, all that had been done in the science up to that time, just as, a century earlier, the man of learning was conversant not only with mathematics and physics, but also with the principal systems of philosophy. With the enormous expansion of the subject matter, or detailed theories, which grew up and flourished with amazing virility in an age characterized by its struggle for intellectual freedom, a point had been reached where it seemed as if mathematics was destined to disintegrate through the very volume of its scientific content.

It was at this time—the eve of the Franco-Prussian War—

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that two youths met in Berlin, who were to become leaders in mathematical thought—Felix Klein and Sophus Lie. True, these men were remote from each other in their specific mathematical interests, and their life work lay in different fields. Lie built up a consistent, harmonious theory which both yielded new results and brought old ones under a common point of view. With Klein it was not a question of developing a method for its own sake, or even of caring for method, except in so far as he was thus able to uncover the natural interrelations of parts of the science which hitherto had seemed foreign to each other.

A pupil of Clebsch and Plücker, Klein early became acquainted with the geometric advances that group themselves about the names of Monge and Poncelet, of Steiner and von Staudt. In analysis, the theory of functions, as developed by Cauchy and his followers, was already beginning to come into its own. Göttingen was filled with the traditions of Riemann, whose life touched fingers with that of Klein. In algebra, Galois's contributions were still new, and Hermite and Kronecker had, hardly more than a decade previously, solved the general equation of the fifth degree.

Klein's first great contribution toward unifying apparently unrelated disciplines was the Erlanger Programm of 1872 mentioned above, on a Comparative Consideration of Recent Advances in Geometry. It was here that he set forth projective geometry, not as an isolated science—geometry, par excellence—but rather as one (true, the most important) of a whole array of geometries, of which, in particular, the geometry of reciprocal radii, or inversion, is a member; for the basis of each of the geometries is the group of transformations which leave invariant certain configurations, and two geometries are essentially equivalent when their groups are isomorphic and their elements stand to each other in a one-to-one and continuous relation.

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It would have been easy for Klein at this stage to found a school on the basis of postulates. If the thought ever occurred to him, he rejected it both because the results to be obtained would lack important mathematical content and because he instinctively sought the specific interrelations of seemingly distinct branches of mathematics, in order that one might yield new theorems, or illumine old ones, in the other.

It was to an environment imbued with such traditions that Bôcher came, when he was matriculated at Göttingen in the fall of 1888. His previous training at Harvard had prepared him to enter at once on advanced work. In the last year of his college course, as has already been said, he had written a thesis on parabolic coordinates. Klein was beginning the continuation of his lectures of the potential function, and these were followed by his lectures on the partial differential equations of mathematical physics, and on the functions of Lamé. He also lectured at this time on non-Euclidean geometry.

It is seldom that a student is brought into such vital contact with the chief branches of mathematics as was the case with Bôcher. His thesis was on Developments of the Potential Function into Series, a subject which he shortly after worked out at greater length in a monograph. Though the leading ideas had been set forth by Klein in his lectures, nothing could be further from the truth than to think that Bôcher merely elaborated some details. The subject was an exceedingly broad one. It required for its treatment not so much a specific knowledge of the theory of the potential, although Bôcher was thoroughly equipped on that side; nor even familiarity with the geometry of inversion, of which he had made himself master; but rather, the power to carry through a piece of detailed analytic investigation with accuracy and skill, and with this work Klein occupied himself

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only in the most general way. Nor was it a question of the proof of convergence for the series obtained. Indeed, these proofs have not as yet been given, though recent advances have been made by Hilbert with the aid of integral equations. The importance of the dissertation in its influence on Bôcher lay largely in the fact that it stimulated his interest in mathematical physics, in pure geometry, in algebra, and in applied analysis. More precisely, beside the general geometrical ideas and theories above mentioned, the specific study of the Dupin cyclides and their generalization by Laguerre, Moutard, and Darboux was involved. Through the method of elementary divisors, he was led to examine in detail a chapter in pure algebra, together with its application in more than a single field in geometry. From the formal solution of the first boundary value problem for Laplace's equation by means of series to the study of boundary value problems for the partial differential equations of physics of other than the elliptic type and the treatment of these problems by the more recently developed methods of integral equations, was a natural course. Throughout all his work, the total linear homogeneous differential equations of the second order were a constant source of further investigations, both by himself and by his pupils, and his last great published work, the Paris lectures, is in this field.

In the fall of 1891 Bôcher began his career as university teacher, being appointed to an instructorship in the department of mathematics of Harvard University. It is the practice of that department to give to each of its members an elementary, an intermediate, and an advanced course. Bôcher's teaching, both elementary and advanced, was successful from the beginning. He did not have to "learn to teach"; teaching came to him naturally. Doubtless he was aided in this direction by the example of his father and the family traditions, for his mother had also been a teacher;

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nor were his parents the only ones of the immediate family who had been engaged in that profession. The standards of clearness, both in thought and expression, which characterize French men of letters and science, Bôcher made his own, not by a conscious effort, but through an inner driving force which made it a part of his very nature to find suitable expression for his ideas. "He never tried to be clear," Major Julian Coolidge wrote me this fall, "because his constitution was such that he did not know how to express his thoughts in any but the clearest form." I would not, however, be understood as saying that he achieved his success as a teacher without effort. He gave careful thought to the preparation of all his instructions, both as regards the choice of material and the presentation; but he was able to do this without serious loss of time or energy.

His intermediate course in the first year of his teaching was on modern geometry. Professor Byerly had already developed this course to a point which gave it an important place in the undergraduate instruction. The outlook on geometry which Bôcher had acquired under Klein enabled him to make the course still more effective as an introduction to the ideas and methods of the higher geometry of the present day. He gave this course repeatedly (about every other year) during the whole period of his service, and he was engaged in the preparation of his lectures for publication at the time of his last illness.[2]

In the minds of some readers the word *lecture* in connection with a sophomore course may cause doubts as to the efficiency of the instruction. The objection is raised that sophomores cannot take notes and get only vague outlines of ideas which they cannot develop further. It must be remembered, however, that this course is a free elective, and that it is chosen by men who have interest in mathematics and capacity for its pursuit. Moreover, frequent

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exercises are assigned (as a rule, daily) which range all the way from simple tests on the essentials to problems whose solution is possible only for students who really dominate the methods. These problems are corrected and returned to the student.

So much by the way of apology. Let me now say, with all aggressiveness, that it was largely to the lecture method that both Professor Bôcher and I owed the awakening of our interest in mathematics when we were undergraduates in Harvard College. The instruction thus imparted stimulated thought, and the exercises assigned developed power—the power to obtain new results, even in the undergraduate stage. It was with exultation that we followed courses given by this method, in which our mathematical powers grew before our very eyes. In saying this, I am also stating Bôcher's views, for he repeatedly expressed himself on this subject in conversation.

Bôcher's advanced course in the first year of his professional life took the form of a seminary, the subject being curvilinear coordinates and functions defined by differential equations. A part of the instruction consisted of formal lectures on the latter topic, and he thus began, even at that early date, to treat topics in a field of analysis in which he was to become eminent.

In the eighties, a number of American students of mathematics from various colleges went abroad, chiefly to Germany for further instruction and guidance in mathematics. When they returned, some of them became university teachers and strove, so far as in them lay, to give to their students advantages like those to be found in Europe at a mathematical center. Bôcher was one of this latter group. With rare discernment for problems of importance, on which advanced students might work with a reasonable prospect of success, he gave himself unstintingly to the task of helping

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such students to carry through pieces of investigation and to put their results into good form. He did not foster work on the part of his students by artificial means—by high praise or an appeal to ambition. He felt that the student must be possessed of idealism and must, of his own nature, find satisfaction in scientific activity; otherwise, the writing of a doctor's thesis would represent only a forced growth. At times, it seemed to the beginner in research that he was unappreciative. But the student who had capacity for mathematical investigation and loved the science found an open ear and a ready response when he came with a contribution of real scientific merit, be that contribution in itself large or small.

The awakening in the science of mathematics in this country was followed at once by the springing up of the New York Mathematical Society, which shortly after became the American Mathematical Society. Of the latter Bôcher early became a member, and he took a keen interest in its affairs, contributing to its *Bulletin* and participating in all its activities. He and Professor Pierpont were the speakers at the first Colloquium given by its members—at Buffalo, in 1896. When the establishment by the Society of a journal devoted to research was under discussion, it was through his insight that a way out of the difficulties which seemed insurmountable was found. Among the older members of the Society were those who saw in the establishment of such a publication an unfriendly act toward the *American Journal of Mathematics*. At a meeting of about a dozen mathematicians, held in New York in the fall of 1898 to discuss the question, this view was represented by the late Dr. McClintock. Bôcher asked him if he saw any objection to the Society's publishing its *Transactions*. To the surprise of all, there came a prompt answer in the negative. The difficulty was overcome. The Society might not establish the "Journal of the

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American Mathematical Society," but it might publish the "Transactions of the American Mathematical Society"!

Producers of mathematical research are, as a rule, not facile in their expression. When one has been engaged in the protracted study of a problem, the early difficulties and the underlying ideas become obscured through familiarity with the facts, and the writer produces a paper hard to read. The early editors of the *Transactions* labored, and not without success, to impress on the contributors the importance of making easily accessible to the reader the main results and methods of the paper, and of showing the relation of the investigation to previous work. It was here that Bôcher's power as a critic was of great service. But a critic, to be helpful in such work, must be constructive. How admirably Bôcher was adapted for this undertaking, could not be shown more strikingly than by the opening paragraphs of his Dissertation, which are a model of what an introduction to a scientific paper of wide scope should be. He was not a member of the first editorial boards, for at that time the *Annals of Mathematics* had just been taken over by Harvard University, and he was doing similar work for that journal. But from the start he was in close touch with the editors of the *Transactions*, and his views on general questions and specific papers were helpful to them. Later, he served for two terms (with the exception of one year, in which he was absent from the country) on the editorial board.

He was president of the Society from 1908 to 1910. For his presidential address he took as the subject: "The published and unpublished works of Charles Sturm on algebraic and differential equations." He delivered the address in Chicago. The meeting will live in the memories of all who were present, especially in those of the eastern colleagues, as a particularly delightful occasion.

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Bôcher's judgment of men, too, was sound, and those who had occasion to discuss nominations or appointments with him felt that a decision which had his approval could be trusted.

The breadth of Bôcher's knowledge of mathematics was accompanied by a true sense of perspective. His estimate of the importance of an investigation was extraordinarily sound. In his own work, this quality of mind was both a help and a hindrance. It helped him to choose well the problems which he and his students were to study. It can fairly be said that Bôcher never occupied himself with an unimportant problem. On the other hand, the enthusiasm just of doing things in mathematics—the joy of living, so to speak—gives to one's mental work a momentum which carries it over the obstacles of disappointment and discouragement, when one effort and another fail to yield results, and along with much which is valueless for others there come, now and then, contributions worthy of a lasting place in the science. I will not say that Bôcher was without such enthusiasm; but he did not show it in his intercourse with others. His nature was reserved. He would not talk on personal matters relating to himself and this disinclination extended even to his scientific work.

He was, however, glad to discuss the work of others with them. He was quick to grasp the central idea and often could express it more clearly than its author. The early meetings of the Society were prized by those who attended them less for the formal papers presented than for the informal gatherings in the evening or about the breakfast table. It was here that the real mathematical discussions took place, and who of those who had the rare good fortune to be associated with that little group will ever forget what Bôcher was to us in those days? His special field was analysis; but so broad were his sympathies and his learning that he usually took a leading part in the discussions. His criticism

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was always helpful, often constructive, and freely given in the finest spirit.

We have mentioned the Presidential Address. At the St. Louis Congress, in 1904, he delivered an address on "The fundamental conceptions and methods of mathematics." He gave a lecture at the Fifth International Congress of Mathematicians, at Cambridge, England, in 1912, his subject being: "Boundary problems in one dimension." In 1913-14 he was exchange professor at Paris. His opening lecture was of a general nature and was entitled: "Charles Sturm et les mathématiques modernes."

It was not until late that Bôcher occupied himself with the writing of text-books. He had published some expository articles, chief among which were the pamphlet on "Regular points of linear differential equations of the second order," Harvard University Press, 1896; an article on "The theory of linear dependence," *Annals of Mathematics,* (2) 2 (1901) and an "Introduction to the theory of Fourier's series," ibid., (2) 7 (1906); three years later he wrote Tract 10 of the Cambridge (England) series, entitled: "An introduction to the study of integral equations.

The Algebra appeared in 1907. Hitherto, books on algebra in the English language had been of the Todhunter type, or they had followed the lead of Salmon, through whom "Higher Algebra" came to mean specifically the study of the algebraic invariants of a linear transformation. What the mathematician needed to know of linear dependence and the theory of linear equations, of polynomials (factorization, resultants, and discriminants), the reduction of one or of two quadratic forms to normal type (including, perhaps, the rudiments of elementary divisors) he had to pick up as best he could. In no one place were they treated systematically, and most of the treatments were inadequate for the present day needs of the science.

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Bôcher filled this gap in a thoroughly satisfactory manner. The Algebra was received with appreciation, both in this country and abroad, and at the suggestion of Professor Study a German translation was prepared. How thoroughly the work had been done originally is seen from the fact that practically no revision was needed.

The Trigonometry (written jointly with Mr. Gaylord) and the Analytic Geometry are so widely and intimately known as to require no detailed comment. These books present elementary subjects in a form accessible for elementary students, and treat them with a degree of accuracy, elegance, and perspective seldom attained by writers of text-books.

I have spoken of Klein's efforts to *unify* mathematics. Bôcher's aim may be described by saying that he strove to *clarify* mathematics. To illustrate by a single, but important example, let me consider the theory of functions of a complex variable. In the early nineties there were two distinct schools, and neither sought to aid or to learn from the other. Cauchy based his theory on the calculus of residues, obtaining Taylor's theorem as a corollary. With Weierstrass and Méray power series formed the foundation. The integral was more pliable and better adapted to the needs of the subject. But the questions which the critics had raised regarding limits, and in particular the reversal of the order in a double limit, had not been settled in a satisfactory manner for integrals, and even for series they were ignored by the writers of the Cauchy school. On the other hand, Weierstrass restricted his infinite processes to differentiation and power series. His treatment was rigorous, but clumsy, and the whole theory took on a formidable aspect.

Riemann's methods were thought of less as forming an independent theory than as yielding an important mode of treatment for certain classes of functions; e.g., the algebraic functions and their integrals, and the functions defined by

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linear differential equations or their resolvents; notably, the *P*-function and the automorphic functions.

In 1893-94 Bôcher gave for the first time in his career the introductory course on the theory of functions of a complex variable, and in the same year he repeated his course on functions defined by differential equations, laying stress on the complex theory. The subjective effect is obvious. For him, it could not be a question of developing the general theory of functions as an end in itself. He was interested in the theory as a tool—as a means of investigating, for example, the functions defined by differential equations. But he was interested in improving the tool, in developing better machinery than had come down to us. He cared nothing for the schools. He sought the simplest method for solving each problem.

Of course, he was rigorous. But for him, rigor was not a strait-jacket. For him, rigor was not something superimposed on a proof, already satisfactory to a normal mind, by a certain cult of mathematicians. If a proof was not rigorous, it was not *clear*—it had not succeeded in analyzing completely the situation. Not that, with him, there was no place for intuition in mathematics. Quite the reverse. He recognized clearly that rigor is relative, depending on the domain of conceptions and the logical maturity of the student, and he was a master of diagnosis in determining what his students required or could receive, and what their minds must reject.

His contributions of the kind we have been considering were not confined to improving proofs already complete. He discovered gaps and filled them; as in the case of the theorem that a function which is harmonic in the neighborhood of a point, that point excepted, and becomes infinite there, must be of the form (when $n = 2$):

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$$u = k \log r + \omega,$$

where ω is the harmonic at the point, also.

How extensive and how useful this work of Bôcher's was will become evident to any one who will turn to the writer's Funktionentheorie, volume I, and look up the references under Bôcher's name in the index. And what he did in this field, he did in others. His Algebra, for example, affords numberless instances in point.

In the early years of our professional lives we were in constant intercourse over such matters. Each of us was seeking to clarify and simplify his subject. Neither of us regarded the theory of functions of a real or of a complex variable as an end in itself, for each had his own ulterior uses for the theory— Bôcher, his differential equations, both complex and real. In fact, for each of us the theory of functions was *applied mathematics,* and in presenting its subject matter and its methods to our students, our aim was to show them great problems of analysis, of geometry, and of mathematical physics which can be solved by the aid of that theory.

Bôcher was quick to grasp the large ideas of the mathematics that unfolded itself before our eyes in those early years. His attitude toward mathematics helped me to have the courage of my convictions. The Funktionentheorie is largely Bôcher's work, less through the specific contributions cited on its pages than through the influence he had exerted prior to 1897—long before a line of the book had been written. We worked together, not as collaborators, but as those who hold the same ideals and try to attain them by the same methods. It was constructive work, and in such Bôcher was ever eager to engage.

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NOTES

1. This club was formed in the eighties for the purpose of bringing the members of the departments of mathematics and physics at the Massachusetts Institute of Technology and Harvard into closer relations.

2. Since this course meant so much to Professor Bôcher, the reader will be interested in his description of it in the Departmental Pamphlet:

INTRODUCTION TO MODERN GEOMETRY AND MODERN ALGEBRA

The subjects considered in this course are:

(a) Affine Transformations; the use of Imaginaries in Geometry; Abridged Notation; Homogeneous Coordinates; Intersection and Contact of Conics; Envelopes; Reciprocal Polars; The Parametric Representation of Straight Lines and Conics; Cross-Ratio; Project and Collineation; Inversion

(b) Complex Quantities; The Elements of the Theory of Equations; Determinants; The fundamental Conceptions in the Theory of Invariants.

The portion of the course devoted to the geometrical subjects (a) will be two or three times as extensive as that devoted to the algebraical subjects (b), and the relations between these two parts of the course will be emphasized.

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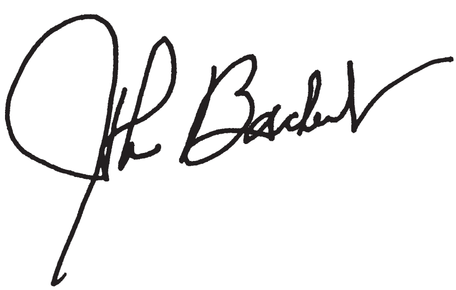

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JOHN ROBERT BORCHERT

October 24, 1918–March 30, 2001

BY JOHN S. ADAMS AND VERNON W. RUTTAN

JOHN BORCHERT WAS A practical scholar of exceptional intellect and charismatic personality who made original and important contributions to physical geography, especially climatology, regional economic analysis of the United States, U.S. metropolitan evolution, urban and regional planning, geographic information science, and geographic education. A Regents' Professor of Geography at the University of Minnesota, he inspired three generations of students at all levels to get out of their armchairs and into the field, to explore and get to know the territory, to ask questions, produce effective map series, generalize from them to infer and advance original interpretations of what is happening on the land, and to participate in public policy debate and planning activity in local, regional, national, and global arenas.

AN EARLY OBSERVER OF AMERICAN SETTLEMENT

John was born in Chicago, the son of Ernest J. Borchert and Maude (Gorndt) Borchert, and grew up in Crown Point, Indiana. As a boy in the 1920s and 1930s he lived on the edge of one of the steepest physical and cultural geographical gradients in the world at that time. On one side of the gradient stood his hometown, a typical quiet Corn Belt county

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seat of 2,500, mostly of German and other northwest European origins. Yet just a few miles north was the city of Gary, laid out less than a decade earlier by U.S. Steel Corporation, and the gates of the largest steel mills in the world with 20,000 Afro-American residents, most of the rest from eastern and southern Europe, plus a small contingent from Asia. In his later years John told one of us (J.A.) that regular 59-minute train rides from his hometown to downtown Chicago took him through Gary with its smoky chaotic array of recent modest residential neighborhoods, past refineries and factories, through vast rail yards sprawling westward across the Calumet flats for 15 miles into Chicago, and then monumental office towers and hotels rising above the commotion and soot of the Chicago Loop.[1] Drawing on his prodigious memory for telling detail, he recalled peering through grime-covered day-coach windows at the rapid-fire and bewildering transitions from the northern Indiana countryside, through an industrial complex that matched the Ruhr and the Pittsburgh-Cleveland axis, past rail yards teeming with box cars displaying railroad system names that read like a gazetteer of North America, to the heart of one of the largest cities on Earth. This was his first important geography lesson, and the more he observed it and mulled it over the more it piqued his curiosity and shaped his thinking.

In the 1920s and 1930s as John was coming of age, academic geography like many social sciences was struggling in the face of received orthodoxies to grasp and define the nature of the social, economic, political, and urban-industrial changes that had spread over the industrialized world during the previous century. Geography had a long history as an Earth science in universities as a subset of nineteenth century natural science, or as a curricular element in schools of commerce borrowing from the universities of Europe's colonial powers. What was slow to evolve in the field was an

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understanding of how urban and rural settlement systems were transforming in response to socioeconomic change, and how human interaction with natural environments was producing profound changes both in society and in the environmental systems they were exploiting in new and different ways. John Borchert grew up during that era, and using his powers of observation, imagination, and intellect he focused on these changes; without realizing it until later in life he provided himself with an experiential foundation for his remarkable career as one of the world's leading geographers during the last half of the twentieth century.

Originally John planned to be a journalist, so it seemed obvious that the best way to start was to get a job with the local weekly paper and work up. But, through the local Methodist minister he met a Chicago Tribune Company executive who firmly advised John to go to college. The following fall John was enrolled at DePauw in Greencastle, Indiana. By chance John took a year of geology as a freshman, and after getting oriented he decided on the geology major for two reasons. The study of historical geology—especially the Ice Age and recent—was his most liberating intellectual experience in college up to that time, and the study of economic geology might lead to employment. The lone geology professor at DePauw offered one course called geography, which satisfied a state requirement for education students but left John unimpressed. However, the geologist, Professor "Rock" Smith, saw the future of such fields as statistics, geophysics, and aerial photography in geology research and applications, and pushed his majors through a well-rounded introduction to geology, the basic sciences, and mathematics—a suite of rigorous courses unusual for the time. Accompanying fieldwork included informal observation of not only the physiographic but also the cultural landscapes throughout Indiana.

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Following receipt of his A.B. degree in 1941 but before proceeding to graduate school in geology at the University of Illinois, John worked a short while in geophysical exploration for oil on the northern Great Plains where he met his future wife, Jane Anne Willson, in Bismarck, North Dakota. One semester of graduate work—just long enough to discover he enjoyed teaching—was punctuated by Pearl Harbor and U.S. entry into World War II. It was difficult to concentrate on graduate work, so John took a position as a topographer with the U. S. Geological Survey, with an assignment on the Tensaw quadrangle, on the delta of the Alabama and Tombigbee rivers at the northeast edge of Mobile Bay. Topographic mapping of coastal areas had been accelerated in response to fears of Nazi and Japanese attacks.

Work on the Tensaw quadrangle ended in early 1942 when John shifted from geology and geophysics to graduate work in meteorology at MIT, where John and Jane were married on June 10th. The meteorology course was part of the Army's aviation cadet program and led to a commission in the Army Air Force. In John's recollection the most exciting part of the program was working with synoptic weather maps. Foreshadowing later events, one course dealt with world regional climatology and introduced the Köppen classification of climates, which unknown to John was a major focus for research and writing in U.S. geography at that time.

Following commissioning John went to England to work as an operational weather forecaster at headquarters of the B-24 "Liberator" bomber division, briefing flight crews and general officers and de-briefing crews after missions were completed. He found the drawing and analyzing of weather maps and preparing weather forecasts to be powerful learning experiences. He worked with a large array of numerical data to produce twice-daily isopleth maps to locate highs, lows, gradients, air flows, and weather conditions generated

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by those flows as they diverged, converged, and crossed relief features and water bodies. Then he applied a mixture of rigorous procedures and intuition to extrapolate the patterns through time—what amounted to four-dimensional cartographic analysis, which he later came to believe was the heart of the geographic method, and of which he became one of modern geography's outstanding practitioners.

The end of the war triggered a chain of chance events that led John quickly to the field of academic geography. At an army base library in East Anglia he happened upon a copy of *Elements of Geography* by Vernor C. Finch and Glenn T. Trewartha, geography professors at the University of Wisconsin, a top U.S. geography department in the 1940s. Much of the text was devoted to efforts to relate Earth science material to the human use of the land, with a final section addressing in a minimal way the morphology of human settlement. The effort was halting but the idea was intriguing, so he decided to look into it further.

He visited Madison en route back to Indiana following his discharge from the army at Camp McCoy, Wisconsin, located the Geography Department, and found Finch's office. Finch received him graciously; they talked for some time, then Finch looked at his watch announcing that he had to give a lecture to the introductory physical geography class in a few minutes. He paused and said to John, "The lecture today deals with marine West Coast climates in the Köppen system. You're certainly familiar with that climate and what it meant for our fliers in northwestern Europe. Would you like to give the lecture?" "Recklessly," John later recalled, "I accepted the invitation, illustrating the lecture with blackboard sketches describing weather forecasting episodes from the war." The lecture went well, and the 200 students applauded. A week later he and his family were living in

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Madison. John Borchert had found the field he was looking for, and was hooked for good.

DISCOVERING GEOGRAPHY

Following his lecture John remained in the department for lunch with the half-dozen graduate students, including Alan Rodgers and Wilbur Zelinsky (both of whom went on to distinguished careers at Penn State). By chance a guest speaker after lunch was geographer Wellington Jones from the University of Chicago reporting on his research in the Punjab. Jones's presentation was an eye-opener for someone at John's level of preparation. The maps were simply work sheets, portraying Indian census data on crops at successive time intervals. Data were overlaid with isopleths describing areas of high and low production, intervening gradients, and changes in patterns from one time to another. Jones laid out his explanations for the patterns and changes based on archival work, interviews in the field, and comparisons with other maps. He also examined his subject at several geographical scales. Behind him hung large wall maps on which he placed his study area within South Asia and the world, and at the opposite end of the scale he showed photographs of landscapes that were generalized on his maps. He discussed questions that puzzled him, and he speculated about further questions the maps suggested.

Years later John recalled that this approach to geographical analysis was analogous in many ways to what his weather forecasting team had done with weather observations—isopleth analysis, with description and classification of patterns; description at different scales from global to local; interpretation using both theories and simple, direct observations; then discussion of results with others who were interested. Here once more was a demonstration to John of what he would later come to regard as the core of the

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geographic method. Jones's data were for minor civil divisions rather than specific weather stations. Jones was sampling an extensive surface using small areas rather than points. His time intervals were in years rather than hours. But there was plenty of opportunity to observe and map the change as it was taking place. In later years John recalled with one of us (J.A.) that in those early days of graduate training he had no idea how far we would still be from understanding the cognitive aspects of all this when he retired 50 years later. Nevertheless, he was sensing the value of the map as a powerful intellectual tool, and would later conclude that it would be hard to imagine a more efficient way to understand the locations and interactions among a great variety of day-to-day activities while at the same time contributing to the quest for understanding the role of humanity on the Earth. He admitted that with the benefit of hindsight it was probably easy to make too much of that brief encounter with Wellington Jones, but he remained confident in later years that the seeds were planted.

It set in motion his thinking about the discipline and practice of geography. The inspiration of it and his infectious enthusiasm for it never waned. It led to many rewarding discussions with fellow graduate students, including Rodgers and Zelinsky as well as E. Cotton Mather (later a colleague at Minnesota), John E. Brush (who taught many years at Rutgers), and John W. Alexander (later a faculty member at Wisconsin). Richard Hartshorne (one of John's teachers at Madison) added historical depth to John's understanding of the history of the field of geography, background that he had missed in his early schooling. Arthur H. Robinson (America's leading cartographer, also on the Wisconsin geography faculty) instilled insight into discussions of scale, generalization, and measurement. Glenn T. Trewartha (climatologist and expert on Japan) contributed his penchant

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for orderly and unequivocal description. Reid A. Bryson (fellow graduate student, later geography professor at Wisconsin) shared ideas about flows, gradients, boundary zones, and interactions between Earth and human settlement that ranged far beyond his central interest in dynamic climatology. Thinking begun during that lunch hour with Wellington Jones in 1945 carried through to later discussions with Minnesota colleagues, especially Jan O. M. Broek, John C. Weaver, Phillip W. Porter, Joseph E. Schwartzberg, Fred E. Lukermann, and a procession of graduate students including one of us (J.A.).

Maps in time series to analyze geographic processes became a hallmark of most of John's research from the time he became a geographer. His first major publication, in 1949, was his doctoral dissertation in which he compared patterns of central North American atmospheric circulation, rainfall, and temperature in different dry seasons through a series of decades. Later, two studies of municipal water supplies of American cities compared patterns of water use with available supplies in wet and dry periods.

His celebrated 1967 study "American Metropolitan Evolution" depended on maps of the locations of the country's cities, using comparable size classes at successive dates in the evolution of transportation and industrial technologies. Comparison of this paper with the grassland study illustrates his continued focus on new methods for mapping geographic processes, notwithstanding a creative shift of application from natural resources to human settlements.

A subsequent paper on "Major Control Points in American Economic Geography" dealt with one component of metropolitan evolution by mapping a half-century of change in the location of headquarters of large business organizations. The maps reflected the importance of entrepreneurship, instability, inertia, and drive for security, as well as the impact

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of local cultures. Another follow-up study in 1983 on "Instability in American Metropolitan Evolution" described a century of increasing variability in local urban growth rates that accompanied ever-greater speed and capacity of intermetropolitan transportation and communication.

His prize-winning 1987 book, *America's Northern Heartland*, possibly his magnum opus, was based on maps comparing the settlement patterns of the upper Midwest at successive times—at the beginning of railroading, the beginning of the auto-air age, and the beginning of the jet-satellite-fiber optic era. The study documented and interpreted changes in the way the region functioned. It also highlighted persistent features of the culture and circulation network of a busy part of the country, whose winters, most Americans think, make it basically uninhabitable.

Later John had an opportunity to reflect on metropolitan system change after the 1960s in a chapter on "Futures of American Cities" in the book *Our Changing Cities* conceived and edited by geography colleague John Fraser Hart on the occasion of John's retirement. In that paper John argued that we had been in a new epoch since the 1970s, and he speculated on the settlement features that would be hallmarks of the resulting new metropolitan "age rings."

He commented later that he could not escape from impressions developed over the previous three decades—the importance of evolving and pervasive technologies; unique local sites and histories; entrepreneurship; and increasing instability, complexity, and fragmentation, adding that an outpouring of atlases and interpretation would be more essential than ever as residents of cities sought to understand their options and effects of their actions. He had in mind two converging trends: society's growing need for geographic analysis and forecasting and the potential power of geographic information systems.

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Both trends had been foreshadowed by a major study of the influence of highway improvements on land development in the Twin Cities area, carried out in collaboration with Philip M. Raup, a University of Minnesota agricultural and land economist, and their students in the late 1950s. This work helped establish his credentials as one of the nation's leading urban geographers. One product of that study was "The Twin Cities Urbanized Area: Past, Present, Future," published in 1961, which brilliantly illustrated a remarkably precise method for producing a geographical forecast of the expansion of suburban land development around a metropolitan area. Although that paper also rested on a time series of maps, there were added features. For one thing, the goal of the study was to map a probable future geographic pattern of land subdivision in the metropolitan area. That demanded an historical series of data more consistent and detailed than the census. Computerized land records were still well in the future, so he had to devise a measure that could be obtained readily from both old and recent maps and would be consistent through time. From a large sample of mile-square sections in the land survey his team determined that a count of public-street and road intersections per square mile provided a virtually perfect indicator of the emerging density of platted building lots and street mileage, that is, a physical descriptor of the cultural landscape.

The resulting maps provided an exceptional picture of the spatial growth of the Minneapolis-St. Paul area from 1900 to the height of the post-World War II building boom in 1956, plus an extension of the growth picture to 1980 with a forecasted map that accommodated the number of new persons in accepted gross population forecasts. The map also assigned all of the projected new people to places that developed logically from past decisions, terrain, and

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accessibility. The map showed unprecedented geographical detail, and a quarter century later it turned out to be about 80 percent accurate. Meanwhile it helped to plan major expansions of highways, parks, utilities, and shopping facilities, schools, and subdivision locations.

What was important and innovative about John Borchert's geographical scholarship during the period from the late 1950s to the mid-1960s was his meticulous use of quantitative data and replicable technique to portray on a series of maps the evolution of the geographical structure of a modern industrial metropolis. No one in or outside geography had done this before. Urban geography was a relatively new direction in geographical scholarship in Europe and the United States in the 1950s, and research frontiers of modern quantitatively oriented urban geography were being extended.[2] One research thrust involved cross-sectional investigations of national and regional systems of cities involving empirical testing of central place theory. A parallel thrust focused on the emergence of national and regional systems of cities, and the growth and spread of individual metropolitan areas within such systems. John was an early leader contributing to both, in contrast to much earlier geographical work on individual metropolitan areas, which often was impressionistic or idiosyncratic in its execution, and lacked a theoretical basis and replicable research procedures.

Within this scholarly milieu John's 1961 Twin Cities study went beyond the notion of a time series of synoptic maps unfolding from past to present and attempted systematically to extend the series into the future. It also set an example locally for the use of fine grids and quantified descriptors of the landscape, anticipating computerized geographic information systems, in the development of which he was an important pioneer. In that respect it was part of the movement spearheaded by the area transportation studies stem-

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ming from the federal Interstate highway program in the late 1950s. It established the direction for two subsequent large-scale research projects that he directed: the Minnesota Lake Shore Development Study and the Minnesota Statewide Land Use Management Study—affectionately known to students and state legislators in the late 1960s as the LSD and SLUM studies.

GEOGRAPHY IN EDUCATION AND PUBLIC POLICY

John Borchert joined the University of Minnesota geography faculty in 1949 upon completing his Ph.D. at the University of Wisconsin. Several of his former fellow graduate students (Zelinsky, Rodgers, Mather) told one of us (J.A.) that John was recognized by most of them as the best and the brightest of the postwar geography cohort at Madison, so it was not surprising that John was offered an instructorship in the Wisconsin department. He decided instead to move west to the Twin Cities and join the small but prominent geography department and its new chair, Jan O. M. Broek, who arrived in 1948. By the early 1950s the trio of Broek, John C. Weaver (earlier at the American Geographical Society; later University of Wisconsin president), and Borchert, supported by a number of graduate assistants and instructors, mounted a balanced and innovative program of courses with burgeoning enrollments many times their prewar numbers. In those days "Big Ten" universities, along with the Ivy League, the University of California, and the University of Chicago, accounted for almost all of the most prominent comprehensive research universities in the country, with disciplinary leadership in most fields centered in these 20 or so schools. Within this academic setting a top graduate from a top graduate program, bristling with energy and creativity, was about to make his mark in scholarship, teaching, and outreach to government at all levels. John was already rather

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well known, and the work he did gained quick acclaim for originality and its emphasis on U.S. urban development and science resource policy, topics that in all areas of the social and physical sciences were seriously underdeveloped at the time.

In the preface to *Minnesota's Changing Geography* John asserted that the book's maps and narrative "reveal one of the most exciting facts which the human mind can discover—the fact that the varied landscapes all around us are parts of an orderly spatial pattern. That spatial pattern is the focus of the study of geography. And it is a fascinating, ever-changing composite expression of the combined works of men and nature." He also claimed, "Organized knowledge of the present is essential to give relevance to the historical past. Knowledge of the pattern of land and settlement provides the concrete framework upon which to build more abstract knowledge of human society. Knowledge of today's changing patterns provides the foundations from which plans for tomorrow must grow." In subsequent decades of use of the book by hundreds of teachers, and in the face of frequent re-statements of those convictions in classes and workshops, John recalled, no one ever challenged them. So he remained convinced that if those convictions were true, little doubt existed about the importance of geography in liberal education, formal and informal, at every level.

Like most geography departments in the 1950s the University of Minnesota had several lower division courses that provided an opportunity to introduce large numbers of students to the field and the discipline. When he began teaching at Minnesota in 1949, John inherited one of those: a long-established though poorly attended course on the geography of Minnesota. He assumed somewhat naively that students would enter the course with the shared attitude that they already knew the territory because they lived "there."

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Hence they would be expecting an unrewarding but easy three credits. He wanted to demonstrate to them that they could gain new insights about their own territory, or any other, by studying it as geographers; to show that although the place was familiar, the discipline was new to them and that as a result they not only would enlarge their substantive knowledge and understanding but also learn useful skills and concepts. He decided to organize the course content around major problems of public policy, selecting issues that not only had a major geographic dimension but also were important and persistent. The procedure was to state each problem in general terms, sort out the major dependent variables, study their geographic distribution, and ask what were the principal independent variables that accounted for the distribution. Students then compared the resulting series of maps, attempted to explain dimensions of the problem and related issues, and showed which variables would have to be changed in which ways to resolve them.

As one of us, who was a teaching assistant for John at the time (J.A.), observed the problems themselves were not peculiarly geographical, nor were the answers, but the analytical approach was. It used the vocabulary of regional patterns, place knowledge, generic terms of map legends, and concepts—location, scale, circulation, nodality—which are hallmarks of geography. It showed that geography is a way to clarify an issue, analyze a problem, identify and evaluate issues surrounding the problem, and propose solutions. John selected five broad, interrelated issues vital in Minnesota at that time, and likely to be around for some years: the farm problem, promoting industrial growth, metropolitan organization, future of small towns, and outlook for the depressed northeastern Minnesota Iron Range.

Rather than eliminate a need for traditional material, this course framework demanded more rigorous descrip-

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tion of location and form of such features as moraines and summer drought; and it gave to their understanding an obvious urgency that was evident to the students. It also demanded new maps of cultural and economic features that had been unneeded in the traditional approach, and had never been prepared because the questions had never been asked.

Students helped do the research. The material turned out to be so timely and informative that the instruction soon spread far beyond the classroom to political podiums, panels, and editorial pages, leaving no doubt that it was geographical analysis. People had to discuss the ideas from maps, comparing and analyzing patterns, locations, and spatial trends. They had to know what was where, and they concluded with place-specific statements about issues. Reflecting on those days devoted to developing and teaching a novel and policy-oriented course on the "Geography of Minnesota," John commented that it was not stretching history to say that his scholarly direction for much of the following five decades of work flowed from that experience.

The need for material for the Minnesota course motivated the first atlas of the state of Minnesota, which he produced in the early 1950s with Neil Salisbury, a senior undergraduate major. The first edition emphasized the state's agricultural geography and helped John cultivate a lasting relationship with the University of Minnesota's Institute of Agriculture. That relationship in turn led to funded research in the late 1950s with faculty and graduate students from geography and applied economics investigating freeway impacts on land use and land value. The geographical studies used a time series of maps to differentiate freeway influences from independent, long-term trends in both rural and urban settings and thereby showed the complexity of the changes that were assumed to result simply from free-

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way building. Meanwhile, later issues of the Minnesota atlas along with other policy-oriented research projects inspired an extended family of graduate students: Thomas J. Baerwald (National Science Foundation), John Wolter (Library of Congress), Robert W. Marx and Jacob Silver (U.S. Census Bureau), Robert C. Lucas (U.S. Forest Service), Rodney A. Erickson, William J. Craig, Ronald F. Abler, William Casey, and others who went on to positions of leadership in local, state, and national public agencies and universities.

Development of material for the metropolitan unit in the Minnesota course led eventually to "The Twin Cities Urbanized Area: Past, Present, Future." Work with various planning organizations led to the opportunity for John to organize and lead the urban research component of the Ford Foundation-financed Upper Midwest Economic Study (1959-65). This major regional development study, a joint undertaking of the Upper Midwest Research and Development Council (a Ninth Federal Reserve District banking and business group) and the University of Minnesota, was inspired in part by the University's Economics Department chair, Walter W. Heller, and directed by James M. Henderson and Anne O. Krueger. John's research team focused on the changing geography of towns and cities across the upper Midwest through ingenious applications of central place theory. The ostensible goal was to encourage more urban planning in the changing economy. But, the studies produced a much deeper understanding of the irreversible geographic trends that the postwar automobile era had visited on every part of the region's settlement system.

Meanwhile, the visibility of the atlas and industry studies led to an opportunity to work with Minnesota state legislators on a new program responding to the federal Outdoor Recreational Resources Act. In Minnesota's natural resource setting attention went directly to lakes and forests—to

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fisheries, public access, tourism, control of polluted runoff, exchange of public and private forest lands, and so on. The state badly needed centerpiece studies of the basic geography of those topics, and by the mid-1960s Minnesota geographers under John's direction were involved with virtually all of them.

An urgent need was for a study of the state's thousands of recreational lakes—their physical properties and status and trends in their development. John's team brought together data from sources scattered through state agencies and local courthouses and supplemented them with survey research. They joined all the data on a grid of 40-acre cells in a basic land survey covering 12,000 miles of inland lakeshore. The study had applications to public policy, lakeshore property development, and recreational businesses, and provided a context for contemporaneous research in the basic sciences that was necessarily localized.

Widespread interest led to the expansion of the lakeshore study to a statewide land inventory covering more than a million 40-acre cells. By 1972 the project had produced a land use map of the entire state, along with files that became the basis for the Minnesota planning agency's pioneering land management information system, an achievement of national renown. The big colored map might well have been the first such computer-generated civilian work in the United States. It soon hung in hundreds of state and local offices and libraries and raised many aspects of geographic awareness to a higher level.

Other applied and policy-oriented studies in the later 1970s and 1980s dealt with such disparate topics as higher education enrollments, historic preservation of buildings, origins and destinations of redistributed tax revenue, and the market value of land and buildings across the state. All included fieldwork and the analysis of a time series of maps.

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They focused on features of the settlement pattern, bringing geographic detail to topics that were otherwise being dealt with in generalities with only limited value in policy making.

SERVICE TO THE UNIVERSITY, COMMUNITY, AND SCIENCE

John Borchert described his principal academic interest as "geography applied to public policies in land use and resource management." He was University of Minnesota Geography Department chair in the 1950s and served as associate dean of the Graduate School and assistant to the vice-president for educational relationships and development. In 1969 he accepted directorship of the university's new Center for Urban and Regional Affairs. He served on numerous committees of the Association of American Geographers, served on its council and assumed its presidency in 1968 during the worst of the Vietnam War years, just in time to deal with controversy over the annual summer meeting of the association, that year scheduled for Chicago in the immediate aftermath of the Democratic National Convention. Amid hue and cry on several sides of the issue John ordered the meeting moved to Ann Arbor.

He served generously and effectively on scores of local, national, and international committees, commissions, and boards concerned with transportation, natural resources, land management, and pollution control. Notable professional service and recognition included chairmanship of the U.S. National Committee for the International Geographical Union and chairmanship of the National Research Council's Transportation Research Board and the Earth Sciences Division, as well as membership on numerous National Research Council committees; Social Science Research Council; National Science Foundation Social Science Advisory Committee; Science Advisory Panel, U.S. House of Representatives Com-

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mittee on Public Works; American Academy of Arts and Sciences; and Commission on Environmental Problems of the International Geographical Union. The American Geographical Society awarded him the Eugene van Cleef Gold Medal for Outstanding Contributions to Urban Geography, and the Association of American Geographers awarded him the John Brinkerhoff Jackson Publication Award for his book *America's Northern Heartland*.

In the last decades of his life John remained an active scholar, teacher, and public citizen. At the end he was close to finishing a book on the expansion and eventual contraction of the Pennsylvania Railroad system, using records of postal receipts as indexes of the functional importance of each urban node on the lines as they were laid down, used, and eventually abandoned. The method? What else? A series of meticulously constructed maps of lines and urban nodes.

When in his office, John's door was always open and the phone usually ringing, but he welcomed students and colleagues in with a smile, sat back in his chair with a foot on a desk drawer, hands behind his head, and gave us his full attention as we settled in for a chat. A question would elicit a story, a problem a thoughtful frown, followed by helpful advice or offers of help. Unopened mail and a backlog of reading were neatly stacked on his desk, alongside the picture of Jane, the love of his life, his financial manager, travel companion, square-dance partner, full-time homemaker, and mother of their four children, Dianne, William, Robert, and David.

NOTES

1. Several years before John Borchert died he began a memoir, a task he was unable to complete due to his abrupt death on March 30, 2001, following surgery to repair an ailment linked with an earlier health problem. Portions he finished appear on a family

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Web site at <http://www.borchert.com/john/>. We have drawn extensively on this material in preparing this memoir. One of us (J.A.) was a student (1960-66) and later departmental colleague (1970-2001); the other (V.R.) was a university colleague (1965-2001) and is a fellow member of the National Academy of Sciences.

2. Adams, J. S. 2001. The quantitative revolution in urban geography. *Urban Geogr.* 22:530-39.

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Wallace R. Brode

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WALLACE REED BRODE

June 12, 1900–August 10, 1974

BY DONALD S. McCLURE

DURING HIS LIFETIME Wallace Brode was known for his broadly based development of applied spectroscopy and for his able administration of numerous science-related organizations. He was equally at home in academe and in government. He used his high intellect and breadth of knowledge to promote the welfare of other people, being truly a scientific statesman.

He was born on June 12, 1900, as one of triplet brothers, each of whom became distinguished as a scientist. Their father, Howard, was a professor of biology, teaching at Whitman College in Walla Walla, a small town in southeastern Washington, where the family was reared. Like other colleges in the Northwest at that time, Whitman had been struggling out of its recent pioneer past in an attempt to become a credible educational institution with slim financial resources but having a dedicated president and faculty.[1] Everyone—father, mother, the triplets (Wallace, Robert,[2] and Malcolm), and an older son, Stanley—worked for or studied in the college, learning high ideals and hard work. Howard Brode is still honored by a yearly lectureship at Whitman College.

After receiving his B.S. at Whitman, Wallace studied under Roger Adams at the University of Illinois and was awarded

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his Ph.D. in 1925 with a thesis entitled "A Study of Optically Active Dyes, Mechanism of Dyeing and Absorption Spectra." His lifelong interest in dyes and the relation between their color and their constitution began here.

During his graduate school days he demonstrated an ability to handle several jobs at once: He was listed as a junior chemist at the National Bureau of Standards (NBS) in Washington, D.C., where he found better equipment for his thesis project, but was still a student and assistant at the University of Illinois. The position at the NBS was his introduction to the institution where he later became associate director.

In the years 1926-28 he was a Guggenheim fellow and did what budding scientists did then, went to Europe, where he studied at Leipzig (with Arthur Hantzsch), Zurich (with Victor Henri), and Liverpool (with E. C. C. Baly and R. A. Morton). Publications from these visits appeared promptly in the chemical literature. At the same time, however, he had an appointment with the Bureau of Engraving and Printing to travel in Canada and Europe and to report on methods of currency printing in those places. He was complimented for doing an effective job while getting his travel expenses taken care of. He returned to the United States to take up an appointment as assistant professor of chemistry at Ohio State University in the fall of 1928.

During his 20 years at OSU he produced most of the work in spectroscopy for which he is known. His book *Chemical Spectroscopy* appeared in 1939, the outgrowth of notes for a course in this subject, and in a greatly expanded second edition in 1943, which sold about 10,000 copies.[3] It was the first book to cover a broad range of topics in spectroscopy of interest to chemists. His research papers during this period (about 65) deal with the relation between absorption spectra and constitution of organic dyes, optically

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active dyes, analytical applications of spectroscopy, and several other subjects. He trained about 40 graduate students during his tenure at OSU, many of whom later assumed major positions in industry and academe.

His course on spectroscopy was well attended, and he must have spent much time and thought on teaching effectively. He persuaded Sargent Scientific Co. to manufacture the ball-and-stick molecular models he designed in 1930, which later became standard teaching aids in chemistry courses everywhere. He designed and built one of the first recording spectrophotometer/spectropolarimeters (1941).

He had a fascination for solar eclipses and observed about six. He was a member of the observing team of a successful expedition in Russian Siberia in June of 1936. This was followed by a grand tour of Russian universities and astronomical observatories. His ability to design and use spectrographic equipment was crucial to the success of these expeditions. His first wife, a physicist at the NBS whom he married in 1926, accompanied him on the Russian expedition. This marriage ended in divorce a few years later.

In late 1940 or early 1941 he was hospitalized for an infection that had to be treated with an antibiotic. His condition worsened until an alert head nurse, Ione (Sunny) Sundstrom, realized that it was the wrong medicine. He was saved and shortly afterward he married Sunny.

During the war he became associated with the Office of Scientific Research and Development and was head of the Paris branch in 1944-45. Intelligence was the function of this branch, and he followed the armed forces as they advanced in order to learn as much as possible about scientific and technical matters in the formerly occupied territory. One example was the manufacture of hydrogen peroxide

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in highly concentrated form, the oxidant in the V-2 rockets.

Extending his leave from OSU, he became head of the science department at the Naval Ordnance Test Station, Inyokern, California, 1945-47. He turned down an offer to extend his tenure there but remained as a consultant for some time. While still a professor at OSU in 1947, he accepted a temporary position at the NBS as an associate director, but later that same year he acceded to a request from the Central Intelligence Agency to set up a science advisory branch in that organization. He worked very effectively at this project for most of a year but, realizing that he was losing contact with science and scientists, he felt that he could not continue to attract well-qualified people and asked for a part-time arrangement with the NBS. This was not agreed to, so with three jobs to choose from he made up his mind to resign his rather neglected professorship at OSU and in 1948 became associate director of the NBS.

His acceptance, however, depended on a commitment from the then director, Edward Condon, that he could have a small laboratory where he could continue active research. This research was carried on with the help of George Wyman, John Gould, and later May Inscoe, who worked on the spectroscopy of dye molecules. During the years of this project they discovered some unexpected photochemical changes in the spectra and thus began accidentally a study of photochemistry.

Wallace had under his cognizance the following areas of the NBS: chemistry, metallurgy, mineral products, organic and fibrous materials, optics and metrology, foreign relations, education (the NBS had a graduate school), and editorial and publications.

He was an able administrator and was genuinely interested in the people who staffed these programs. He gave

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encouragement to many young people in furthering their professional careers. In addition to the above duties he edited the *Journal of the Optical Society of America* with the aid of his assistant, Mary Corning.

Honors that came to him during this period were election to the National Academy of Sciences in 1954, an honorary degree from Whitman College, and an honorary degree from Ohio State University. These two educational institutions sought his advice on several occasions, and he actively assisted in several projects for them.

In his new position Wallace became increasingly a public figure, as the activities of the NBS had impacts on society. In one very public fracas Eisenhower's new secretary of commerce, Sinclair Weeks, actually "fired" the NBS director, Allen V. Astin, when he refused to accept the "judgment of the market place" for a highly promoted battery additive, ADX-2, which NBS research had shown to be worthless. In the uproar that followed, during which Brode and others testified before Congress, the firing was rescinded and Astin served as director for many years afterward. On this and many other occasions Wallace found himself explaining to non-scientists what science is all about. (The hazards of being director of the NBS are also illustrated by the case of Edward Condon, who was hounded by the House Committee on Un-American Activities from March 1948 until he resigned in August 1951. These were the first three years of Brode's associate directorship.)

During this period the NBS was outgrowing its downtown Washington headquarters, and over Wallace's strong objections the decision was finally made to move out of town to Gaithersburg, Maryland. He feared that the NBS would lose valuable contacts with other science-based organizations in Washington and that he personally would lose many valuable ties there. Thus in 1958 he resigned his posi-

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tion at the NBS. He was the obvious choice for the next job, science advisor to Secretary of State John Foster Dulles and later Christian Herter. Probably the major motivation to place a prominent scientist in this position was the sudden realization that the United States was years behind the Soviet Union in space science (the Soviets had just launched their first *Sputniks*, while the United States months later launched the much smaller *Explorers*). One aim of this job was the re-establishment of scientific attaché positions in a number of embassies. He was able to persuade several reputable scientists to accept such posts and to identify major scientific opportunities and concerns within the context of foreign policy.

In this same year, 1958, he assumed several new positions: president of the American Association for the Advancement of Science, member of the Board of Governors of the American Institute of Physics, and member of the President's Committee on Scientists and Engineers. He was already a director of the American Chemical Society. He also received two honors: the Exceptional Service Medal from the Department of Commerce for his work at the NBS and the Applied Spectroscopy Medal from the Society for Applied Spectroscopy. In a few more years he became the president of the Optical Society of America in 1961, president of Sigma Xi in 1961, and was awarded the Priestley Medal of the American Chemical Society, its highest award, in 1960. He became president of the American Chemical Society in 1969. This list does not include the many committees and boards he joined during his Washington years.

Because he was a public figure he was invited to give talks at special occasions. He was both entertaining and informative. One subject that seemed especially needed in Washington was the distinction between science and pseudoscience. He also published articles on scientific manpower,

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developing a national science program, international aspects of science, and science in elementary schools, all of these being subjects of great interest to him.

The 26 years in Washington must have been the best in his busy life. He was a member and officer of the professionally and socially important Cosmos Club where visitors to Washington could be hosted. He and Sunny had a beautiful apartment across the street from the NBS, where many friends were entertained and Wallace could use his musical talents. He could sing parts in Gilbert and Sullivan operettas and also played the flute, as did his brother Robert. These two brothers had great affection for each other and met often in Washington and Berkeley.

Wallace and Sunny made many trips to the American Southwest to indulge their deep interest in American Indian history, culture, and arts. They possessed many unique and beautiful examples of pottery, baskets, and fabrics. Wallace's knowledge of dyes and dyeing led him to study the natural dyes and pigments used by the Indians.

After leaving the State Department in 1960 he had no major institutional affiliation and was able to write, travel, attend to professional society duties, and consult for industry. As his reputation had grown over the years, he was in great demand as lecturer, counselor, and consultant. He was a highly organized person as his detailed diaries show and he remained heavily engaged in these activities for the rest of his life. He died of cancer at the age of 74.

In the words of one who knew him well, "Wallace was a man of unusual intellect with a rare depth and breadth of knowledge. He could extrapolate from specifics and details to broader concepts, from one discipline to interdisciplinary considerations. He was the epitome of integrity in science and in public service."

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CONTRIBUTIONS TO SCIENCE BY WALLACE BRODE

Brode's scientific publications can be arranged in the following categories beginning with the most important one: relation between the optical spectrum and the structure of organic dyes; analytical methods; organic synthesis; colors and spectra of some inorganic materials; and chemistry of the fatty acids.

Brode's studies of the chemistry and spectroscopy of organic dyes began in 1922 when he was a graduate student of Roger Adams at the University of Illinois. Synthetic organic chemistry had been developing rapidly at this time, driven in part by the search for new dyes. A central question was how does the molecular structure of a dye determine its color? Spectrographs of that period, though clumsy and inaccurate, could measure the absorption spectrum responsible for color, but the absorption bands could not be explained by existing theory: Consider that G. N. Lewis had proposed the electron pair bond only in 1916, and Schroedinger's equation was still to come. In the expectation that useful empirical understanding of the spectra would result, Brode produced a series of carefully executed and extensive studies of the effects of structure on the absorption bands, published from 1926 to 1959.

The azo compounds, related to azobenzene, are the commonest types of dye molecule. They could be prepared easily and in great variety. A typical study was to add substituents such as methyl, halogen, or nitro at various positions on the benzene rings and see how the spectrum changed. Alternatively, one could couple two azobenzenes at the para, meta, or ortho positions and determine from the spectrum how much the two parts interacted. The results showed the importance of the planarity and conjugation of pi-electron systems. Later, with his NBS group he

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worked on indigo-type dyes in which the photochemistry or sometimes its absence was the most important aspect. For example, the inhibition of cis-trans isomerization by hydrogen bonding in these dyes was discovered.

In the meantime quantum theory was being discovered and developed and actually applied to large molecules. Some of the earliest thinking was done by G. N. Lewis and Melvin Calvin and by T. Foerster in the 1930s. In the next decade Coulson and Longuet-Higgins and later Dewar were making good sense of the spectra of aromatic molecules. During the 1950s the Pariser-Parr-Pople method was being applied with the help of increasingly capable computers. Brode never caught this wave, and much of his detailed work remains to be interpreted in terms of molecular electronic structure. Nevertheless, he did build a large body of information on dye spectra that has been extensively referenced and has influenced the field down to the present day.

ACKNOWLEDGMENTS

Mary Corning, formerly Wallace Brode's assistant at the NBS and later at the State Department, and George Wyman, who carried out research in Brode's NBS laboratory, have provided me with much valuable information for this memoir. Former associates Bourdon Scribner and Eugene Kovach were also helpful. A file of Brode's papers is held in the library of the National Institute of Standards and Technology (formerly the NBS). The most extensive file of personal and professional papers is the one held at the Library of Congress. A nearly complete publication list is available from SciFinder Scholar (chemical abstracts) and at <www.webofscience.com>.

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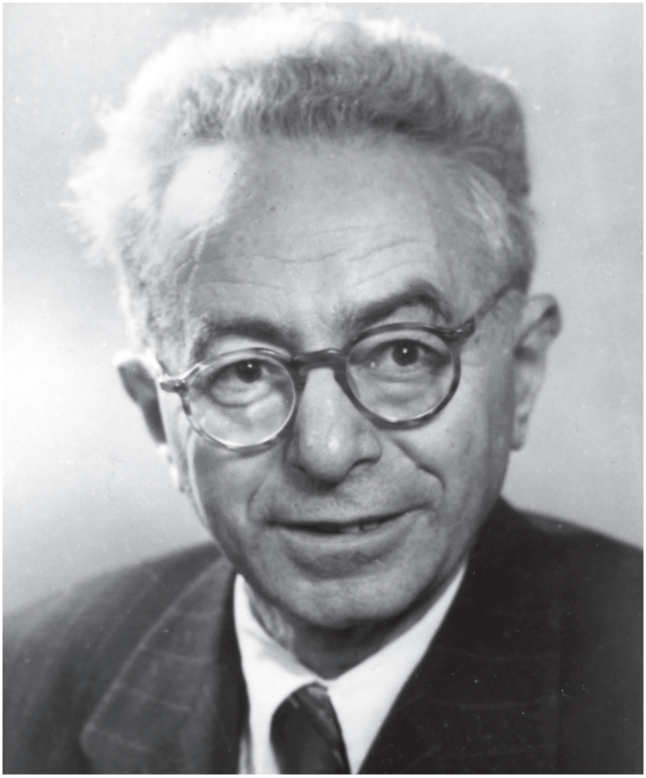

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RICHARD COURANT

January 8, 1888–January 27, 1972

BY PETER D. LAX

During his long and adventurous life Courant achieved many things in mathematics: in research and the applications of research, in the exposition of mathematics and the education of students, and in administrative and organizational matters. To understand how he, essentially an outsider both in Germany and the United States, accomplished these things we have to examine his personality as well as his scientific works. But let's start at the beginning.

Courant was born on January 8, 1888, in the small town of Lublinitz in Upper Silesia, now part of Poland but then of Germany. His father, Siegmund, was an unsuccessful businessman. The family moved to Breslau, and soon the precocious Richard was beginning to support himself by tutoring. In the *gymnasium* he came under the influence of a charismatic teacher of mathematics, Maschke, who inspired specially selected, talented students with a love of mathematics. Six years after Courant the young Heinz Hopf entered the *gymnasium* in Breslau and came under the tutelage of Maschke, who trained his special pupils by posing challenging problems. Many years later Hopf recalled that he was able to solve most of them, but was stumped every once in a while. "Courant could solve it," said Maschke.

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This no doubt was the first bond in the intimate friendship that developed later between Courant and Hopf.

After the *gymnasium* Courant was ready to attend university lectures on mathematics and physics at the University of Breslau. Because of the weakness of the physics faculty he gravitated toward mathematics. He spent a semester at the University of Zürich, but still dissatisfied, he set out in the fall of 1907 for what his Breslau friends, Otto Toeplitz and Ernst Hellinger, described as the Mecca of mathematics, Göttingen.

Not long after his arrival in Göttingen Courant was accepted as a member of the "in-group" of young mathematicians whose leader was Alfred Haar, and which included Toeplitz, Hellinger, and von Kármán but did not include Hermann Weyl. These brilliant young men were attracted to Göttingen by its stellar mathematics faculty: Klein, Hilbert, Minkowski, Runge, Zermelo, the fluid dynamicist Prandtl, and the astrophysicist Schwarzschild. For young Courant the shining light was Hilbert, and it was his great good fortune that in 1908 Hilbert chose him to be his assistant.

The next phase in his career was the writing of a dissertation. It is illuminating to go back more than 50 years to the dissertation of Riemann. Riemann proved the existence of harmonic functions by a variational method called Dirichlet's principle, according to which a certain quadratic functional—the Dirichlet integral—for functions of two variables assumes its minimum value. Weierstrass challenged the validity of this proof, because the existence of a minimum cannot be taken for granted. Weierstrass even gave an example of a fourth order functional whose minimum is not assumed by any function. In view of the basic nature of Riemann's work, there was a feverish effort by the leading mathematicians to furnish an alternative proof. Poincaré

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did it with a method he called "balayage." Carl Neumann derived and then solved an integral equation; this work paved the way to Fredholm's famous study of general integral equations, which in turn was followed by Hilbert's and subsequently Frederic Riesz's analysis of the spectrum of compact operators. Herman Amandus Schwarz supplied the missing proof by his famous "alternating method." One hundred years later, by one of those twists that are not infrequent in mathematics, the alternating method, now called "domain decomposition," turned out to be the most efficient *numerical* method for solving Riemann's problem and more general problems, when the calculations are performed by computers with many processors in parallel.

Hilbert's way of filling the gap was to supply the missing step in Riemann's argument, the existence of the minimizing function. Curiously, according to Haim Brezis and Felix Browder, Hilbert's own work is incomplete. The crucial idea for fixing Dirichlet's principle was supplied in 1906 by Beppo Levi.

Back to Courant. Hilbert suggested to him as a dissertation topic to use Dirichlet's principle to prove the existence of various classes of conformal maps. Courant succeeded, and was awarded his Ph.D. summa cum laude in 1910. The same topic served for his *habilitation* dissertation in 1912.

Dirichlet's principle remained a lodestar for Courant; he kept returning to it throughout his career. He was fascinated not only by its use in theory but also by the possibility of basing numerical calculations on it, as was done by the young physicist Walther Ritz.

Courant liked to spice his lectures with remarks about the personalities of scientists, to render them more human. Thus, in a talk in Kyoto in 1969, his last public lecture, he described the work of Walther Ritz and recalled that Ritz

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died young, at the age of 31, of tuberculosis, and that he refused to enter a sanitarium for fear that it would prevent him from completing his life's work. Then Courant added that Walther Ritz was a member of the Swiss family whose hotels all over the world made their name synonymous with luxury.

In 1912 Courant married Nelly Neumann, a fellow student from Breslau; the marriage lasted only four years. They were joined in Göttingen by Courant's favorite intellectual cousin from Breslau, Edith Stein, who became a student and later assistant to the philosopher Husserl. She attained martyrdom as a Jew and posthumous fame as a saint of the Roman Catholic Church, canonized by Pope John Paul II in 1998.

Harald Bohr turned up in Göttingen in 1912; he and Courant became fast friends. They wrote a joint paper on the distribution of the values of the Riemann ζ function along the lines Re ζ = const in the critical strip. This was Courant's only venture into number theory. The friendship with Harald Bohr later came to include Harald's brother, Niels, and lasted until the end of Niels's life.

The idyllic Göttingen life was shattered, like everything else in Europe, by the outbreak of the First World War; the flower of European youth was led to slaughter. Courant was drafted into the army; he fought on the western front and was seriously wounded. While in the trenches, Courant had seen the need for reliable means of communication, and came to the idea of a telegraph that would use Earth as a conductor. He consulted Telefunken, the German telephone company, and his teacher Carl Runge in Göttingen, who brought Peter Debye and Paul Scherrer into the project. In the end the Earth telegraph became a resounding success; equally important, the experience taught Courant how to

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deal successfully with people of all classes: officers of rank high and low, engineers, industrialists.

Courant's absence in the army did not make his and Nelly's hearts grow fonder. On the contrary, it made both of them realize their incompatibility. After their divorce in 1916 he found himself drawn to Nina Runge, daughter of Carl Runge, professor of applied mathematics in Göttingen, and she to him. They were married in 1919. They had much in common—a passionate love of music—but in many respects they were very different. Their marriage was a successfully shared life. They had four children: two boys, who became physicists, and two girls, a biologist and a musician.

The years 1918-20 were banner years for Courant. He proved that among all plane domains with prescribed perimeter, the circle had the lowest fundamental frequency. This was followed by a max-min principle that enabled him to determine the asymptotic distribution of eigenvalues of the Laplace operator over any domain, a result of great physical interest, established previously by Weyl with the aid of a min-max principle. Weyl's method leads naturally to upper bounds for the eigenvalues, Courant's to lower bounds. The combination of the two methods is particularly effective.

It was during this period that Courant's friendship with the publisher Ferdinand Springer matured. Courant encouraged Springer to enlarge his offering in mathematics. This led to Springer's taking over the *Mathematischen Annalen* and starting the *Mathematischen Zeitschrift*. Equally important was the new book series "Grundlehren," affectionately known as the Yellow Peril for its yellow cover.

After the war Courant was offered and accepted a professorship in Münster, but this was merely a steppingstone for a position the following year in Göttingen, pushed through by Hilbert and Klein. The latter saw Courant—correctly—

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as one who would share his vision of the relation of mathematics to science, who would seek a balance between research and education, and who would have the administrative energy and savvy to push his mission to fruition.

The early 1920s were a tough time in Germany. The defeat in the First World War had demoralized large segments of society and had led to rampant inflation. Courant showed his resourcefulness by keeping things afloat, partly with the help of the far-sighted industrialist Carl Still.

In 1922 Courant's first book appeared, *Hurwitz-Courant on Function Theory*, the third volume in the Yellow Peril series. The first part, based on lecture notes of Hurwitz, was written from the Weierstrass point of view; its main subject was elliptic functions. Courant supplemented this material with nine chapters on Riemann surfaces, conformal mapping, and automorphic functions. Courant used an informal, intuitive notion of a surface that displeased some readers but pleased others.

Two years later, in 1924, the first volume of Courant-Hilbert appeared. It was based on lecture notes of Hilbert but even more on Courant's own research in the past five years. The book starts with a 40-page chapter on linear algebra, presented from an analytic point of view, so that generalization to infinite dimension comes naturally. This is followed by chapters on orthogonal function systems, the Fredholm theory of integral equations, the calculus of variations, and the vibrations of continuum mechanical systems, using extensively the spectral theory of self-adjoint ordinary and partial differential operators. In 1926 Schrödinger invented his wave mechanics, formulated in terms of partial differential operators and their eigenvalues and eigenfunctions. Fortuitously, Courant-Hilbert Volume I contained much of the mathematics needed to understand and solve

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Schrödinger's equations. This was a striking example of mathematics anticipating the needs of a new physical theory.

In 1928 Courant, Friedrichs, and Lewy published their famous paper on the partial difference equations of mathematical physics. The main motivation for writing it was to use finite difference approximations to prove the existence of solutions of partial differential equations. The paper discusses elliptic, parabolic, and hyperbolic equations; it contains a wealth of ideas, such as the probabilistic interpretation of elliptic difference equations, and the restriction that has to be imposed on the ratio of the time increment and the space increment. The latter, known as the CFL condition, became famous during the computer age. Woe to the computational scientist who ignorantly violates it. This is an outstanding example of research undertaken for purely theoretical purposes turning out to be of immense practical importance.

In 1927 Volume 1 of Courant's calculus text appeared, soon followed by Volume 2. It has been extremely successful in every sense; its translation into English by McShane has sold 50,000 copies of Volume 1 and 35,500 copies of Volume 2 in the United States. It has shaped the minds of many who wanted and needed a deeper grasp of the calculus. Even after 70 years it is better than most, nay all, calculus books in use today in the United States.

In the 1920s and early 1930s Göttingen became again a Mecca of mathematics, as well as of physics. A list of visitors, long term or short term, reads like a Who's Who of mathematicians: Alexandrov, Artin, Birkhoff, Bohr, Hopf, Hardy, Khinchin, Kolmogorov, Lyusternik and Shnirelman, MacLane, von Neumann, Nielsen, Siegel, Weil, Weyl, Wiener, and many others. Paul Alexandrov described the atmosphere thus: "[Göttingen was] one of the principal centers of world mathematical thought; the place to which all mathemati-

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cians came from all over the world, of all possible trends and ages, where there was an exchange of all mathematical ideas and discoveries as soon as they had arisen, no matter where. . . ."

There were many assistants and postdocs around; Courant had private sources of money to pay their stipends. This caused some confusion after the Second World War, when the German government, to its credit, decided to compensate not only faculty members who were dismissed by the Nazis but assistants as well. Many of the assistants in Göttingen thus dismissed had a hard time establishing their claim, for their names did not appear on the roster of those whose salary was paid by the university.

In 1926 concrete negotiations were started, and plans laid, for housing the institutes of mathematics and physics in a permanent building, for long a dream of Felix Klein, now enthusiastically taken up by Courant. The International Educational Board of the Rockefeller Foundation agreed to supply $350,000, and the Prussian Ministry of Education agreed to cover the maintenance costs.

The building of the institute was finished and dedicated in 1929. Courant became its director. Yet this moment of triumph already contained the seed of its own destruction, and that of most civilized Western institutions. The stock market in the United States crashed a few months earlier, leading to a deep economic depression that soon became worldwide. The misery caused by this drove a sizeable part of the German voting population, already embittered by the defeat in the First World War, to support the Nazis. In January 1933 the Hitler gang took over the government. It soon established a new age of barbarism in Germany. For a start, Jewish employees of the state, including professors, were dismissed summarily, Courant among the first. For once his grasp of reality deserted him, and he went from

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pillar to post to be reinstated. A chance encounter with a member of the Nazi party, who was a member of the university community, set him right. "No doubt you and your friends believe that the excesses of the first few months of the new regime will die down and everything will return to how it was before," said the man. "You are mistaken. Things will get worse and worse for you. You had better get out while you can."

Get out Courant did. After a brief stay in England he, his family, and some family friends landed in New York, where thanks to Oswald Veblen and Abraham Flexner, a position was offered to him in the department of mathematics of New York University, with the charge to develop a graduate program. His host there was the mathematician Donald Flanders, admired by all for his saintly character. He and Courant formed a deep friendship that today extends to their children.

At NYU Courant found a mathematical desert. How he made it bloom is a fascinating story. It started in 1936 with a burst of creative energy. He showed how to solve Plateau's problem—finding a minimal surface spanning a given contour in space—by using Dirichlet's principle. A solution of this classical problem had been found earlier by Jesse Douglas, and in another way by Tibor Radó, but the elegance and simplicity of Courant's method had opened the way to attack more general problems concerning minimal surfaces, such as minimal surfaces spanning multiple contours, of higher genus, and having part of their boundary restricted to a prescribed surface. Courant pursued these generalizations during the next 10 years. The culmination was the book *Dirichlet's Principle, Conformal Mapping, and Minimal Surfaces* that appeared in 1950.

In 1937 Courant was joined at NYU by his brilliant former student from Göttingen, K. O. Friedrichs, and by James J.

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Stoker, an American. Stoker's original training had been in engineering. In the 1930s, mid-career, he decided to seek a Ph.D. in mechanics at the Federal Institute of Technology in Zürich. One of the first courses he took there was by Heinz Hopf on geometry. Stoker was so charmed by the subject, and the teacher, that he switched his doctoral studies to differential geometry. Hopf wrote to Courant to call his attention to this *junger Amerikaner* whose scientific outlook and temperament were so close to Courant's.

With Friedrich's help Courant was able to complete the long-awaited second volume of Courant-Hilbert. It was, along with Hadamard's book on the Cauchy problem, the first modern text on partial differential equations.

In 1941 *What Is Mathematics?* by Courant and Robbins appeared, a highly popular book written "for beginners and scholars, for students and teachers, for philosophers and engineers, for classrooms and libraries." In the preface Courant warns about the danger facing the traditional place of mathematics in education, and outlines what to do about it. It ought to be compulsory reading for all who today are engaged in reforming the teaching of mathematics.

Courant found in New York City a "vast reservoir" of talented young people, and he was eager to attract them to study mathematics at NYU. To enable those who worked during the day to attend classes, graduate courses were offered in the evening, once a week, for two hours at a time.

America's entry into the Second World War transformed most American academic scientific institutions, none more than Courant's operation at NYU. Government funding was made available for research relevant to war work through the Office of Scientific Research and Development (OSRD). Its head, Vannevar Bush, saw the importance of mathematics for the war effort and set up the Applied Mathematics

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Panel under the direction of Warren Weaver. Courant was soon invited to be a member of this elite group.

The mathematical project at NYU sponsored by the OSRD was about the flow of compressible fluids in general and the formation and propagation of shock waves in particular. There was enough money to support young research associates (Max Shiffman, Bernard Friedman, and Rudolf Lüneburg), who also served as adjunct faculty in the graduate school. There was also money to provide stipends for graduate students, some of whom were drawn into war work. Courant insisted that graduate training continue even during the war.

This is a good place to describe Courant as a classroom teacher. He seldom bothered to prepare the technical details of his lecture. He muttered in a low voice, and his writing was often indecipherable. Nevertheless, he managed to convey the essence of the subject and left the better students with a warm glow of belief that they could nail down the details better than the master.

Courant supervised many graduate students' doctoral dissertations, more than 20 in Göttingen and a like number at NYU. Among the former were Kurt Friedrichs, Edgar Krahn, Reinhold Baer, Hans Lewy, Otto Neugebauer, Willi Feller, Franz Rellich, Rudolf Lüneburg, Herbert Busemann, and Leifur Asgeirson. In the United States he taught Max Shiffman, Joe Keller, Harold Grad, Avron Douglis, Martin Kruskal, Anneli Lax, Herbert Kranzer, and Donald Ludwig—a very fine record.

The bulk of the research conducted during the war was fashioned into a book on supersonic flows and shock waves. The editing was in the hands of Cathleen Morawetz, who had the delicate task of reconciling Courant's freely flowing exposition with Friedrichs's demand for precision. *Supersonic Flow and Shock Waves* appeared in book form in

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1948; it was a very useful and successful treatise, with a mathematical flavor, on the flow of compressible fluids.

As the war neared its end Courant's thoughts turned to postwar developments. He shrewdly realized that after the war the U.S. government would continue to support science. The critical contribution of science to the war effort had been noted by statesmen: radar, the proximity fuse, bombsights, code breaking, aerodynamic design, and the atomic bomb. Courant also realized that applied mathematics would be an important part of the government's plans, and he successfully used his wartime contacts to gain support for his vision of applied mathematics. Support came from the office of Naval Research, the Atomic Energy Commission, the offices of Army and Air Force research, and later from the National Science Foundation. As always, he emphasized that research must be combined with teaching.

When it came to hiring faculty, Courant relied on his intuition. The candidate's personality was often more important than the field he was working in. Courant did not like following fashions and fads. "I am against panic buying in an inflated market" was his motto.

Courant did not extend his hatred of Nazis to the German nation. He overcame his resentments and was eager to help those deserving help. He traveled to Germany as soon as it was possible, in 1947, to see the situation there firsthand, to talk to people he trusted. He arranged visits to the United States for a number of young mathematicians; this had a tremendous effect psychologically and scientifically and earned Courant the gratitude of the younger generation of German mathematicians and later the Knight Commander's Cross of the Order of Merit of the Federal Republic of Germany.

In 1948 Courant's sixtieth birthday was celebrated with much emotion by mathematicians invited from both sides

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of the Atlantic; nostalgia flowed like water, held within bounds by Courant's natural irony.

In 1954 the Atomic Energy Commission decided to place one of its supercomputers, the UNIVAC, at a university. After a fierce competition Courant's institute was chosen. The UNIVAC had a memory of 1,000 words and used punched cards. The commission repeatedly replaced it with newer models; the last one was a CDC 6600, installed in 1966 and put out to pasture in 1972.

Courant retired in 1958 at age 70; his successor was Stoker. In retirement Courant succeeded in finishing the translation into English of Volume 2 of *Methods of Mathematical Physics*. This was no mere translation. Courant made a serious attempt, with much help from younger colleagues, to update the material from a mere 470 to over 800 pages. The book ends with a 30-page essay written by Courant on ideal functions, such as distributions. The last chapter in the German original, on existence proofs using variational methods, was omitted. Courant planned to rework it, together with a discussion of finite difference methods, and to issue it as Volume 3. Alas, he was not up to the task.

Courant was deeply concerned about the Cold War. He felt that the natural comradeship of scientists, in particular of mathematicians, might set an example and overcome the "us versus them" stereotypes. Accordingly, he was among the first to visit the Soviet Union. The time—the summer of 1960—was not auspicious, for the Soviets had just shot down a U.S. U-2 spy plane. The remains of the plane and the spy paraphernalia were displayed in the middle of Moscow's Gorky Park. There was a long line of curiosity seekers. As a distinguished visitor, Courant was whisked to the head of the line and was introduced to the aeronautical engineer who was there to explain the workings of the U-2. The engineer was deeply honored: "Professor Courant, I learned

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aerodynamics from your book." It had been translated in 1950 into Russian, as were all of Courant's other books.

In 1963 Courant led a delegation of about 15 U.S. mathematicians to a two-week conference on the occasion of the opening of the Academic City and University at Novosibirsk. It was a golden time and gave rise to friendships that lasted lifetimes.

A very generous gift from the Sloan Foundation, augmented by the Ford Foundation and the National Science Foundation, was used to construct a handsome 13-story building just off Washington Square, in which the Courant Institute, so named at its dedication in 1965, still nestles. Its architects won all kinds of prizes and went on to build for departments of mathematics at Princeton and Rutgers.

Courant's last years were full of recognition and honors. Solomon Lefschetz admired Courant for having built an enduring school of mathematics, and had nominated him to receive a National Medal of Science. None of these encomiums, however, could lift Courant's spirits in his extreme old age. His institute was thriving, his children and grandchildren were happily launched on careers, but none of that would dissipate his gloom. His old stratagems to overcome depression—embark on a new project, make the acquaintance of a fascinating woman—were no longer available to him. He even stopped playing the piano, which had been a great source of pleasure for him in the past, a way of transcending conflicts and disappointments. He died on January 27, 1972, at the age of 84.

It is time to look back and ask what manner of man was Courant. For this we must look at the testimony of people who knew him intimately. Surprisingly, they were utterly different from Courant in many, sometimes all ways, such as Flanders, a descendent of Puritans and a puritan himself. Flanders was haunted by a lack of confidence. He loved

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and needed Courant's ebullience, and Flanders's wit and pure spirit were deeply necessary to Courant.

There was Otto Neugebauer, a meticulous and workaholic scholar, about whom Courant said that "he had all the virtues and none of the faults of pedantry." Neugebauer in turn described Courant's style of operation thus:

All that lies before us as scientific achievement and organization seems to be the outcome of a well conceived plan. To us, nearby, things seemed sometimes more chaotic than planned, and we were far from always in agreement. But a never failing loyalty bound Courant's associates together; he inspired an unshakable confidence in his profound desire to do what was right and what made sense under the given circumstances. His ability to create a feeling of mutual confidence in those who know him intimately lies at the foundation of his success and influence.

Friedrichs was another former student utterly devoted to Courant, although totally different from him. He described the excitement he felt when as a young student he read Courant's presentation of geometric function theory in Hurwitz-Courant.

It is true that there were some passages in which matters of rigor were taken somewhat lightly, but the essence came through marvelously. I was reminded of this effect much later, when I heard Courant play some Beethoven piano sonata. There were also some difficult passages which he somehow simplified; but the essence carried over wonderfully. In a way, one could perhaps say the same thing about his skiing—a sport which, incidentally, his assistants were expected to have mastered, or else to learn from him. Never mind the details of the operations, he always managed to come down the mountain quite safely.

Here are the observations of another close friend, Lucile Gardner Wolff.

There are some great men—and among them some of the greatest—who owe their preeminence not merely to their good qualities but to their bad, or to what would have been bad in another man; whose talents, however remarkable, cannot in themselves account for their achievements or ex-

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plain why they succeeded where others, equally gifted, failed; whose genius lay precisely in the ability to turn their weak points to good account. Such a one is the late Richard Courant.

Again Friedrichs:

As a person Richard Courant cannot be measured by any common standard. Think of it: a mathematician who hated logic, who abhorred abstractions, who was suspicious of truth—if it was just bare truth. For a mathematician these seem to be contradictions. But Richard Courant was never afraid of contradictions—if they could enhance the fullness of life.

Courant loved to be with young people. He understood their ambitions and anxieties and was ready with useful, often unconventional advice. For many the Courant Institute was a second family, and some of this spirit abides today.

Courant has been gone for nearly 30 years, surely enough time for the verdict of history. What is remembered? His insistence on the fundamental unity of all mathematical disciplines and on the vital connection between mathematics and other sciences. The name he gave to his institute—Institute of Mathematical Sciences—expressed his attitude. It is remarkable that today many of the leading mathematical institutions have adopted this appellation.

Courant insisted that research be combined with teaching, a philosophy he liked to trace back to the French Revolution and the founding of the Ecole Polytechnique.

Courant was a superb writer, in both German and English. Three of his books—*What Is Mathematics?*, his two volumes of calculus, and *Supersonic Flow and Shock Waves*, written with Friedrichs—are alive and well. Even his occasional pieces are worth re-reading. For example, in an article on mathematics in the modern world, which appeared in 1964 in *Scientific American,* he wrote,

To handle the translation of reality into the abstract models of mathematics

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and to appraise the degree of accuracy thereby attainable calls for intuitive feelings sharpened by experience. It may also often involve the framing of genuine mathematical problems that are far too difficult to be solved by the available techniques of the science. Such, in part, is the nature of the intellectual adventure and the satisfaction experienced by the mathematician who works with engineers and natural scientists on the mastering of the real problems that arise in so many places as man extends his understanding and control of nature.

Those who wish to find out more about Courant's adventurous life can learn much from Constance Reid's biography subtitled "The Story of an Improbable Mathematician." Those who knew him remember his great warmth and kindness, often disguised by irony, his energy and enthusiasm coupled with skepticism, and his inexhaustible optimism in the face of seemingly insurmountable obstacles.

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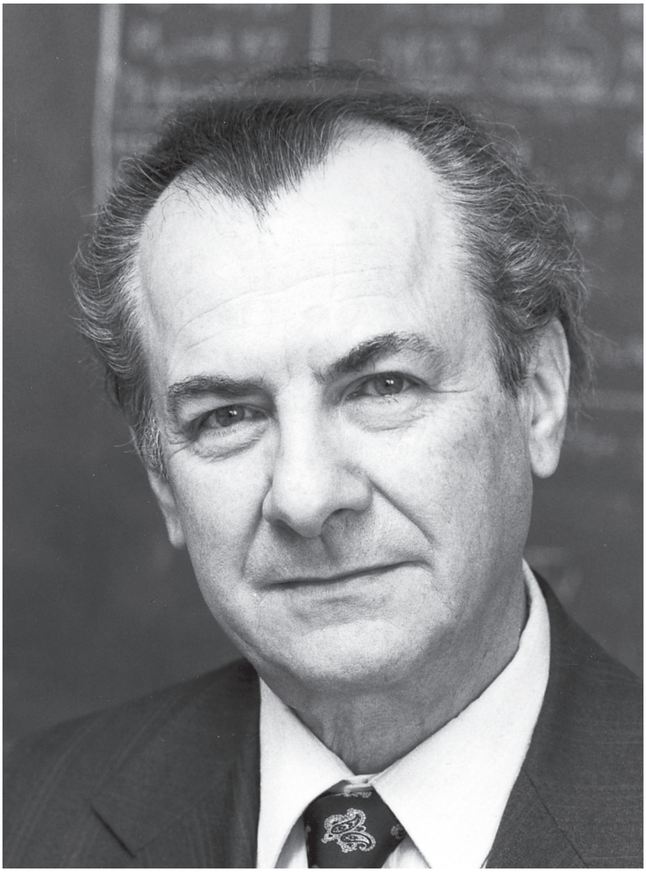

G. de Vaucouleurs

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GÉRARD DE VAUCOULEURS

April 25, 1918–October 7, 1995

BY E. MARGARET BURBIDGE

GÉRARD HENRI DE VAUCOULEURS began a lifetime in astronomy as an amateur, observing planets in the solar system from early childhood; his name is especially linked with his early studies of the planet Mars. His major achievements, however, have been in his work on galaxies—the extragalactic universe—and his name will always be tied to that of his wife and coworker in these fields, Antoinette (Piétra) de Vaucouleurs. Gérard was elected to the National Academy of Sciences in 1986.

Gérard de Vaucouleurs was born in Paris on April 25, 1918; he died in Austin, Texas, on October 7, 1995. His family background and father's name have proved elusive, since early in life he took the maiden name of his mother, de Vaucouleurs. He had a sister; she died in the same year as his mother, some eight years before his death.

From an early age Gérard was interested in astronomy. As a boy of 10 he observed the Moon from the balcony of his family's apartment, using a marine telescope borrowed from a friend.[1] A few years later his mother purchased a small telescope for him. By age 15 he was an enthusiastic amateur astronomer,[2] passionately interested in solar system astronomy. At this young age he observed various stellar

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occultations and planetary conjunctions, an eclipse, and with his small telescope he photographed the planet Mars in close approaches to the Moon and to the Pleiades star cluster. He presented his data to the amateur Societé Astronomique de France; these included observations of Mercury, Venus, Jupiter, and Saturn, and some good drawings of Jupiter.

A photograph taken in the summer of 1934[2] shows a group of astronomers and visitors at the Juvisy Observatory of Flammarion, including the well-known French astronomers Henri Mineur and André Couder and, second from the right-hand edge of the photo, a neatly dressed young man labeled tentatively from his short height and the widow's peak in his dark hair as G. de Vaucouleurs.

EARLY EDUCATION IN PARIS; THE PLANET MARS; JULIEN PÉRIDIER'S OBSERVATORY AT LE HOUGA, FRANCE; WORLD WAR II

Gérard began formal studies at the Sorbonne Research Laboratory, Paris, France, in 1936 and received his undergraduate degree in 1939. By this time, at age 19, he had acquired a 7.5-cm refractor and was deeply involved in solar system observations, particularly of the planet Mars. By 1939 the Commission on Mars of the International Astronomical Union had been reconstituted because of the forthcoming favorable opposition of Mars on July 23, 1939. The commission met, with G. Fournier presiding and de Vaucouleurs as secretary.[2]

In 1939 de Vaucouleurs began his long association with Julien Péridier, who had built a private observatory at Le Houga in southwest France, with an excellent 8-inch visual and photographic f/13 double refractor and a 12-inch Newtonian reflector.[3] A description of Gérard at this time[2] is of a rather short, neat young man, smiling, dedicated to his work, precise, and letting no detail escape his attention.

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The association with Julien Péridier was interrupted by the start of World War II; de Vaucouleurs served in the French army (artillery) from November 1939 to May 1941.

RETURN TO PARIS AND LE HOUGA: NEW INTERESTS IN ASTRONOMY, CONTINUATION OF FORMAL EDUCATION

De Vaucouleurs returned to Le Houga in July 1941, and without abandoning his lifelong interest in Mars he turned his attention to the measurement of close double stars, variable stars, and the brightness variations of the asteroid Eros. He was in Le Houga from 1941 to 1943.

He returned to Paris in 1943 to work from 1943 to 1949 on his dissertation at the Laboratoire des Recherches Physiques of the Sorbonne and at the Institut d'Astrophysique, Boulevard Arago, Paris. His thesis was on the molecular (Rayleigh) scattering of light, with accurate laboratory measurements of scattering and depolarization in various liquids and gases. This was published in full in 1950.[4] He met Antoinette Piétra, who was studying mathematics, physics, and astronomy at the Sorbonne, and they were married on October 31, 1944.

De Vaucouleurs continued his association with the Péridier Observatory in Le Houga, applying the research done for his thesis to light scattering in the Earth's atmosphere. He made observations with a visual photometer of his own design on the zenith sky brightness during evening and morning twilight at Le Houga. This experience stood him in good stead years later when he had been appointed at the University of Texas and was doing stellar and galaxy photometry at the McDonald Observatory. He made a similar study of the sky in southwest Texas then and published it in detail.[5]

INSTITUTE OF ASTROPHYSICS, PARIS

From his work at Le Houga Observatory, Gérard had

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taught himself to be an expert in astronomical photography and photographic photometry. After the end of World War II, when the Institut d'Astrophysique reopened, Gérard was appointed to work with Daniel Chalonge. He had responsibility for the Chalonge microphotometer, a photographic instrument for recording the intensity of photographic spectra obtained at the telescope onto photographic paper. Subsequent to the recording the paper had to be developed in the darkroom. This apparatus was not the simplest to use (Fabry is quoted as having said "le microphotomètre Chalonge, il ne marche que quand son papa est là!"[6])

When Geoffrey and I arrived in late summer of 1949, after a month spent photographing stellar spectra at l'Observatoire de Haute Provence, with permission to stay in the Institut d'Astrophysique and run our spectra through the Chalonge instrument, Gérard de Vaucouleurs was assigned the task of showing us how to use it. We discovered that "le microphotomètre Chalonge marche assez bien sous la direction de Gérard!" That was the beginning of our friendship with Gérard.

LONDON

Gérard's fluency in spoken and written English, as well as his scientific qualifications, led to his appointment with the BBC in 1949 to run a weekly radio program on science, with guest speakers who could deal with questions in French. He and Antoinette rented a small house in northwest London, not too far from the University of London Observatory (ULO) in Mill Hill. Gérard lost no time in asking whether he could carry out some useful work at the observatory.

Following the end of World War II the lenses of the 24-inch photographic and 18-inch visual Radcliffe refractors had been retrieved from their wartime tomb at the bottom of the telescope's concrete pier, and the ULO program on

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parallax and proper motion had been resumed. Gérard was delighted to join this program, both in taking the photographic plates during the night and in the daytime measuring them on the large two-coordinate measuring engine in the Wilson Building. Gérard would come to the observatory in the evening by bus, and it was my pleasure to seat him on the pillion of my small motorbike and run him back to his home after dawn. Antoinette's daytime work at ULO was in measuring spectra taken with the Wilson 24-inch reflector.

Gérard and Antoinette became good friends with C. C. L. Gregory and family, in whose home Geoffrey and I were living at the time. Clive Gregory prided himself on a special recipe for spaghetti with tomatoes and onions (Mill Hill grown!) involving careful timing and judicious use of one beaten egg (food rationing was still in force in 1949, so eggs and butter were scarce and precious). Spaghetti was easily available, and tomatoes and onions grown in the grounds of Mill Hill Observatory were plentiful, so Gregory's dish was enormous and, since it comprised the whole dinner, large helpings and seconds were given. At the dinner hosting Gérard and Antoinette, they ate modest portions, expecting that something would be coming afterwards. As the plates were cleared, it became obvious that this was the end of dinner. Gérard confessed later that they left hungry and that Antoinette, who had great skill in the kitchen, would have been ashamed to serve a dinner consisting only of spaghetti.

MT. STROMLO OBSERVATORY, AUSTRALIA

The telescopes at Mill Hill and the all too frequent cloudy skies over England meant that Gérard was soon looking for better opportunities to carry on his life work. In 1951 he and Antoinette were appointed at the Mt. Stromlo Observa-

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tory, Canberra, Australia, where they worked from 1951 to 1957. During the period 1951-54 Gérard was a research fellow at the Australian National University, and all observational astronomers will recognize what he must have felt on surveying the skies of the Southern Hemisphere, including especially our nearest companion galaxies, the Magellanic Clouds.

His first important papers published shortly after settling in Australia, where he and Antoinette lived on Mt. Stromlo, was on two pieces of research for which he is famous: (1) the relation between the mass and luminosity in elliptical galaxies (that the luminosity varies as the 1/4 power of the mass from the outermost reliable luminosity measures to the inner core) and (2) "Evidence for a Local Supergalaxy" (1953) expounding his observational evidence for the organization of galaxy clusters into the larger structures, superclusters. It was not long, however, before Gérard was deeply into Southern Hemisphere studies.

Gérard brought to Mt. Stromlo a twin Aero-Ektar camera, which he set up to investigate the structure of the Magellanic Clouds. The objective lenses had a 7-inch focal length and, stopped down to f/4, a usable field of over 20°. He used this camera to make a mosaic of long-exposure photographs covering about 40° of sky, including both the Large and Small Clouds. His important paper (1955) reproduces his mosaic of both clouds made from the Aero-Ektar plates.

As Gérard's abstract to the 1955 paper states, his plates showed an extensive spiral pattern in the Large Magellanic Cloud, with trailing arms, and this study was followed up by studies with the radio astronomer Frank J. Kerr, using the 21-cm line from neutral hydrogen. This line had been predicted by H. van de Hulst in Leiden during World War II and was detected shortly afterwards. Therefore, it was natural that de Vaucouleurs should look at Frank Kerr's map of 21-cm velocity residuals across the Large Magellanic Cloud,

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and he was able to recognize the presence of a typical rotation pattern for a disk galaxy. The paper by Kerr and de Vaucouleurs giving their results was published in the *Australian Journal of Physics* in 1955.

RETURN TO THE UNITED STATES: LOWELL OBSERVATORY AND HARVARD COLLEGE OBSERVATORY

I met Gérard and Antoinette again in 1957 at a meeting of the American Astronomical Society. I had with me some spectra of galaxies that Geoffrey and I had obtained at the McDonald Observatory. We were at the time working at the Yerkes Observatory, University of Chicago, and were spending much time at McDonald, observing the rotation curves of galaxies for determination of their masses and mass-to-light ratios. I recall during this society meeting Antoinette loaning me a small iron with which she always traveled (she was always so beautifully dressed!) so that I could iron creases out of my dress.

I showed Gérard and Antoinette a spectrogram we had obtained of the southern galaxy NGC 5128. We were proud of having managed, by lying on the roof of the McDonald coudé telescope room, to observe that far-south galaxy (declination –43°!) and clearly detecting the rotation of gas in the dark belt of dust across NGC 5128. Gérard was fascinated, and I think the spectrogram played a part in persuading him that it was time to leave the Southern Hemisphere and come to the United States. With Gérard's early interest in the planet Mars as a clear recommendation to the Lowell Observatory, Arizona, long famed for planetary studies, he was appointed there. He spent one year at Lowell, and then the years 1958-60 at Harvard College Observatory. Publications while he was at Lowell included a study "Magnitudes and Colors of Galaxies in the U, B, V System" (1959) (using Harold Johnson's photometric system rather

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than the old photographic and visual magnitudes). This may have been the start of a friendship between Gérard and Harold Johnson, leading to Gérard's appointment at the University of Texas in 1960.

PASADENA AND THE CLASSIFICATION OF GALAXIES

Allan Rex Sandage was working at 813 Santa Barbara Street on his major work on the extension of Edwin Hubble's classification of galaxies, from ellipticals through the spiral sequence to the irregular galaxies, and production of the Hubble Atlas. He spent much time with de Vaucouleurs during the time that Gérard and Antoinette were visiting Pasadena, gave Gérard access to his photographic plates, and encouraged Gérard to develop his own classification scheme. This classification scheme was described in several publications starting with a 35-page chapter in the *Handbuch der Physik* on "Classification and Morphology of External Galaxies."

THE UNIVERSITY OF TEXAS AND MCDONALD OBSERVATORY: WORK ON GALAXIES FROM 1960

The McDonald Observatory in southwest Texas was created in 1938 under the leadership of Otto Struve, at Yerkes Observatory, University of Chicago. For many years McDonald Observatory was managed by the director and colleagues at Yerkes Observatory and its instruments were designed and built there. The so-called B spectrograph, used at the prime focus of the 82-inch telescope to photograph galaxy spectra and whose spectrogram of NGC 5128 had so intrigued de Vaucouleurs, had been built by Horace Babcock and used extensively by Thornton Page for radial velocities of double galaxies, and by Geoffrey Burbidge, Kevin Prendergast, and me for our work on rotation curves, masses, and mass-to-light ratios in spiral galaxies.

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In 1959 the University of Texas began the development of an astronomy department and by 1969 management of the McDonald Observatory had passed from the University of Chicago to the new University of Texas department. From his appointment in Texas to his death in 1995 Gérard was a leader in observational work, the teaching and encouragement of students, and the publication of much important work. He developed new instrumental techniques, such as the use of Fabry-Perot interferometry, to measure the velocity fields in spiral galaxies and along with Antoinette turned his attention to galaxies with activity in their nuclear regions.

An article by Jean Heidmann,[7] published in the memorial volume for Antoinette de Vaucouleurs, reported that he had been told by his friend Pascal Fouqué that Antoinette had noted in 1958 that the luminosities of the central parts of the nuclear regions of several Seyfert galaxies varied perceptibly in as short a time as a month. Seyfert galaxies, named from the original Mt. Wilson paper on a dozen of these by Carl Seyfert, have bright nuclei displaying emission lines in their spectra and are now recognized as prototype active-galactic-nuclei galaxies. Antoinette, with her careful attention to all details of the galaxies she was examining, had spotted the variability. Apparently Gérard, as he admitted later, was skeptical and told her that if anything in the Universe did not vary, it was surely the galaxies! "That was the greatest error in my life!" Gérard told Pascal Fouqué.[8]

Ten years after Antoinette's observation, details of observations of such variability appeared in 1968 in two joint publications by Gérard and Antoinette, and two short papers by Gérard and Antoinette in 1972 and 1973 described variations in the nuclear luminosity of the Seyfert galaxies NGC 3516 and 5548.

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THE HUBBLE CONSTANT AND EXTRAGALACTIC DISTANCES

Distances in the extragalactic universe will always be linked with the names Hubble and Sandage. Gérard de Vaucouleurs also became interested, particularly after spending time with Allan Sandage in Pasadena. The Hubble Constant, H_0, defines the relation between the luminosity of galaxies selected to have as similar properties as possible, as measures of their distances, and the redshifts measured in their spectra, produced by their recession velocities in the expansion of the Universe.

Sandage and Gustav Tammann, in a series of papers during the 1970s, had a value for H_0 of 57 (later 50) km/sec per megaparsec. But de Vaucouleurs favored a larger value, up to 100 km/sec per megaparsec. He worked on this between 1976 and 1987 and wrote numerous papers in various journals. He drew a figure that became famous for a few years: the Eiffel Tower in Paris with all the steps leading to determination of H_0 from the base (the Cepheid variable stars in nearby galaxies) to the far Universe. He called it "Une Construction Solide et Durable pour Atteindre H_0." Lucienne Gougenheim (at Meudon Observatory, France) and Brent Tully (University of Hawaii), commenting on the different results obtained by Sandage and Tammann and by de Vaucouleurs, expressed this disagreement as, "The Universe may be more complicated than the standard model would suppose, something that de Vaucouleurs has long suspected."

THE REFERENCE CATALOGUES

If one had to select one of his hundreds of publications that is used by astronomers worldwide and that always brings to mind Gérard de Vaucouleurs and his contributions to the study of galaxies, one might choose the *Third Reference*

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Catalogue of Bright Galaxies. This was the three-volume successor to the first and second catalogues, and was published in 1991, with coauthors in Texas and France and, of course, Antoinette. She was working on this when she heard the dreadful diagnosis of cancer of the bone marrow in October 1986. Although in pain, she was able to work on the catalogue until June 1987, just ten weeks before she died. During this period I used to receive phone calls from Gérard two or three times a week to tell me of her progress or lack of it and to ask about doctors and what might be newly discovered treatments and cures. It was a sad time. Gérard and Antoinette had no children.

SYMPOSIUM IN THE EIFFEL TOWER

There were some happy occasions near the end of Gérard's life. A special occasion in his honor was organized by his friends in Texas, to be held in a place of his choice—where else but the Eiffel Tower Restaurant in Paris! Following this he met an old friend in Paris, and they were married—Elysabeth, who survives him. He was elected to the National Academy of Sciences in 1986. But he had not much longer to devote to his lifelong love of astronomy—he died on October 7, 1995. His legacy to astronomy is demonstrated by this necessarily brief list of selected references; only a few of his more than 400 contributions are included here.

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Stanford University, Department of Reprographics

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PAUL JOHN FLORY

June 19, 1910–September 8, 1985

BY WILLIAM S. JOHNSON,[1] WALTER H. STOCKMAYER, AND HENRY TAUBE

PAUL J. FLORY, who received the 1974 Nobel Prize in chemistry, died unexpectedly of a heart attack on September 8, 1985, at his vacation home on a hilltop in Big Sur, California. The citation of the Nobel award reads: "For his fundamental achievements, both theoretical and experimental, in the physical chemistry of macromolecules." He occupied a towering position in the chemical community, and was noted not only for his outstanding leadership in macromolecular chemistry but also for his role as a passionate defender of human rights throughout the world.

The importance of his work was clearly recognized during his lifetime. Among the honors he received are four national awards of the American Chemical Society, five section awards of that society, ten honorary degrees, the National Medal of Science, and the Nobel Prize. His activities in the cause of human rights, especially after his Nobel award, were prodigious and universal. He was elected to the National Academy of Sciences in 1953.

[1]Deceased August 19, 1995.

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EARLY LIFE, EDUCATION, CAREER, AND FAMILY (BY WILLIAM S. JOHNSON)

Paul Flory was a warm and loyal friend to those people who, like himself, had high standards of integrity and were honestly modest about their own accomplishments and potential. These friends in turn greatly admired Paul. On the other hand, Paul was not everyone's friend. Indeed, he was not reluctant to show his disdain for those whose behavior suggested that they had exalted opinions of themselves, particularly if they were in dominating positions (e.g., administration) where they could influence the lives of others. Paul was a strong and vociferous champion of the oppressed in such situations and a fierce adversary of the offender.

Flory's puritanical principles could well have been derived from his background. The Flory family traces its roots back to Alsace, then England, later to Pennsylvania, and then to Ohio. Paul appeared to be especially proud of his Huguenot origin. His father, Ezra Flory, was a minister in the Church of the Brethren, a sect somewhat like the Quakers. The family moved frequently as he was appointed to different parishes. Ezra married Emma Brumbaugh, by whom he had two daughters, Margaret and Miriam. After Emma died in childbirth, Ezra married her cousin, Martha Brumbaugh, and they had two boys, James and Paul. The farmland outside of Dayton was given to the Florys by a Presidential grant and is still in the family.

Paul was rather frail as a child but was very precocious. He was always especially attached to his half-sister, Margaret, who was also his sixth-grade teacher. She recognized his potential, and was eager to have him further his education. As he matured Paul worked diligently on developing his physique through activities such as ditch digging, vigorous swimming, and mountain hiking. He became a strong

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man with great vitality, which he enjoyed for the better part of his life. He was always adamantly opposed to having regular physical checkups even when he began to be bothered by tiring while swimming, not very long before he died of a massive heart attack.

Although it was during the Great Depression, Paul managed to attend Manchester College in Indiana, graduating in three years and supporting himself by various jobs. It was at Manchester that his interest in science, particularly chemistry, was inspired by Professor Carl W. Holl, who encouraged Paul to enter graduate school at Ohio State University in 1931. During the early period at Ohio State he helped to support himself by digging ditches and working in the Kelvinator factory, and he first pursued a master's program in organic chemistry under Professor Cecil E. Boord. In his second year, having decided to opt for physical chemistry, he became laboratory assistant to his dissertation adviser, Professor Herrick L. Johnston, whom Paul described as "having boundless zeal for scientific research which made a lasting impression on his students." On the other hand, a fellow graduate student of that time has recalled that Johnston and Flory "did not see eye to eye."

Paul was a restless person and hardly ever was satisfied with the status quo. He was always looking for better places or conditions where his scientific interests and those of his colleagues could flourish. After graduate school he joined DuPont in 1934 and four years later, in 1938, he left to join the Basic Research Laboratory at the University of Cincinnati. The urgency of the development of synthetic rubber provoked by World War II brought him back to industrial research at the Esso Laboratories of the Standard Oil Development Company (1940-43) and then in the Research Laboratory of the Goodyear Tire Company (1943-48). In 1948 he accepted a professorship at Cornell University, where

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he was fairly content for nine years. Then in 1957 he was lured to the Mellon Institute in Pittsburgh to establish a broad program of basic research. Under his direction this enterprise thrived for several years until top management began to lose interest in the project. In 1961 he accepted a professorship at Stanford University, where he remained until his death in 1985.

Paul enjoyed a rich family life. In 1936 he married Emily Catherine Tabor, who was strongly supportive of all of her husband's activities. They had three children: Susan, who is now the wife of George S. Springer, a professor in the Department of Aeronautics and Astronautics at Stanford University; Melinda, whose husband, Donald E. Groom, is professor of physics at the University of Utah; and Dr. Paul John Flory, Jr., research associate in the Department of Human Genetics at the Yale University School of Medicine. There are five grandchildren in the family: Elizabeth Springer, Mary Springer, Susanna Groom, Jeremy Groom, and Charles Groom.

SCIENTIFIC WORK (BY WALTER H. STOCKMAYER)

Commencing in 1934 Flory dealt with most of the major problems in the physical chemistry of polymeric substances, among them the kinetics and mechanism of polymerization, molar mass distribution, solution thermodynamics and hydrodynamics, melt viscosity, glass formation, crystallization, chain conformation, rubberlike elasticity, and liquid crystals. The restricted bibliography presented at the end of this memoir necessarily cannot convey fully the content of his more than 300 publications.

The special characteristics of Flory's work were well stated by his longtime friend and collaborator Thomas G. Fox.

The secret of his success is unparalleled intuition for grasping the physical

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essentials of a problem, for visualizing a phenomenon in terms of simple models amenable to straightforward treatment and productive of results that are valid to the degree required by the original statement of the problem. Consequently, Flory's concepts and results are presented in a way that is instructive, understandable, and directly useful to the reader. This is equally true for those working in basic polymer science and those interested in industrial applications.

DUPONT AND CAROTHERS (1934-1938)

Flory was offered a position at DuPont during the height of the depression, when very few jobs were available in either industry or the academic world. He was especially fortunate in being assigned to work directly under the great Wallace H. Carothers, whose contributions to a firm grounding of the macromolecular concept rivaled those of Hermann Staudinger. Paul chose to investigate the simplest and most established reactions involving bifunctional reagents (e.g., esterification between ethylene glycol and succinic acid). It was becoming clear that such condensation polymers as produced would consist of chain molecules of different length, and the problem that Carothers set Flory was to develop a mathematical theory of this distribution. When this work was started, it was commonly supposed that the normal reactivity of a given kind of functional group would be suppressed if it were on a large molecule: Mere size per se was considered to impart a sluggishness that would bar unlimited chain growth. This was a conclusion based on the then prevalent collision theory of bimolecular chemical kinetics. Flory, in constructing a straightforward statistical treatment of the distribution problem, took the contrary view that reactivity under given conditions of solvent, temperature, pressure, and concentration is essentially a function only of local structure and not of overall molecular size. He argued that increasing size would indeed reduce translational mobility of a molecule, but that this would be compensated by

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increasing duration of each contact between reactants. Good experimental information was meager at that time, but in subsequent years he provided with his own hands much of the kinetic data that sustained his view. The resulting distribution formula could not be simpler: The number of chains with x links decreases exponentially with x. This "most probable distribution," as Flory called it, remains the norm that often describes actual polymeric products. When published in 1936, direct observations of chain length distributions were tedious and inaccurate, but today they are routinely observed by the methods of gel exclusion chromatography.

During his DuPont years Flory made another fundamental contribution to the understanding of polymerization reactions. In a paper reviewing the kinetics of olefin polymerization he pointed out the need for including the step known as chain transfer, whereby an actively growing chain molecule abstracts an atom from another molecule, transferring the seat of activity and ending its growth. The practical importance of chain transfer is in the control of many industrial polymerization processes, including those responsible for all the U.S. synthetic rubber of World War II. The chain transfer reaction is an essential part of most polymerization mechanisms. Shortly after the premature death of Carothers by suicide in 1937, Flory left DuPont and went to Cincinnati.

ACADEME I: CINCINNATI (1938-1940)

While continuing to accumulate experimental results on linear systems, Flory turned his attention to polyesters containing an ingredient bearing three or more functional groups, so-called "three-dimensional" polymers, containing branched structures. One example of this type was already a well-known commercial product, glyptal (made from *gly*cerol and *phthali*c anhydride), and it was well known that

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such systems attain a state of zero fluidity (the gel point) at a stage well short of complete reaction. Carothers had correctly concluded that this state indicated infinite molecular weight, with the chains forming a giant network; but he calculated from simple stoichiometry the *number* average molecular weight as the appropriate signal. In fact, the gel point is found to occur much earlier, when the number average molecular weight is still modest. Here Flory recognized that the branched polymers would have a size distribution much broader than that of linear polymers, and that the gel point corresponds to a diverging *weight* average molecular weight. In a series of three papers, characterized by mathematical sophistication far in advance of his previous work, he developed the quantitative theory of the gel point and of the entire molar mass distribution.

ESSO LABORATORIES (1940-1943)

The onset of World War II greatly increased the urgency of development of synthetic rubber and convinced Flory to return to industry. He nevertheless managed to produce some very fundamental results in macromolecular physical chemistry. With John Rehner, Jr., he developed a useful model of rubber networks and its application to the swelling phenomenon. In polyisobutylene solutions he personally measured viscosities over a very wide range of molecular weights, far greater than any earlier examples, and showed their strict adherence to the Mark-Houwink-Sakurada law with a fractional exponent of 0.64. Doubtless his outstanding achievement of those years was the development of the famous Flory-Huggins, or "volume fraction," formula for the entropy of mixing of polymer solutions. (This result was obtained essentially simultaneously by Maurice L. Huggins in the United States and by A. J. Staverman in Nazi-occupied Holland.) This now classic formula plays a role analo-

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gous to that of the van der Waals equation of state for real gases, because although approximate, it conveys the essential physics and leads to reliable qualitative predictions. It remains the norm to which real behavior is customarily compared. He later extended his treatment to polymer solutions of arbitrary complexity.

GOODYEAR RESEARCH LABORATORY (1943-1948)

In these years Flory's concerns with applied polymer science were at their height. He studied the tensile strength of elastomers in relation to network structural defects, and measured viscosities and glass temperatures of polymer melts. He also began work on the thermodynamics of polymer crystallization, a field that previously was not well defined. His theories predicted the dependence of the degree of crystallinity on temperature, molar mass, chain stiffness, chemical uniformity of the polymer, and elongation under a tensile force. From his equations one can determine the heat and entropy of fusion of the polymer and the thermodynamic interaction parameters with added diluent.

In the spring of 1948 Flory was invited to Cornell University to deliver the George Fisher Baker Non-Resident Lectures, and he found the atmosphere in Ithaca so congenial that he readily accepted an offer to join the faculty there.

ACADEME II: CORNELL (1948-1957)

During the Baker lectureship Flory had started to work on a major project that was finished only in 1953: the composition of his massive *Principles of Polymer Chemistry* (672 pages), which after almost half a century is still a greatly used text. No other single book has had such a great influence in an ever expanding field.

Also first conceived during the Baker year, one of his

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greatest achievements was speedily completed: a viable theory of the so-called excluded volume effect, accounting for the fact that real chain molecules have effective lateral dimensions and therefore cannot intersect themselves, and that furthermore their atoms experience van der Waals interactions with their close neighbors whether these belong to the same chain or to surrounding molecules. Proceeding beyond earlier incomplete discussions by Werner Kuhn, by Huggins, and by Robert Simha, Flory's "mean field" theory is still in extensive use today. Except in special circumstances (see below) the net effect of the volume exclusion and other interactions does not vanish. In a good solvent, chain molecules experience a net perturbation that increases without limit as the chain is lengthened, and the numerical relation between molecular weight and effective radius (the root-mean-square radius of gyration measurable by light scattering) deviates from the square-root law that must hold for flexible chains if all the interactions could be ignored. Flory's theory leads to a limiting exponent of 3/5 relating radius to molecular weight, which is not very far from the value 0.5887 yielded by the best modern theories.

Flory's result was not welcomed at the time by Debye and many other workers, for an "unperturbed" chain following the square-root law would precisely obey the laws of random flights already well understood in the theory of Brownian motion. However, he showed that very often there was a special temperature (called the "theta" temperature by Flory, but the "Flory temperature" by most others) at which the attractive and repulsive interactions would just cancel. This special state could be recognized (as in the analogous case of the Boyle temperature of an imperfect gas) by the vanishing of the osmotic second virial coefficient, also the subject of intensive study by Flory and Krigbaum.

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Flory next turned to an interpretation of polymer solution viscosity. Recognizing that the incomplete hydrodynamic shielding featured in the earlier theories of Kirkwood and of Debye could be neglected, he and Fox showed that the increase in viscosity produced by each chain molecule is proportional to the cube of its effective radius, as given by the excluded volume theory, and that the proportionality constant is essentially universal for all flexible chains in all solvents. There was thus made available an especially simple method for extracting, from a vast body of existing data, information about chain conformations, which became one of Flory's major preoccupations for the rest of his career. Soon after the viscosity breakthrough Flory with coworkers Mandelkern and Scheraga produced a similar treatment of sedimentation velocity in the ultracentrifuge and showed that from both measurements taken together one could extract the molecular weight of the polymer. For some years this method was much used by biochemists, as it required less sample than the other methods available at that time. Another pioneering effort of the Cornell years was the production, during a sabbatical term in Manchester, United Kingdom, of a theory for the thermodynamic properties of stiff chains, which Flory put to further use many years later in his work on liquid crystals. Also, his Goodyear work on polymer crystallization was applied to the phase behavior of fibrous proteins.

MELLON INSTITUTE (1957-1961)

Flory, having served on the Mellon Board of Trustees for several years, strongly urged the board to modify its long-standing program of industrial fellowships and to move heavily into basic research. The board's response was that Flory was just the man to lead this effort, and so he felt obliged to take up the offer, on condition that the institute's

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considerable financial resources would be firmly dedicated to this goal. After several years, however, the board had failed to follow through, and Flory decided to return to academic life.

ACADEME III: STANFORD (1961-1985)

The circumstances of Paul's move to Stanford are related by William S. Johnson in the next section. Continuing work started before the move and, with the special help of R. L. Jernigan and later Do Yoon, he developed powerful matrix methods for describing the conformations of chain molecules. He not only mastered the works of M. V. Volkenshtein (Soviet Union), K. Nagai (Japan), and S. Lifson (Israel) but also actually surpassed them and produced significant new results. These are embodied in his second book (1969), *Statistical Mechanics of Chain Molecules*, and applied to a great variety of polymers, including polypeptides and polynucleotides. Some examples are described in his 1974 Nobel lecture.

Flory also returned to one of his favorite topics: the thermodynamics of polymer solutions. The Flory-Huggins entropy was not abandoned, but much effort was expended on improving the details of the enthalpy of mixing. Compressibility and free-volume effects were introduced, called by Flory the "equation of state" terms. The treatment was also applied with considerable success to non-polymeric liquid mixtures.

Two other areas of earlier interest were also resumed. The theory of anistropic solutions, begun in his 1956 paper, was developed to deal also with mixtures of rigid and flexible chains. The theory of rubber networks, begun in 1943, has been greatly refined. An important source of information on the energetics of chain conformation is the temperature dependence of the elastic force in rubbery

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polymers, provided that the excluded volume effect can be neglected. Flory regarded this neglect as justified. In his own words: "Although a chain molecule in the bulk state interferes with itself, it has nothing to gain by expanding, for the decrease in interaction with itself would be compensated by increased interference with its neighbors." Many years after he made this statement, neutron-scattering studies at Grenoble and Julich confirmed it. By taking advantage of the big difference in neutron-scattering cross-sections between deuterium and hydrogen, it was directly shown that the mean dimensions of a number of different polymers in undiluted amorphous samples are identical to their "unperturbed" dimensions in dilute solution.

Questions concerning the morphology of semi-crystalline polymers have given rise to an extensive and thorny literature, and the principal matter at issue was not resolved during Flory's lifetime. When polymers crystallize from dilute solution in thin plates, single crystals can be observed, and it is found that the direction of the elongated chains is perpendicular to the lamellar plane. The chain length typically exceeds the lamellar thickness by a factor of 10 or more, so the chains must therefore fold back and forth many times. When polymers crystallize in the bulk, lamellar crystals also frequently form, and the question is whether the chains usually fold sharply at the crystal surface and reenter the lattice in an adjacent position, or whether they make larger loops in an amorphous region before finding reentry some distance away. This latter "telephone switchboard" model was strongly favored by Flory and Yoon, but the adjacent reentry model also had many strong and able supporters. It has turned out that an intermediate situation is needed to reconcile all the facts, with a figure of roughly 50 percent to 70 percent adjacent reentry taking place.

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PERSONAL RECOLLECTIONS (BY WILLIAM S. JOHNSON)

My first contact with Paul was in 1960, the year I moved to Stanford University as head of the chemistry department, where my main assignment was to recruit a number of distinguished scholars. In December I learned accidentally that Paul Flory was resigning his position at Mellon Institute and was interested in returning to academic life. I immediately contacted our provost, Fred Terman, and within 15 minutes had approval to make Flory an offer. When I phoned Paul, whom I had never met, he indicated that it was probably too late to become involved, since he was committed to reach a decision soon regarding three other academic offers. However, always interested in new ventures, he agreed to pay us a quick visit during a very rainy, windy weekend. Upon returning home he wrote characteristically as follows:

Dear Bill:

I want to thank you again for the opportunity afforded me over the weekend to become informed on the outlook for science in general, and chemistry in particular, at Stanford. The time was brief, but I feel we covered the area of preliminary discussions thoroughly and satisfactorily. I have great admiration for the course you are pursuing.

The opportunities for contributing to the physical chemistry program as you have outlined them are indeed challenging, and I beg permission to weigh them carefully and deliberately in relation to other proposals which I am seriously considering at the present time. You may expect to hear from me again around the first of January on whether or not the next step in our negotiations seem advisable at that time.

Expenses for the trip: Chicago, San Francisco, Pittsburgh, came to $298. I shall be glad to supply a breakdown if desired. Incidentally, any adverse prejudice which might have been engendered by the sample of California weather over the weekend was dispelled immediately upon my return to Pittsburgh in the midst of a raging blizzard. The plane was delayed on this

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account, driving to my home area was slow and hazardous and, to cap it all, I could not get up the hill a mile from my home. Taxis had mysteriously disappeared. Finally, I sought mercy from the local constabulary, who kindly took me home. The hour was late even on California time. When next I am brought home by police car, my wife will insist upon some other explanation.

My very best regards to Dr. and Mrs. Terman, to Dr. and Mrs. Mason, and especially to you and Barbara.

Sincerely,

Paul

Flory's acceptance had a profound effect on our program. Henry Taube (then at the University of Chicago), whom we had been trying to attract to Stanford for some time, presently decided in our favor. In a biographical memoir on Flory, Henry wrote,

Flory, with characteristic decisiveness, made up his mind before I did, and his decision made in 1961 to accept an offer from Stanford influenced my own. By then his scientific reputation was widely and firmly established, and by then I had met him and his wife Emily several times. All factors contributed as strong inducements to join him as a colleague.

At that time Flory and Taube were already widely recognized as truly distinguished scientists, and with their help it became relatively easy to attract top scholars like Gene van Tamelen from Wisconsin in 1962 and Harden McConnell from Caltech in 1964. These early appointments were responsible for the spectacular rise of Stanford's chemistry department from fifteenth position (in 1957) to fifth (in 1964) in the nation, according to the 1966 report of the American Council on Education, "An Assessment of Quality in Graduate Education." By 1968 all six of the new professorial appointments made since 1960 (note that Carl Djerassi was also among this group) were members of the National

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Academy of Sciences. Flory's coming to Stanford represented the critical mass for this explosive sequence of events.

In the summer of 1961 Paul and his family moved into a lovely house in Portola Valley with a magnificent view of the lower Bay Area and the Santa Clara Mountains. One of his first projects was the installation of an outdoor swimming pool, which he used regularly for the rest of his life. I saw a great deal of Paul in these early days, mainly because he was so very interested in the development of the new chemistry program. He was very generous with his time, and we frequently visited each other's offices. I enjoyed this relationship immensely, for even while dealing with serious matters, Paul's fine sense of humor would provide needed relief of tension. With the aim of taking full advantage of his administrative expertise, I established an Executive Committee (comprised of Flory, Associate Head Douglas Skoog, and me) to address such matters as departmental policy issues, salaries, promotions, and teaching loads. In this role he was indispensable.

Paul told me in the early summer of 1964 that he had been offered the Todd professorship (formerly held by Peter Debye) at Cornell University and that he was seriously interested. The Stanford honeymoon was over and Paul was lapsing into his normal state of moderate discontentment about the slow progress that was being made in the resolution of certain problems, in particular the lack of adequate building facilities for chemistry. In view of Flory's record of changing jobs frequently, Terman took the matter seriously and promised high priority for a new chemistry building. In addition, he quickly convinced a donor to establish the first endowed chair in chemistry and Paul was appointed the Jackson-Wood Professor of Chemistry at the September trustees' meeting. Despite these reassuring moves Paul came to my office on September 28, 1964, and announced apolo-

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getically that he had just about decided to accept the Cornell offer. His friends and colleagues throughout the university were apprised of the situation, and they rallied magnificently, with the result that he changed his mind.

As it turned out Flory remained at Stanford for the rest of his life. When I resigned the headship in 1969, the administration and the department agreed to change over to the more conventional system with a chairman serving a three-year term. Paul was the favorite for the first chairmanship, which he accepted for only two years, because as he argued, he had already served a year as acting head when I was on sabbatical leave in 1966-67. By the time Paul became chairman the Stanford administration was well on its way to a complete changeover to a highly democratic system. Both Sterling and Terman had retired, and Paul was reporting to an associate dean. His relentless campaign for new physical facilities continued, but it was not until 1974, just after it was announced that he was the recipient of the 1974 Nobel Prize in chemistry, that the Board of Trustees approved the expenditure of funds for a new chemistry building.

Flory's was the first Nobel Prize in chemistry at Stanford, and the day of the announcement was one of tremendous excitement and revelry at the department. Paul was not the sort of person whose ego was inflated by this honor. Nevertheless he was very pleased because the prominence and media interest that the Nobel laureate commanded afforded him the opportunity to be much more effective than before in his work on human rights issues.

Flory's strong commitment to and reputation as a relentless fighter for the human rights of oppressed scientists abroad is well known. This became one of his most important concerns during the last 10 years of his life. His efforts were strongly supported by Emily, who did background read-

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ing for him and accompanied him on visits to dissident scientists in East European countries. Various other activities included being interviewed a number of times on the Voice of America for broadcast to the Soviet Union and Eastern Europe. He served on various committees concerned with human rights, such as the Committee of Concerned Scientists, and he was highly critical of the National Academy of Sciences, the American Chemical Society, and other scientific societies for not taking a strong stand in defending the rights of scientists. In 1980 he was a member of the U. S. delegation to a 35-nation scientific forum in Hamburg, West Germany, that discussed scientific exchange and human rights under the Helsinki Accords. Flory was especially identified with the SOS as a founder, spokesman, and activist. This non-establishment group consisted of about 9,000 scientists throughout the world who voluntarily withdrew their scientific cooperation with the Soviets in response to the imprisonments of Sakharov, Orlov, and Shcharansky. This boycott surely was a most important factor in the relatively favorable developments that have taken place in the last few years. It is a pity that Paul did not live long enough to enjoy some of the recent fruits of his labors. The intensity of his devotion to the cause is illustrated by his offer to the Soviet Union to be a hostage, guaranteeing the "good" behavior of Sakharov's wife, Yelena Bonner, if she would be allowed to leave the country for badly needed medical treatment.

Even though he had won just about every major award available to a scientist in his field, he still needed reassurance that his colleagues appreciated him. It is too bad that the department waited until 1984 to establish the Flory Lectureship in his honor, because this pleased him very much. Paul delivered the first lecture, which was followed by a dinner celebration that attracted a huge number of his former

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collaborators, colleagues, and other friends. Jean-Marie Lehn gave the second lecture in January 1985, but Paul could not attend because urgent matters (see above) called him to Europe. Up until the end Paul was a human dynamo that ran unflaggingly with great efficiency and high output. Becoming emeritus in 1975 had no effect on his activities; indeed, it was about that time that he became heavily involved in his human rights activities, all in addition to his scientific work at IBM as well as Stanford and consulting for industries that he helped establish.

Paul did have his periods of tranquility. He was a delightful host, seemingly completely relaxed, and he obviously greatly enjoyed entertaining his friends. Exercise was Paul's major tranquilizer. After a vigorous swim he would emerge from his pool with a broad smile on his face and an obvious feeling of well-being. Another of his great pleasures was hiking in the mountains. He and Emily were apparently tireless, and completely at home on the trails. They had a splendid collection of maps, which they were very familiar with and they felt free to go almost anywhere. Neither of them ever did quite understand Barbara's and my concern for their safety during an experience with them in Yosemite, where Paul and Emily ended up on a steep, unfamiliar trail well after dark. Paul's pleasure in this environment was almost euphoric. He relished being close to nature and, although a newcomer to the area, he proved to be extraordinarily well informed about the plant and animal life of the vicinity. On another occasion in the early days we hiked with the Florys at Big Sur when they were beginning to fall in love with the area. Eventually Paul bought property there and built a small house, accessible only via dirt roads at a high elevation. It was here that Paul escaped whenever he could to write uninterruptedly, enjoying the isolation with a telephone, hiking, clearing trails, and chop-

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ping his own wood. It was here that he died suddenly on September 8, 1985, of a heart attack, while he was getting ready to return to Portola Valley.

PERSONAL RECOLLECTIONS (BY WALTER H. STOCKMAYER)

My first meeting with Flory came some time in 1942, while he was at the Esso Laboratories and I was at Columbia University. After hearing Tom Fox, then a graduate student, describe Flory's recent theories on gelation of multifunctional systems, I began to switch my own interests to polymer problems and succeeded in developing alternative methods to Flory's approach. When I wrote to Flory about this, he invited me to visit him and encouraged me to further my work and continue along such lines. Although we never worked in close proximity, he and I kept in fairly close touch by letters or telephone for the rest of his life. I recall particularly several years before his death when he took a whole day out of his busy life to drive me in his Jeep on the long trip from Portola Valley to his vacation house on top of a hill in Big Sur.

An earnest of our friendship was his relatively benign reaction to the few times we disagreed on scientific matters. The first of these dealt with the description of three-dimensional polymers after their critical gel point is passed: His treatment permitted cyclic structures in such networks, while mine forbade them strictly at all stages of reaction. I now know that his result was physically far superior, but it involved a somewhat arbitrary step missing from my perhaps more rigorous but physically less plausible mathematics. A second disagreement came many years later when Kurata and I neglected the conformational consequences of the so-called "pentane effect" between adjacent internal rotations in certain polymer chains. Here we were dead wrong, and Flory of course was right. In both these instances Flory

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never criticized me in print. As has already been said, frequently he did not hesitate to point out such disagreements with others in strong language. In my case, however, he didn't do that; he simply ignored them and omitted all mention of them in his writings.

Finally, I was always impressed by Flory's ever increasing command of formal mathematics. Recall that at Ohio State he had to take remedial math courses and study on his own to make up for his relatively meager background from Manchester College. Yet he continued to develop what was needed, even relatively late in a theoretician's career.

PERSONAL RECOLLECTIONS (BY HENRY TAUBE)

I first saw Flory in person when I was a member of the audience in the chemistry department at Cornell where he appeared as a seminar speaker, probably around 1944. I retain a vivid recollection of his talk, and look back on it as one of the best and most instructive scientific lectures I have heard. It was mainly based on his paper "Thermodynamics of High Polymer Solutions," and in the course of his presentation the power and incisiveness of his intellect, qualities that in part account for his preeminence as a scientist, were made manifest. He had an extraordinary capacity to penetrate to the heart of a scientific problem and to isolate the essential features of even complex systems, making them amenable to rigorous mathematical analysis. I still remember my feeling of exhilaration at the end of the seminar, thinking to myself, "Flory can make scientific sense even out of glue."

I did not meet Flory on the occasion of this seminar, but I did get to know him rather well in our time together at Stanford, which began with overlapping visits in the recruiting phase of our association with this institution. For most of his time at Stanford we shared space on one floor

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of a chemistry building, and our offices were separated only by space shared by our secretaries. Thus, except when either of us was out of town, I saw him almost daily. One strong impression I have is that he worked hard and never seemed to be idle. He spent most of his time in his office, where he developed theory, wrote papers, and dealt with correspondence; apart from this he was usually to be found in the laboratory, talking to his research collaborators.

Despite our physical propinquity and what I believe was a mutual regard and despite the fact that I liked his company, our relationship did not ripen to a relaxed, intimate level. Nevertheless, even through accidental and casual contacts I did learn a great deal about him, and my own impressions of him as a person confirm the laudatory statements that have appeared in earlier memoirs. Paul had a very good sense of humor and often the subject of our conversation would be an anecdote of his that he would relate with great gusto. His own enjoyment of the humor was expressed by a warm, ready smile that brightened an already handsome face, and often by a hearty chuckle. He was a kind and caring man, and his concern for the welfare of others was translated into action. After being awarded the Nobel Prize the tempo of his activities in the cause of human rights increased, and he used the added prestige to try to ameliorate the condition of Soviet scientists who for reasons of conscience had run afoul of the authorities. He involved himself in this cause with the same kind of passion and devotion that he brought to his science throughout his career.

He was of strong character, of high integrity, and his convictions on important issues ran deep and were unwavering. Because of the depth of his feelings he could be severely critical of others who did not agree with him, even on matters that according to my opinion, those of good will

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might reasonably hold opposing views. His convictions could run deep even on less important matters and he frequently resorted to expressing them and his disagreement with others in writing. He wrote with passion and flair, and the resulting prose was forceful, even in the versions made public after Emily had the opportunity to edit the originals.

For a short time, while Flory was still on active duty, I was chairman of the department and in the course of discharging these duties he revealed a facet of his personality that would likely not have come to light in our casual contacts. I was astonished to learn that he had no appreciation of the very high esteem in which he was held by his colleagues. In fact, on one occasion he remarked that he felt that his colleagues were not particularly supportive of him. That this kind of misapprehension could persist is ascribable to what I believe to be the case, namely that his circle of intimate friends did not include many departmental colleagues. The origin of it may be that despite his record of distinguished achievement and though all of his actions spelled strength and forcefulness, there was a residue of insecurity in his make-up. Another insight that was revealed to me in the official contacts we had while I was chairman bears on this. Having still retained a vivid recollection of the early seminar of his I had heard, it came as something of a surprise to learn that Paul did not particularly enjoy teaching in a classroom setting. The reports are that in formal courses his lectures tended to be dry. I doubt that he had any interest in trying to make his lectures entertaining, and I believe that he saw no need to do so, sharing with many of us the view that the subject itself speaks to the receptive. At any rate I know that he was often unhappy with the student response to his courses. This helps to explain why Paul, who was a vocal and strong advocate of bringing more of the science of polymeric materials into

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the core curriculum in chemistry, failed to respond to invitations to offer concrete proposals on how this might best be done in our department. The responsibility of implementing any proposals that were adopted would likely have devolved on him and would have interfered with activities with which he felt more comfortable.

Throughout his life he enjoyed his work, and he greatly enjoyed and was proud of his family. He enjoyed nature. He had physical stamina and did not shrink from physical exertion. He led a good full life, and I doubt that he was ever bored. His name is boldly inscribed in the annals of science, and he will be remembered by succeeding generations. Those of us who knew him personally remember him in a different way. By the force of his personality this remarkable man made such an impression that we feel he is still among us.

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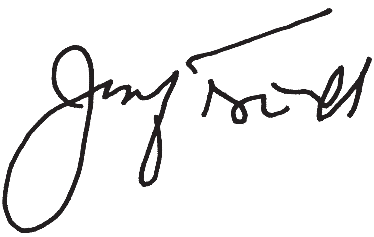

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JOSEF FRIED

July 21, 1914–August 17, 2001

BY NELSON J. LEONARD AND ELKAN BLOUT

JOSEF FRIED WAS AN outstanding organic chemist who made very special contributions to the field of medicine. "Gus" Fried was one of the few scientists, but now increasing in number, who have had outstanding careers in both an industrial organization and an academic environment. In his graduate and postgraduate study at Columbia University he concentrated on methods of synthesis of cardiac aglycones. Following Columbia he had two industrial positions, the second one being at the Squibb Institute for Medical Research. In this friendly scientific setting he found, in his research on corticoids, that fluorine substitution at the 9 position of hydrocortisone increased its anti-inflammatory potency. This finding upset the belief that the activity of the natural material could not be enhanced. He also discovered that 16, 17-acetonide corticoids increased the potency while eliminating the salt-retaining effect. This discovery led to the commercialization of the first superpotent anti-inflammatory steroids. The fluorosteroids have revolutionized the treatment of many endocrine and skin disorders.

After 20 years at Squibb, Josef Fried moved to the University of Chicago as a professor in the Ben May Laboratory for Cancer Research and then in the departments of biochemistry and chemistry, where his research expanded

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broadly in the area of natural products having biological activity. Within the class of human hormones known as the prostaglandins he devised new total syntheses of all the natural hormones and made analogs, some of them fluoro-substituted, that showed selective inhibitory action at specific prostaglandin receptors. He enjoyed close collaboration with professors in the medical school of the University of Chicago.

Josef Fried was born in the town of Przemsyl, Poland, on July 21, 1914, the first son of Abraham and Frieda Fried. The family moved to Leipzig, Germany, in 1919, where Josef received his education from elementary school through high school (1921-34). A second son, John, was born into the family in 1929. Josef developed an early interest in science, particularly chemistry. He also learned to play the violin, and he enjoyed listening to the classical church music that had abounded in Leipzig from the time of Bach. He received a thorough grounding in chemistry at the University of Leipzig during 1934-37, but he was in danger of not completing his bachelor's degree because of the 1935 Nuremberg Laws. By intrepidity and optimism he solved the problem. He went to the *Gauleiter* of the district, whom he convinced that the decree excluding German Jews from universities did not apply to Polish nationals. The combination of optimism with a willingness to confront and an ability to surmount difficulties was a special aspect of Josef Fried's character. In 1937 it enabled him to leave for Switzerland, where he spent a year at the University of Zürich with Professor Paul Karrer.

Professor Karrer, who was a sympathetic teacher, recommended, in the light of the political situation in Europe, that Josef should work toward his Ph.D. degree in the United States. Presumably he suggested Columbia University as one appropriate venue and Professor Robert C. Elderfield as a mentor. Josef entered Columbia as a graduate student in 1938, was awarded a Ph.D. in 1940 for his research on cardiac

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glycosides with Elderfield, and stayed on as an Eli Lilly postdoctoral fellow until 1943.

Josef's parents and brother, John, left Germany for Switzerland one week before the Second World War began. It had been the family habit, after vacationing in Lugano each summer, to obtain re-entry visas to Switzerland before leaving. In August of 1939 this foresightedness saved their lives. While the three of them remained in Switzerland during 1939-41, through further foresightedness, Josef was able to help support them by the sale of his set of *Beilstein* and some Leica cameras that had accompanied him to America. Abraham, Frieda, and John Fried managed to come to the United States in 1942. Josef married Erna Werner in the United States in 1939. She had been a children's nurse, was well known to the family, and had reached the United States also by the Swiss route.

COLUMBIA UNIVERSITY

When we two authors were in our second year of Ph.D. study at Columbia University, Josef was in his final year of research for the degree. He had been renamed "Gus" because Elderfield, despite his minor in German at Williams College, did not like to pronounce "Josef." It was not quick enough! From that time onward he has been "Gus" to friends, colleagues, and even family. Gus was most generous with his practical chemical advice to fellow graduate students and also when he was a postdoctoral fellow. He taught us all how to do column chromatography, which had not yet been adopted broadly in U.S. universities; how to induce almost any solid to crystallize; how to recrystallize rapidly using centrifugation rather than filtration in sequential operations; and how to keep up with the literature. His enthusiasm for organic chemistry was unlimited and infectious. Where Elderfield lacked, or was too busy for, hands-on advice,

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Gus supplied it. Outstanding among Gus's qualities was his ability to listen carefully and then to give cogent advice that was wide-ranging, whether about a particular laboratory technique or on a decision as to what courses we should take. If we required an answer as to method or conditions of organic synthesis, we went first of all to Gus Fried. Usually, the answer was so helpful and correct that he had solved our problem. We appreciated that Gus had an encyclopedic knowledge of organic chemistry and wanted to share it with students and colleagues. He made himself available to all of us, and he became a coauthor on the papers of six of Elderfield's Ph.D. students, including a 1942 publication with William S. Knowles, one of the three 2001 Nobel laureates in chemistry.

Gus and Erna were enthusiastic attendees at chamber music concerts in New York City. Regular Sunday afternoon visits to the Museum of Modern Art provided a balance for the week's chemical work in the laboratory. Erna's delicious dinners were frequently offered to those of us who were single graduate students. Strong and lasting friendships were established during those graduate years. Gus and we two authors have enjoyed steady contact with each other over a very long span of years, enthusiastic about each other's successes and supportive when there were difficulties—great, great friends.

One of the early difficulties that Gus Fried encountered was obtaining a suitable position to follow his postdoctoral fellowship at Columbia University. He was interested in teaching and had already shown himself to be effective in directing graduate research as Professor Elderfield's assistant. However, at that time no teaching position was available to him. He interviewed at several pharmaceutical companies but had no success there either. We have concluded that the anti-Semitism prevalent at the time in both academic

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and industrial settings was to blame. Gus was aware of prejudices that existed, but he never seemed unduly bothered by them. He accepted life in a way that others would have found unacceptable then or now. He never allowed difficulties to dampen his optimism and enthusiasm. He settled for a job at the Givaudan Research Institute of Givaudan-Delawanna, guided in part by the Swiss connection. His hope for involvement in pharmaceutical research, however, was maintained. In 1944, when Oskar Wintersteiner, who had recently been appointed director of research at the Squibb Institute for Medical Research, offered Gus the position of head of the antibiotics and steroids department, Gus eagerly accepted.

SQUIBB INSTITUTE FOR MEDICAL RESEARCH

What attracted Gus was Wintersteiner's description of the long-range basic programs and his trust that these would prove highly profitable to Squibb in due course. No less attractive to Gus were Wintersteiner's personality and scientific accomplishments. The two had met briefly at Columbia University when Wintersteiner was on the staff of the College of Physicians and Surgeons, and both shared an interest in steroid hormones. A bond between them was established readily because of Wintersteiner's Austrian background, his culture, and his style of doing chemistry, in addition to their common interest in music. Wintersteiner played the piano and organ, and Gus the violin. Gus spoke of his new boss as "a highly civilized human being, a man of great modesty and integrity." Squibb research was concentrated initially on the isolation of new antibiotics. Fried and Wintersteiner collaborated on streptomycin and related compounds. A side effect of this research was their successful chromatographic technique for the crystallization of sugar components, which constituted a vast improvement over

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earlier methodology that included a long residence time of a saturated carbohydrate solution in the refrigerator or the mythical "seeding" process of stroking one's beard or mustache above the solution containing the sugar. Another research project was centered on the active compounds responsible for the powerful hypotensive effects of the roots and rhizomes of *Veratrum viride*, a chemical investigation carried out in close association with biological and clinical assays. The dramatic results of the published clinical trial of cortisone in rheumatoid arthritis at the Mayo Clinic guided the Squibb research in the direction of fermentation of natural steroidal products that would provide the necessary ring substitution in a readily available intermediate for the economical synthesis of cortisone and cortisol. Indeed, Gus was tremendously successful in this project. The discovery of the effect of 9α-fluoro substitution (for hydrogen) was the fortuitous result of his adjusting the stereochemistry at the 11-position of the steroid ring system. He produced 9α-fluorohydrocortisone that showed a 10-fold increase in potency due to the fluoro substituent. Gus Fried's patent application of 1954 survived all interferences and was issued in 1958 (U.S. Patent 2,852,511) with all generic claims intact. It was one of several hundred patents that he would author or coauthor in his lifetime of research. Ironically Squibb management dissuaded Gus from spending time making materials for clinical studies; nevertheless, Gus and his capable assistant Emily Sabo quietly continued to do just that, with very good effect.

By further modification of the steroid nucleus with a 16, 17-acetonide grouping and a 1,2-double bond, Gus synthesized Squibb's Kenalog AE, the first of the superpotent anti-inflammatory steroids, with 100 times the activity of cortisol. It was the prototype of some of the most potent and effective corticoids used in dermatology today. During the years

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1959-63 Gus was director of the Division of Organic Chemistry at Squibb. During the Squibb years Gus and Erna lived in Princeton, New Jersey, and vacationed on Nantucket. There were regular weekly sessions of string quartet playing at their Princeton home, and there was sailing in Nantucket waters. Daughter Carol, born in 1946, attended Barnard College and became a teacher in a private school. Gus's brother, John, 15 years his junior, says that Gus's excitement and enthusiasm for chemical research and his invention of important hormonal therapeutics led John to shift from engineering to a career in medicinal chemistry. When they were both working for pharmaceutical companies in New Jersey, they finally had the opportunity to come together for family visiting and the discussion of new developments in medical science.

UNIVERSITY OF CHICAGO

During his time at Squibb Gus never lost interest in academic work. When Charles B. Huggins, who liked Gus and appreciated his accomplishments, invited Gus to become a professor in the Ben May Laboratory for Cancer Research in 1963, he was willing to switch from an industrial to an academic position. Professorial appointments followed in the Department of Biochemistry and the Department of Chemistry of the University of Chicago, the Louis Block professorship in the biological sciences division in 1973, and service as chairman of the Department of Chemistry during 1977-79. The Chicago situation was ideal because of Gus's concentration on natural products having biological activities. The medical applications were now expedited and complemented by his direction of young research colleagues in whom he showed great personal interest. Moreover, he could participate actively in collaboration with colleagues in the medical school of the University.

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He concluded his studies of steroids, especially their chemical and enzymatic interconversions, with a novel and rapid procedure, by the use of the steroid dehydrogenase enzyme from *Arthrobacter simplex*, for indicating relative and absolute configurations at a newly generated center during a total synthesis. This method, coupled with ORD or CD measurements, accomplished what had been possible only by X-ray crystallography prior to 1970. His seminal patent on fluorocorticoids in 1958 received recognition 10 years later with an Outstanding Patent Award from the New Jersey Council of Research and Development.

In an expansion of research interests Gus Fried and Dorothy Schumm penetrated the question of the cause of the carcinogenic activity of polycyclic aromatic hydrocarbons as due to one electron transfer oxidation, using the potent carcinogen 7,12-dimetylbenz[*a*]antracene (DMBA) as a model, with one-electron oxidizing agents. Prior studies had focused on the intervention of metabolites, all of which had lower carcinogenic activity than the original hydrocarbon. In collaboration with colleagues Bruce H. Wainer and Frank W. Fitch in the Department of Pathology and Richard M. Rothberg in the Department of Pediatrics, Gus Fried made and studied morphine antagonists. The immunological studies were extended to antibodies against the opioids codeine and hydromorphone and further to antibodies against the narcotic drug meperidine. The antiserum for the latter could be used in a radioimmunoassay for meperidine that was 100 times more sensitive than assay techniques employed before 1976. It could be employed in clinical practice to measure the clearance from serum and placental transfer of meperidine administered to women in labor.

Guided likewise by biological activity and clinical potentiality Gus turned his attention to the maytansinoids, a group of structurally related ansa macrolides with reported high

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antileukemic potency and cytotoxicity. He contributed importantly to all phases of our knowledge about these compounds. It was during this investigation that William Elliott, an M.D. Ph.D. student in Gus's laboratory, recognized he had an entrance from the maytansine research into another field of natural products, namely, insect pheromones. While Gus was off on a scientific visit to mainland China, Bill Ellliott performed the two synthetic steps that would lead him from one field to the other in a stereocontrolled, efficient synthesis of α-multistriatin, one of the essential components of the aggregation pheromone of the European elm bark beetle. The success of the synthesis was evident when the windows of the laboratory became black with beetles, as Bill relates, and he could greet Gus with the important though diverted research results. Gus was intrigued with the results, and because the racemate had first been synthesized, it remained only to sort out which of the enantiomers was the active one. The diseased elms to which beetle attractant was supplied in a receptacle at the base are still standing on the Chicago campus. Ironically, it was deemed less labor intensive and costly to cut down diseased trees than to service them individually with the synthetic pheromone and then destroy the congregated beetles.

The determination of structure and the total synthesis of the prostaglandins, a class of human hormones with a wide range of biological activity, had been pioneered by Professor E. J. Corey of Harvard University. At the University of Chicago Gus Fried added research on the synthesis of analogs and their chemical and enzymatic conversions, and on the synthesis of prostaglandin antagonists containing a CF_2 group in place of a CH_2 group within their structures. This was an analogical extension of Gus's research in the fluorocorticoid series. Increased stabilization and greater hormonal activity were twin goals achieved in the prostaglan-

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din series as well. He found both agonists and antagonists of the natural prostaglandins and thromboxanes. These derivatives have helped to clarify the biological function of both classes, members of the arachidonic acid cascade in human metabolism.

Gus Fried was elected a member of the National Academy of Sciences in 1971 and a fellow of the American Academy of Arts and Sciences in 1981. The American Chemical Society selected him for the Medicinal Chemistry Award in 1974 and the Alfred Burger Award in Medicinal Chemistry in 1996. In 1994 he received the Gregory Pincus Medal from the Worcester Foundation for Experimental Biology and the Roussel Prize from the Roussel Scientific Institute in Paris. As an executive member of the Council of the International Organization for Chemical Development, he was an active researcher on the development of the elusive male contraceptive.

To honor Josef Fried for his major contributions to the pharmaceutical industry and to the development of fundamental organic chemistry, Bristol-Myers Squibb and the University of Chicago launched in 1990 the first of a series of annual Josef Fried Symposia of Bioorganic Chemistry.

Recently many colleagues and friends spoke with admiration and warmth about Gus. Some of their statements follow.

•I recall the twinkle in his eyes and the valuable administrative advice that he provided.

•He was a skilled mentor.

•The Frieds' hospitality in the Chicago apartment and at their summer and weekend cottage on Lake Michigan was greatly appreciated.

•He had a major effect in changing the University of Chicago's patent policy.

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•He never lost his enthusiasm for science and was always interested in national and international events, focusing on causes of social fairness and justice.

•His advice on chemical and biochemical problems was given in a spirited way that betrayed an excess of enthusiasm for the science.

•He was a warm and generous gentleman in the best sense of that word.

•He had an inveterate optimism about all things scientific and a pervasive understanding of chemistry.

Professor E. J. Corey (Nobel laureate, 1990) says of Gus: "He was an outstanding, highly creative scientist who straddled both the worlds of pharmaceutical research and academic science. He was one of my heroes, and I've always thought of him as a model scientist of great character and great human warmth."

WE ARE MOST grateful to John Fried and Bill Elliott for the information they provided. We were also guided by the material that Gus himself placed on file in the Office of the Home Secretary of the National Academy of Sciences and for his personal account of the discovery of the fluorosteroids at Squibb that appeared in *Steroids* (1992).

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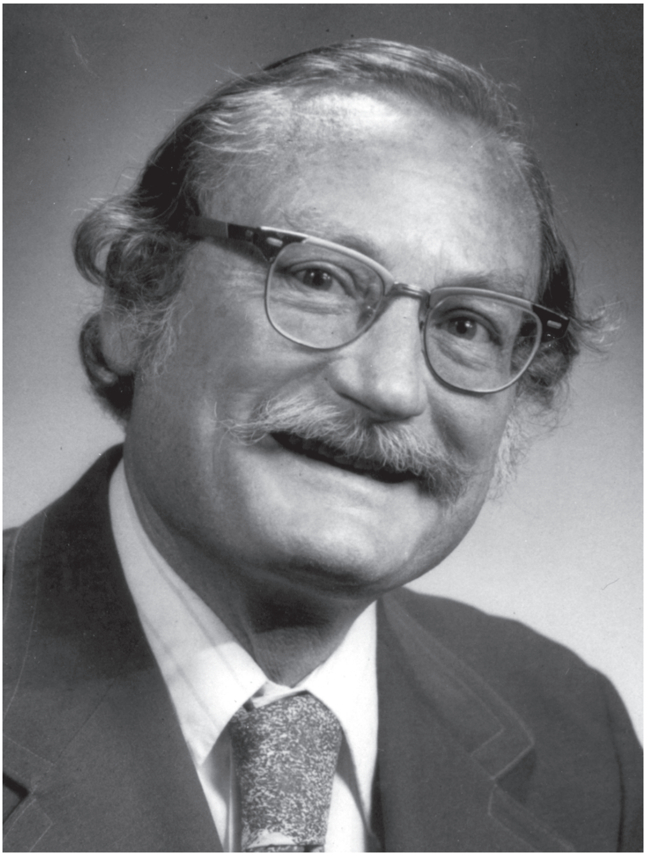

Jack R. Harlan

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JACK RODNEY HARLAN

June 7, 1917–August 26, 1998

BY THEODORE HYMOWITZ

HARLAN WAS BEST KNOWN for his contributions to knowledge of the evolution of crop plants, his plant explorations and archeological excavations, and for his clear elucidation of the interdependence of plants and civilization. Jack Harlan was a botanist, an agronomist, an anthropologist, a historian, and a scholar. He spent most of his academic career as a faculty member in departments of agronomy. However, he never took a formal course in agronomy.

Jack Rodney Harlan was born on June 7, 1917, in Washington, D.C. He was the younger of two sons of Harry Vaughn and Augusta Griffing Harlan. He earned a B.S. degree (with distinction) from George Washington University, Washington, D.C., in 1938 and his Ph.D. in genetics from the University of California in 1942. He was the first graduate student to complete a Ph.D. under the guidance of G. Ledyard Stebbins. On August 4, 1939, he and Jean Yocum were married in Berkeley, California. They had four children: Sue, Harry, Sherry, and Richard. After 43 years of marriage, Mrs. Harlan passed away on October 11, 1982, in Urbana, Illinois.

Jack Harlan was greatly influenced in his choice of career by the professional activities of his father. From 1910 to 1944 Harry V. Harlan was the leader of barley investigations

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for the U.S. Department of Agriculture, Washington, D.C., as well as a plant explorer. He collected barley in South America, Asia, Europe, and Africa. During the summer months he took his young sons, Bill and Jack, to the barley stations in Aberdeen, Idaho, and Sacaton, Arizona. In Sacaton he often led his sons on digging expeditions for artifacts at American Indian sites. In addition, his tales of adventure, of eating different foods, and of living with differing cultures, must have influenced young Jack. His father loved to entertain visitors from all over the world. For example, during the sixth international congress of genetics that took place in Washington, D.C., in 1932, teenager Jack Harlan met the great Russian agronomist, N. I. Vavilov. After receiving his B.S. degree Jack Harlan planned to study with Vavilov in St. Petersburg; however, the imprisonment and subsequent death of Vavilov put an end to Jack's plans.

After receiving his Ph.D. Jack Harlan was employed for a brief period by the Tela Rail Road Company in Honduras as a research assistant. Later, in 1942, his professional career began with the U.S. Department of Agriculture (USDA) at Woodward, Oklahoma, where he directed the Oklahoma Forage Crop and Rangeland Improvement program and the Southern Great Plains Regional Grass Breeding program. In 1951, while still with the USDA, he transferred to Oklahoma State University, Stillwater. While holding a joint appointment as professor of genetics at Oklahoma State, he began teaching on a limited basis and became involved with graduate students. In alternate years he taught classes concerned with classical evolution and evolutionary mechanics. It was during this period that he developed his philosophy concerning the evolution of crop plants and civilization.

To further develop his academic interests Jack Harlan resigned from the USDA in 1961 and joined the faculty of Oklahoma State University on a full-time basis. In the mid-

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1960s he refused to sign an Oklahoma State University faculty loyalty oath developed by and for the university's administrators, demonstrating his fierce desire for independence and his loyalty to the concept of academic freedom. In 1966 Jack Harlan moved to the University of Illinois, where he became professor of plant genetics in the Department of Agronomy. With Professor J. M. deWet, a colleague from Oklahoma State and then at the University of Illinois, he founded the internationally known and respected Crop Evolution Laboratory a year later. Opportunities were established for graduate study in such fields as chemical taxonomy (now called molecular systematics), numerical taxonomy, cytotaxonomy, cytogenetics, genetics, archaeobotany, and ethnobotany concerning cultivated plants and their wild relatives. In 1984 he retired from the University of Illinois with the rank of professor emeritus. Because of his wife's death in 1982 and other factors, Jack Harlan decided to relocate to New Orleans to be near his sons. In New Orleans he became an adjunct professor at Tulane University.

During his professional career Jack Harlan received many honors and awards. He was a member of Phi Beta Kappa, Phi Kappa Phi, and Sigma Xi. Harlan was awarded a John Simon Guggenheim Memorial Fellowship (1959), the American Grassland Council Merit Award (1962), the Frank N. Meyer Memorial Medal (1971), Crop Science Award (1971), and the International Service in Agronomy Award (1976). He received the 1985 Distinguished Economic Botanist Award from the Society for Economic Botany. He was a fellow of the American Association for the Advancement of Science (1956), American Society of Agronomy (1962), Crop Science Society of America (1985), and the American Academy of Arts and Sciences (1975). In 1972 he was elected to membership in the National Academy of Sciences. He served as president of the Crop Science Society of America in 1965-66.

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In addition he received a medal for service to the U.N. Food and Agriculture Organization and the International Board for Plant Genetic Resources and a medal at the N. I. Vavilov Centennial Celebration. In May 1997 the Harlan Symposium was held at the International Center for Agricultural Research in the Dry Areas (ICARDA), Aleppo, Syria. It was titled "The Origins of Agriculture and Domestication of Crop Plants in the Near East." Unfortunately he was too ill to attend. In November 1999 at the Crop Sciences Society of America annual meeting in Salt Lake City, a symposium was held in Jack Harlan's honor. Many of his former students and colleagues recalled with fondness their personal experiences with their mentor and friend.

In the field, Jack Harlan explored for and introduced plants from Africa, Asia, Central America, South America, and Australia into the United States. In 1948 he led a USDA-sponsored plant exploration trip to Turkey, Syria, Lebanon, and Iraq. In 1960 he led a USDA-sponsored plant exploration trip to Iran, Afghanistan, Pakistan, India, and Ethiopia. He was a consultant to the U.N. Food and Agriculture Organization in 1970-71 and a member of the International Board of Plant Genetic Resources from 1974 to 1979. In 1974 he was selected to be a member of the first team of U.S. agricultural scientists to visit the People's Republic of China.

Jack Harlan participated in several archeological digs. From 1960 to 1963 he was a senior staff member for the Iranian Prehistoric project, Oriental Institute, University of Chicago, and the Turkish project in 1964. He was a member of the Dead Sea Archeological project in 1977, 1979, and 1983.

Jack Harlan was an excellent speaker and coupled a strong grasp of the English language with a remarkably dry sense of humor. One of his last public lectures at the

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University of Illinois was entitled "Lettuce and the Sycamore: Sex and Romance in Ancient Egypt." The title was so intriguing that the lecture attracted a huge audience. In addition to being a Sigma Xi chapter lecturer, W. E. Key lecturer in genetics, visiting scientist of the American Society of Agronomy, he lectured at many major institutions of higher education in North America, Europe, Asia, and Africa. He was a visiting professor, University of California, Davis (1975), University of California, Riverside (1976), and the University of Nagoya, Japan (1979). Even in retirement he received requests to present lectures at symposia, conferences, and at individual institutions.

Harlan was interested in music, art, history, sailing, languages, birds, museums, and libraries. He believed that an individual's education should not end with a Ph.D.; rather education should be a continual process throughout one's life. His contributions to science cover the broad areas of agronomy, botany, genetics, anthropology, archeology, and history. These scientific contributions are summarized below.

Grassland Breeding. At Woodward and then Stillwater, Oklahoma, his research focused on the development of rangeland grasses for re-vegetating the southern Great Plains. A number of improved grassland cultivars were developed, tested, and released. Among the many releases were "Woodward" sand bluestem, "Southland" bromegrass, "Caddo" switchgrass, and "Coronado" side oats grama.

Compilospecies Concepts. At Stillwater Harlan established a biosystematics laboratory to study three grass genera, *Bothriochloa, Dicanthium,* and *Capillipedium.* The three genera form a polyploid, largely agamic complex. Of particular interest was the species *B. intermedia*, "a hodgepodge of germplasm assembled from at least 5 species belonging to three genera." Furthermore, "*B. intermedia* seems to have

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genetically consumed its own ancestral form." A compilospecies is therefore a genetic, aggressive plunderer incorporating germplasm of related species and hence able to expand its range.

Cynodon and Sorghum. Harlan and his associates conducted biosystematic analyses of both *Cynodon* and *Sorghum.* These research activities led to revisions in both genera. The revised classifications were based on evidence from morphology, geographical distribution, field observations, collections, and cytogenetics. In the case of *Cynodon* he sided with the splitter faction among taxonomists. For example *C. dactylon* was divided into six taxonomic varieties. On the other hand, with the cultivated sorghums he sided with the lumper faction among taxonomists. The sorghums were lumped together into one species having five basic races: bicolor, guinea, caudatum, kafir, and durra.

Weeds. In his plant collecting trips Harlan was impressed with the association of weeds and cultivated crops. In the Middle East it was wheat and the associated diploid species of weeds; in Africa there were cultivated and weedy races of sorghum; in Asia cultivated and weedy rice; and in Central America and Mexico maize and weedy teosinte grew in proximity. He recognized that these weedy races were living germplasm banks available to the plant breeder as sources for resistance to disease and insect damage.

Plant Exploration. It is estimated that Harlan collected more than 12,000 accessions from 45 countries for the United States. He collected wheat, barley, maize, forage legumes and grasses, large seeded legumes, forest trees, fruits, and ornamentals. Some of these accessions have been used extensively as sources of disease resistance or for their unique genetic properties.

A Rational Classification of Cultivated Plants. Harlan and deWet recognized that the formal method used in

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taxonomy was not very satisfactory for the classification of cultivated plants. On the other hand, users of germplasm as plant breeders had developed their own informal system for grouping plants. Harlan and deWet attempted to reconcile these different approaches by developing a unified system. They looked at the total available gene pool of a cultivated plant and assigned taxa to one of three gene pools: primary, secondary, or tertiary.

The primary gene pool (GP-1) consists of the domesticate and conspecific wild form, corresponding with the biological species. Among forms of this gene pool, crossing is easy, and hybrids are generally fertile with good chromosome pairing.

The secondary gene pool (GP-2) includes those biological species that can exchange genes with the domesticate (i.e., belonging to the same coenospecies). Gene transfer is possible but difficult. Hybrids tend to be sterile and chromosomes pair poorly or not at all.

The tertiary gene pool (GP-3) includes all members of that coenospecies to which the domesticate belongs. Crosses can be made with the crop, but the hybrids tend to be lethal or completely sterile. Transfer is only possible using drastic techniques (e.g., embryo culture, doubling of chromosome number, or using bridge species to obtain some fertility). GP-3 was the outer limit of the potential gene pool of a crop.

Centers of Origin. The Russian agronomist N. I. Vavilov proposed eight centers of origin for most of the cultivated plants of the world. Jack Harlan refined the concept to include space, time, and variation. In a series of papers Harlan proposed new terms to express the specific evolutionary patterns of different crops (e.g., endemic, semiendemic, monocentric, oliocentric, noncentric, and microcenter).

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A Wild Wheat Harvest. Harlan destroyed the prevailing paradigm that hunter-gathers were driven to cultivate plants. In Turkey he demonstrated that he could gather the equivalent of more than 2 pounds of clean wild einkorn grain per hour using a stone-blade sickle. Thus, in about a three-week period a family could gather more grain than it could possibly consume in a year.

Crop Evolution Lab. The Crop Evolution Laboratory was a cosmopolitan place. There were graduate students, post-graduate students, and visiting scholars from all over the world. In early 1983 deWet and I estimated that 19 different languages were spoken in the lab. Visitors from many diverse disciplines literally popped into the lab and often were prevailed upon to present impromptu seminars. The students and visiting scholars studied evolutionary patterns of major and minor seed crops as well as root and tuber crops. During the classroom lectures Harlan often showed slides taken by him of exotic crops grown in their home area as well as sites of historic or cultural interest. The labs often consisted of the students tasting freshly made ethnic foods from various regions of the world. The students were delighted with the master teacher.

MOST OF THE MATERIAL in this memoir was based on a biography I prepared for *Plant Breeding Reviews* (vol. 8, 1990), which was dedicated to Jack Harlan. Family members provided some personal information. Other material is the result of memories from having been associated with Harlan as a graduate student at Oklahoma State University and afterwards as a faculty colleague at the University of Illinois.

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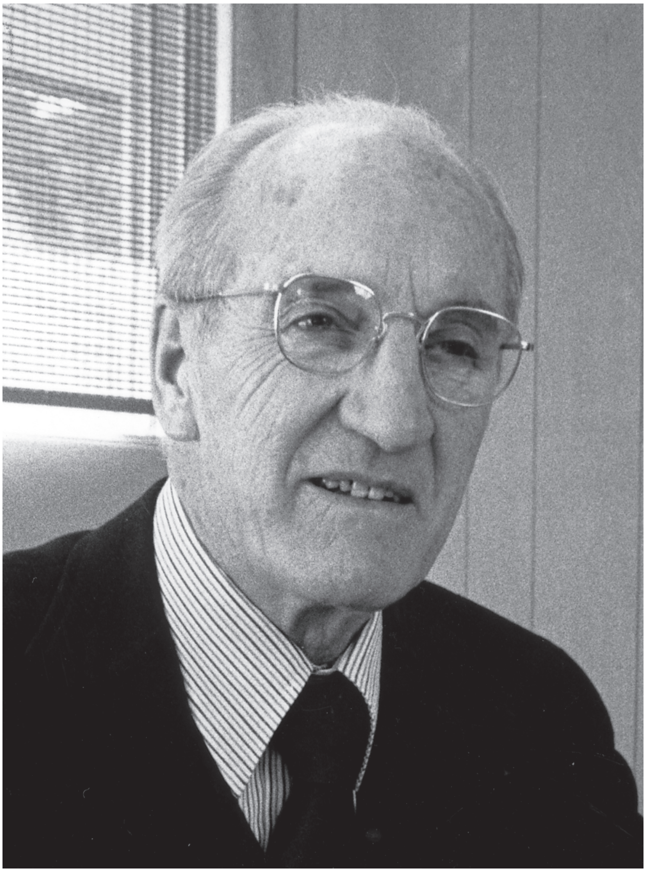

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ARTHUR DAVIS HASLER

January 5, 1908–March 23, 2001

BY GENE E. LIKENS

ARTHUR DAVIS HASLER did pioneering limnological research across a broad spectrum of ecological subdisciplines from ecophysiology and behavior of fish to experimental manipulation of entire lake ecosystems. His work on the mechanisms whereby salmon find their way back from ocean feeding areas to home streams for spawning, for which he was best known, was not only brilliant and innovative but also provided a framework for management of these important fisheries throughout the world.

Hasler was born in Lehi, Utah, the second of four sons of Mormon parents, Walter Thalmann Hasler, a physician, and Ada Broomhead Hasler. His Mormon background played a significant and important role throughout his life, particularly regarding his active role in public service. He was among those who strongly advocated for acceptance of African-American membership in the Mormon Church.

He married Hanna Prusse in 1932, and they had six children: Sylvia, A. Frederick, Bruce, Galen, Mark, and Karl. Hanna was a trained vocalist (soprano) and music was a large part of the family's activities. Hasler's passions went far beyond science. His love of music and poetry was legend among his students and colleagues. He recited the works of Mörike, Heine, or Goethe at every opportunity and played

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the horn (waldhorn) for some 30 years in the University of Wisconsin Symphony and the Madison Civic Symphony. He frequently greeted a woman with a kiss to her hand. On long road trips to research sites and scientific meetings it was not uncommon for Hanna to break out the songbooks, pass them out in the car, and lead everyone in singing. In those days a major professor and graduate students often took long trips together by car to field sites and professional meetings. Hanna died in 1969. In 1971 Hasler married Hatheway Minton Brooks, who shared his love for a healthy environment, and with her own six children forged a close and loving extended family. Hasler had 32 grandchildren and 17 great-grandchildren.

He received a B.A. degree, majoring in zoology, from Brigham Young University in 1932 and a Ph.D. degree in zoology and physiology from the University of Wisconsin-Madison in 1937. He was awarded honorary doctor of science degrees from the Memorial University of Newfoundland in 1967 and Miami University, Oxford, Ohio, in 1988.

Hasler had interrupted his schooling at Brigham Young University in the late 1920s to serve a three-year mission to Germany for the Church of Jesus Christ of Latter Day Saints. It was during this time that his love for the German language began.

After working for the U.S. Fish and Wildlife Service as an aquatic biologist on the Chesapeake Bay during 1935-37, he and Hanna moved to Madison where he completed his Ph.D. in 1937 at the University of Wisconsin, under the supervision of well-known limnologist Chancey Juday. He was hired there as an instructor of zoology in 1937 and promoted to assistant professor in 1941. After serving with the U.S. Strategic Bombing Survey in Germany in 1945, he returned to the university in 1945 as associate professor of zoology and was promoted to full professor in 1948, and

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served in that capacity until he retired in 1978. During that time 52 doctoral students and 43 masters students received degrees under his supervision.

Hasler actively published in the peer-reviewed literature for almost 50 years from 1935 to 1984. He authored, co-authored, edited, or contributed to 7 books and over 200 scientific publications.

For more than a hundred years the University of Wisconsin-Madison has been an international center for limnology. Started by Edward A. Birge and Chancey Juday in the late 1800s, the Wisconsin School of Limnology was continued, strengthened, and enlarged by Hasler from 1946 to 1978. He supervised a large, active, and diverse limnology program conducted in several scattered and some rather Spartan structures on campus, known affectionately as the Lake Lab. In 1963 he became director of the Laboratory for Limnology coincident with the construction of a new and proper Limnological Laboratory on the shoreline of Lake Mendota. He fought aggressively and successfully with the faculty and administration of the university against the construction of a 600-car parking lot on the site and extending into Lake Mendota. His final plea at the faculty hearing was a quote from St. Mark: “Go thy way and sin no more.”

Hasler was one of the preeminent ecologists of the twentieth century. When he was elected to the National Academy of Sciences in 1969 only two other ecologists (G. E. Hutchinson and C. L. Hubbs) had ever received this prestigious honor. Hasler was a Fulbright research scholar in Germany in 1954-55 and a Fulbright visiting professor at the University of Helsinki in 1963. He was elected to the Societas Scientiarum Fennica in 1965, the American Academy of Arts and Sciences in 1972, the Royal Netherlands Academy of Science in 1976, and the Wisconsin Academy of Sciences, Arts, and Letters in 1988. He received 10 distinguished

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scientist awards, including the Award of Excellence from the American Fisheries Society in 1977, the Distinguished Service Award from the American Institute of Biological Sciences in 1980, and possibly most significantly, the Citizen of the Year Award from the Mendota-Monona Lake Property Owners Association in 1987.

An important measure of his influence in professional biology was his service as president of the American Society of Limnology and Oceanography (in 1951), the Ecological Society of America (in 1961), the International Association for Ecology (1967-74), and the American Society of Zoologists (in 1971). Hasler also was the founding director of the Institute of Ecology (1971-74). He was awarded the Naumann-Thienemann Medal from the International Association of Theoretical and Applied Limnology, the highest international award in limnology, in 1992. He was an exchange scholar for the National Academy of Sciences in China in 1983 and in the Soviet Union in 1986.

With very broad interests and expertise he could equally well have carried the scientific descriptor of limnologist, ecologist, fishery biologist, zoologist, and conservationist. He conducted research and informed public policy in all of these disciplines.

Hasler is best known for his research on salmon olfactory imprinting, a powerful and ingrained sense of smell that enables these fish to return to the exact stream of their birth for spawning after traveling thousands of kilometers in the ocean. He often told the story about the genesis of this discovery when he was vacationing in the Wasatch Range of the Rocky Mountains of Utah, where he had spent much time as a boy. Hiking up a mountain, yet out of sight of his favorite waterfall, he suddenly had what he called a “déjà senti” experience, “as a cool breeze, bearing the fragrance of mosses and columbine, swept around

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the rocky abutment, the details of this waterfall and its setting on the face of the mountain suddenly leapt into my mind's eye" (1966, p. 65). Among other things these smells reminded him of childhood memories and of home. If smells could trigger such memories in a human, they must be at least as evocative for salmon, Hasler reasoned. This revelation led to a rich and productive series of experiments and field trials on olfactory and solar orientation in fishes.

Hasler's pioneering research using manipulation of entire lake ecosystems provided a powerful new tool for ecology. Following the early lead of his major professor, Juday, who had added fertilizer to lakes to increase fish production, Hasler greatly developed and expanded this new experimental approach for studying large ecosystems (lakes) within their natural settings. He recognized early that entire ecosystems were just too complex to study piecemeal or only in the laboratory. His first efforts were focused on trying to enhance the productivity of fish in the thousands of acidic brown-water lakes in the upper midwestern United States. The brown staining by dissolved organic matter in these lakes prevented light penetration and thereby reduced productivity of aquatic plants at the base of the food web. In 1947, finely ground hydrated lime (calcium and magnesium hydroxide) was added to an acid, brown-water lake in Langlade County, Wisconsin, to determine whether the water could be cleared and the depth of the trophogenic zone increased. This experiment was aborted. Then, in 1950 hydrated lime was added to two small lakes in Chippewa County, Wisconsin, and resulted in remarkable alkalinization and increased transparency of the water (1951). The stage was set for a rigorous whole-lake experimental manipulation, so he found two lakes on the northern Wisconsin-Michigan border, located on property of the University of Notre Dame, that were connected and together were

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shaped like spectacles or an hourglass. He obtained permission to bulldoze an earthen dam across the narrow constriction between these two lakes in 1951 and thus formed the now famous setting for whole-lake experiments by creating two separate lakes, Peter and Paul. Subsequently, Peter Lake was treated with hydrated lime to flocculate and precipitate the dissolved organic carbon in these humic brown-stained lakes, while Paul Lake was maintained as an untreated reference in this experimental manipulation (Stross and Hasler, 1960).

Other lakes were artificially circulated using compressed air to reduce ice cover and prevent winterkill of fish, experimentally manipulated with additions of hydrogen peroxide to reduce color, manipulated by additions of hydrated NH_4 through rather primitive aeration systems on the bottom of lakes to increase productivity, and labeled with radioactive tracers to study water circulation and biological transport of nutrients from deepwater to surrounding landscapes. These field experiments in whole-lake ecosystems had varied levels of success, but the overall approach was innovative and powerful. Inspired by this model for the study of complex natural ecosystems, W. E. Johnson, one of Hasler's Ph.D. students, and J. R. Vallentyne designed an experimental lakes area in Ontario, Canada, that used whole-lake manipulation very successfully in studies of lake eutrophication, acidification, and toxification by heavy metals. Likewise, F. H. Bormann, R. S. Pierce, N. M. Johnson, and the author (another Ph.D. student of Hasler's) adopted an experimental approach in studies of watershed ecosystems in the Hubbard Brook Valley of New Hampshire. This small watershed approach—in association with a nutrient flux and cycling model and entire watershed manipulations—helped establish fundamental understanding of northern hardwood forest ecosystems.

Hasler always freely acknowledged the role of travel and

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his many professional colleagues, students, and visitors in expanding his reach in scientific inquiry and influence. He insisted that graduate students in residence meet with and discuss their research with each visiting scientist. Because of the international stature of his program, there was a constant flow of visitors to the Lake Lab. Hasler's contemporaries were the noted animal behaviorists Nobelist Karl von Frisch and Konrad Lorenz; Wilhelm Einsele, limnologist and chemist; and G. Evelyn Hutchinson, limnologist and ecologist. He considered them scientific heroes.

Hasler made it a point to provide not just academic training for students but personal advice as well. He usually had a large number of students under his supervision, but he took special interest in each of us. His achievements, career, and style were an inspiration for us, and he invested much time promoting his students.

The National Science Foundation was just beginning to fund science shortly after Hasler started his research career at the university. He successfully obtained financial support for his research from the Atomic Energy Commission, Office of Naval Research, and of special significance, wealthy landowners in northern Wisconsin. Several research projects were supported for decades by and on the properties of these philanthropists. He was able to convince these landholders of the practical importance of this research, and thus of its benefit to them.

Not only was Hasler a preeminent scientist but he was also a preeminent statesman of science. Constantly working to enhance organizations, networks, and teams to promote the betterment of ecology and the conservation of natural resources, he had few peers, and his efforts have provided a continuing legacy. Late in his career he tried to initiate a "Salmon for Peace" project, which attempted to bring together the governments of Russia and China to restore and manage

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the salmon population in the Amur River, which had been depleted because of overfishing. Even though this effort was unsuccessful, it clearly demonstrated his desire to apply ecological understanding to practical problems.

Hasler played a key role in developing and promoting the fact that land-water interactions are important for what occurs in lakes, such as variable water quality. His early classic paper on "cultural eutrophication" (1947) helped to guide efforts regarding sewage diversion, fertilizer and manure runoff, and soil erosion from agricultural fields to lakes. He focused much attention on his beloved Lake Mendota, which he could see from his office window at the university. Prior to these efforts much of lake management still revolved around the idea of a lake as a microcosm (Forbes, 1899). John Magnuson, who succeeded Hasler as director of the Laboratory for Limnology, said of Hasler, "He was a big thinker and had grand ideas. He believed you were not done in research until you dealt with its application to society." During a time when a Washington presence was not in fashion, Hasler spoke out frequently, eloquently and effectively on environmental issues that he knew about and cared about. Hasler was an outstanding scientist, a mentor, a wonderful friend, and an effective spokesman for the protection of natural resources.

Although he had survived four bouts of cancer (colon, lung, skin, and prostate) starting in 1972, all without major chemotherapy, he continued to be active in campus activities until December 2000. He died peacefully in March 2001 at 93.

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Er ist's
[by Eduard Mörike]
Frühling läßt sein blaues Band
Wieder flattern durch die Lüfte;
Süße, wohlbekannte Düfte
Streifen ahnungsvoll das Land.
Veilchen träumen schon,
Wollen balde kommen.
- Horch, von fern ein leiser Harfenton!
Frühling, ja du bist's !
Dich hab ich vernommen !

I AM PLEASED to acknowledge Hatheway Hasler, Linda Holthaus, John Magnuson, and William Schmitz for details and suggestions; autobiographical materials from the National Academy of Sciences and the University of Wisconsin-Madison; various *Limnology News* newsletters from the Center for Limnology, University of Wisconsin-Madison; and "Resolution of Respect" in the *Bulletin of the Ecological Society of America*, July 2001 (S. Carpenter and J. Kitchell; G. E. Likens) in the preparation of this memoir. A transcript of an oral interview by Laura Lord Smail, University of Wisconsin-Madison, Oral History Project, in 1977 was especially helpful.

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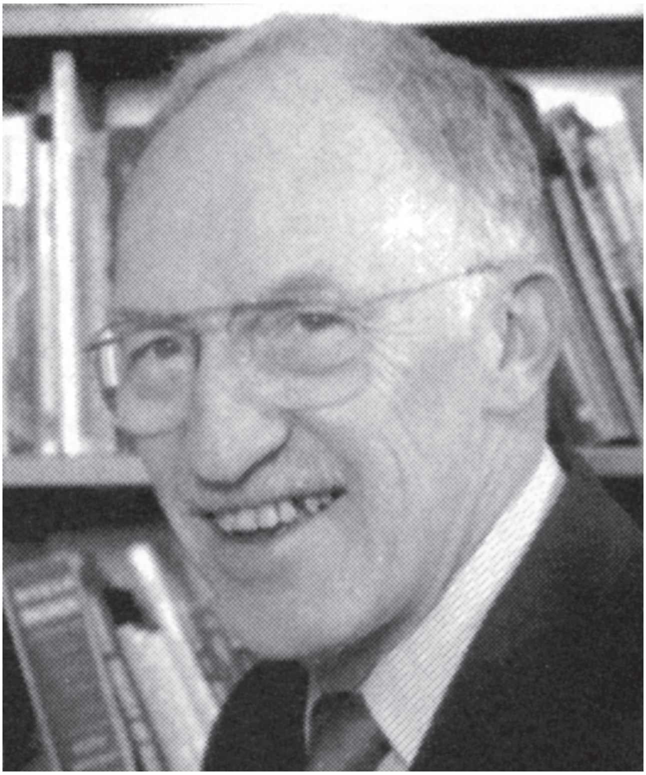

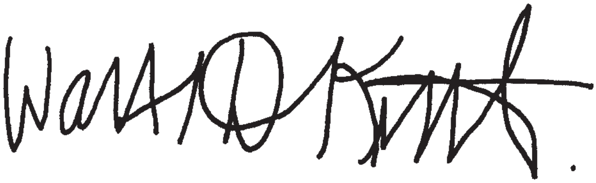

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WALTER DAVID KNIGHT

October 14, 1919–June 28, 2000

BY ERWIN L. HAHN, VITALY V. KRESIN, AND
JOHN H. REYNOLDS

WALTER DAVID KNIGHT was a consummate experimental physicist. His 50 years of research spanned widely different techniques, albeit with a central focus on the physics of metals. His first experiments were with test tube-like samples at room temperature and atmospheric pressure. The electronics and magnets were homemade; the data emerged as red ink tracings of galvanometer deflections on long strips of paper. Unafraid of a whole new world of techniques, he much later carried out experiments on samples of clustered metallic atoms, produced in vacuum by supersonic jets of argon and analyzed in molecular beams. There his complex apparatuses were computer controlled, and the data emerged into digital files.

Walter's name has been immortalized in condensed-matter physics as the discoverer of a nuclear magnetic resonance phenomenon known as the "Knight shift." It was a major development in the understanding of the electronic properties of metals, and to this day remains a mainstay tool in research on metals, alloys, and superconductors. He pio-

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neered the use of electric quadrupole resonance as well as magnetic resonance as probes of structural and electronic effects, phase transitions, liquid metals, etc.

Later in his career he initiated groundbreaking work on the physics of small metal clusters, now referred to as nanoclusters. His group discovered their electronic shell structure and traced the evolution of their metallic properties with increasing size. The field of nanoparticle science has blossomed in the past 20 years, and Walter Knight is acknowledged as one of its pioneers.

His 40-year career at the University of California, Berkeley, also involved a continuing full load of teaching and for a time extraordinary service as an academic dean.

FROM BOYHOOD TO THE DOCTORATE

Walter was born October 14, 1919, and was raised mostly in New York City. His father was a leading Presbyterian minister, who was able to maintain a comfortable home in the city and to send Walter and his sister, Paula, to excellent schools. The Knight family, however, hailed from a farming background in Marlborough, New Hampshire, and Walter spent his summers there as a country boy, "Ginger" Knight to his numerous cousins. New Hampshire had a deeper stamp on Walter than the big city, and his practicality with tools and his earthy sense of humor attested to that fact.

His college years were at Middlebury College in Vermont. His graduate work was at Duke University, where before his World War II military service as a radar officer in the U.S. Navy, he received an M.A. degree in physics and completed all the requirements for a Ph.D. except for the required research. He recalled that he had "plenty of training in electronics in the Navy" that stood him in good stead in his subsequent research. He wrote an interesting account

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of his immediate postwar research in a paper entitled "The Knight Shift," which appears in volume I of the *Encyclopedia of Nuclear Magnetic Resonance*, cited at the end of this memoir. There one reads about the events that led to his important discovery of the shift in nuclear magnetic resonance frequencies that occurs in metals and provides a probe of the local internal fields in materials without disturbing the basic structure of the system under study. The research for his Ph.D. thesis at Duke was carried out while Walter was an assistant professor of physics at Trinity College, Hartford, and while he was commuting to or "summering" at the Nuclear Moments Laboratory at Brookhaven National Laboratory, where his apparatus was put together. In practical matters of experimental technique he interacted closely at Brookhaven with Bill Cohen and Bob Pound. In the theoretical interpretation of his results he interacted closely with Norman Ramsey, Samuel Goudsmit, Conyers Herring, and perhaps most importantly, with Charles Townes, whom Walter sometimes described as his informal thesis supervisor.

EARLY RESEARCH AT BERKELEY

His successes in research brought him to the attention of Berkeley physics professor Francis Jenkins, who had been dispatched by other senior professors there to travel eastward and recruit new talent for the Physics Department, where a new addition to LeConte Hall was nearing completion and where expansion of the department had been authorized. One of Jenkins's assignments had been to recruit people who could establish a group in solid-state physics, hitherto unrepresented as a field at Berkeley. His success in this assignment can be gauged by the fact that he recruited Erwin Hahn, Carson Jeffries, Arthur Kip, Charles Kittel, and Walter Knight who eventually came to form the nucleus of this group.

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Walter moved to Berkeley and began experimental work there in the summer of 1950. Walter's important discovery in Brookhaven of the effect of the magnetism of conduction electrons in metals on the nucleus had opened up an important subfield of research on nuclear magnetic resonance. With this new way available to study properties of electrons in conducting materials, Walter and his students at Berkeley contributed a series of pioneering investigations of many different types of conducting solids, including alloys, semiconductors, and superconductors. As these experiments evolved it was learned in more detail that the Knight shift had to be distinguished more carefully from other competing shifts. These were the chemical frequency shifts due to the action of chemical bonds, and nuclear electric quadrupole shifts occurring in non-cubic metals. Not only was the Knight shift an important parameter but also the theory behind it was closely connected with the parameter of nuclear spin-lattice relaxation times measured in metals. Because of the skin depth penetration limit of radio-frequency fields into the interior of metals, Walter and his students were often preoccupied in developing techniques to prepare metals in the form of very small spheres, with as much uniformity in diameter as possible.

Two very important research results came about. First, in the course of looking for the shift properties in non-cubic metals Walter and his students discovered the first nuclear quadrupole resonance in a metal, namely, gallium. They followed this discovery by seeing zero field quadrupole resonances in several other metals. Second, after the BCS theory of superconductivity in metals was introduced Walter exploited the Knight shift in such a way as to have it play an important experimental role in probing the BCS theory. The theory predicted that the Knight shift of the superconducting electrons should be reduced (that is, the

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spins should pair off more and more) as the temperature approached absolute zero. After overcoming a number of experimental artifacts, Walter confirmed that this reduction of the shift with temperature indeed did occur. (See also his narrative in the article from the *Encyclopedia of Nuclear Magnetic Resonance*.)

In addition to research papers Walter over the years wrote influential reviews. His 1956 review "Electron Paramagnetism and Nuclear Magnetic Resonance in Metals," published in *Solid State Physics*, vol. 2, p. 93, was designated a "citation classic" by Current Contents in 1985.

UNIVERSITY SERVICE AT BERKELEY

Walter, like most of his Berkeley colleagues, taught a full load of physics classes as a matter of course. In addition he consented to make one of his first contributions to university service as a so-called "baby dean" in the College of Letters and Science. There were several such deans assisting the principal dean of Letters and Science and their duties were mainly in interacting with students in the college and helping to solve the difficulties that students encounter in navigating their ways through the degree requirements and minimum scholastic achievements expected in the college. In 1963-64 an active issue arose at Berkeley as to how to decide which applicants to undergraduate status at Berkeley should be chosen among a large excess over the number of places available. The mechanism that seemed to be in ascendancy among the various proposals was the so-called "random selection," whereby places would be awarded in some sort of lottery. To Walter this device seemed so idiotic that he uncharacteristically became a campus activist against random selection and soon emerged as the leader of a successful faculty protest. His having served from 1961 to 1963 as an associate dean in the college, a step up

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from "baby dean," together with his newly recognized prominence as a faculty leader, led to his being named in 1967 as the principal dean of the College of Letters and Science. Suddenly Walter was serving in a job where all academic appointments at Berkeley, other than in the professional schools, were his to allocate (and help evaluate) among the many departments he oversaw. As well, he had to deal with the countless faculty dissatisfactions and frictions that arise so often in academia. For five full years Walter was in this administrative hot seat and pretty much on his own, because it was only upon his leaving that office that associate deans in the college, one to each of the four disciplinary branches, were assigned many of his responsibilities. One way of saying this is that when he retired as dean, the job was assumed not by one but five people. The years of his deanship, from 1967 to 1972, were beset with extra difficulties encompassing, as they did, strident times of student unrest at Berkeley and elsewhere in the United States.

RESEARCH AT BERKELEY AFTER HIS DEANSHIP

We believe it fair to say that seldom do academics who have to become so immersed in campus administration that their teaching and research is brought to a virtual standstill emerge from such service and return successfully to a combination of classroom teaching and significant research. So here begins a truly remarkable chapter in Walter's career, namely, two decades of groundbreaking work on the physics of small metal clusters.

Walter's first involvement with small metallic particles goes back to his work in the 1960s on the Knight shift in superconductors. Later in that decade he became very interested in the possibility of observing quantum size effects in small systems (or nanoclusters, as they are often referred to nowadays). The issue of energy level separation in small

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particles was first considered by Fröhlich in 1937 and was given great stimulation by a landmark paper of Kubo in 1962. It was predicted that for temperatures $k_B T < \delta$, where δ is the average electron level spacing, particle susceptibilities would be strongly altered and would display dramatic even-odd alternations. Walter's group began low-temperature nuclear magnetic resonance studies of 10- to 100-nm particles deposited on substrates and obtained results supporting the theoretical ideas.

They also came to realize that matrix interactions and associated problems prevented a clear view of quantum size effects. The path that Walter decided to follow to resolve this problem is nicely outlined on the last page of a 1975 paper by Yee and Knight.

> It therefore appears important to design complementary experiments which eliminate the problems of impurities and boundary interactions with the matrix at the surface of the particles under study. Such an experiment has been proposed and is being carried out in this laboratory . . . Beams of freshly condensed particles are formed by an oven and collimating slits and pass through an inhomogeneous magnetic field, as in the Stern-Gerlach experiment, to a detector. It appears to be quite feasible to employ particles in the range from 10 to 1000 atoms, thus making accessible a large range of measurements capable of elucidating the development of the electronic structure of the semi-infinite metallic lattice from the primordial metallic molecule . . . Ultimately, it should be possible to observe the effects of surface contamination and measure the heat capacity of the particles. The method is applicable to non-metallic particles . . . Electric deflections should be possible.

Today, 25 years after the appearance of this paper, it is remarkable that every single prospect outlined there has come to be realized and currently represents an active avenue of research.

Within the next several years the aforementioned cluster beam machine was indeed constructed (formed out of a big atomic beam apparatus donated by a department col-

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league), and beams of alkali-metal clusters were generated. This marked the transition of Walter's research from the world of solids and surfaces to that of beams, jets, and mass spectrometers. It also marked the first steps to a new discovery. Following initial experiments on Stern-Gerlach deflections of alkali trimers, the group proceeded to install a high-quality supersonic oven source and a high-range mass spectrometer in order to study larger clusters. A mass spectrum of sodium clusters spanning from 2 to 100 atoms was measured. A beautiful pattern appeared on the display: The cluster population did not form a simple featureless landscape, but instead resembled an impressive mountain range, with specific peaks (at 8-, 20-, 40-, 58-, and 92-atom sizes) especially prominent. It soon became apparent that these "magic numbers" were nothing less than a dramatic and long-sought manifestation of size quantization: shell ordering of the discrete states of delocalized electrons in small clusters. This observation is striking and beautiful, representing the third known appearance of shell structure in nature (after atoms and nuclei). Now it forms the basic framework for much of the work on understanding metal cluster evolution, a research enterprise that has grown from just a few groups in the late 1970s and early 1980s to many times that number today. Furthermore, it has established metal clusters as a fascinating laboratory for the accurate study of many-body physics. According to the Science Citation Index, by the end of 2002, the 1984 paper by Knight et al. on "Electronic Shell Structure and Abundances of Sodium Clusters" has been cited close to 900 times.

The discovery of shell structure was followed by a decade of further fundamental contributions by Walter, his students, and postdocs to the field of metal cluster physics. Space limitations prevent us from a detailed account of these results, so we shall only briefly mention benchmark

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measurements of cluster polarizabilities and ionization potentials, the landmark discovery of giant electronic resonances, or plasmons, in small clusters (the 1987-91 series of papers on this effect already share over 600 citations in the literature), and experiments on cluster scattering.

Walter enjoyed strong research ties with many colleagues around the world, including longstanding close contacts with cluster researchers at the Niels Bohr Institute in Copenhagen and at the École Polytechnique in Lausanne. He was undoubtedly pleased that his work on metal clusters has generated interest across so many research disciplines, attracting solid-state, atomic, chemical, and even nuclear physicists. (Shell effects analogous to those in metal clusters were recently discovered in the quantized conduction spectra of metal nanowires. This has potentially important practical consequences for nanoscale electronics and illustrates once more how beautiful results from basic research can lead in unpredictable ways to very useful applications.)

TEACHING AT BERKELEY

Walter reassumed his classroom teaching after serving as dean. Unlike many of his colleagues he *enjoyed* teaching the large lecture course in general physics designed for premedical students and the like. He taught it effectively for many years, bridging physics and liberal arts. With a friend in chemistry he also created an honors course in physical science that appealed to many of the brightest students in the arts. At a higher level of general physics Walter was a coauthor of the first volume of the Berkeley Physics Course, one of the U.S. curriculum-enrichment projects in the post-Sputnik era.

Between 1950 and 1991 Walter guided 29 graduate students to a doctorate in physics.

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WALTER AND SARA

Walter was married in 1972 to Sara Pattershall Blanpied, a native of Maine. This union, a second marriage for both, was an exceptionally happy one. Between them Sara and Walter were parents to four children. Sara shared Walter's interests in the arts and became an active member of the campus and town community. Their home, high in the Berkeley Hills and commanding a breathtaking view of San Francisco Bay, was a gathering place for entertaining friends and colleagues, including visitors from the many places on the globe where researches and visits had taken the Knights. In this house one could always be assured of enjoying a superb glass of wine and truly wonderful conversation, friendly and erudite, full of love of art and history, nature and travel, music and science. Visiting this beautiful house and enjoying its genuinely warm hospitality was always a special experience.

We hope that this memoir will attest to the high regard that friends and colleagues had for Walter Knight, a son of New Hampshire whose work at Berkeley contributed so richly to science, to his university, and to his students.

WE WOULD LIKE to acknowledge the kind permission of Taylor & Francis, Ltd., to use the text of our biographical sketch published in a Festschrift for Walter Knight's eightieth birthday: *Philosophical Magazine B*, vol. 79, no. 9, 1999 (<http://www.tandf.co.uk>). We drew upon the recollections of Walter's friends, colleagues, and family, and upon his autobiographical article entitled "The Knight Shift" published in the *Encyclopedia of Nuclear Magnetic Resonance*, vol. 1, edited by D. M. Grant and R. K. Harris (Chichester: Wiley, 1995) and reprinted in the aforementioned Festschrift. We also made use of the UC Berkeley press release of June 30, 2000, by R. Sanders.

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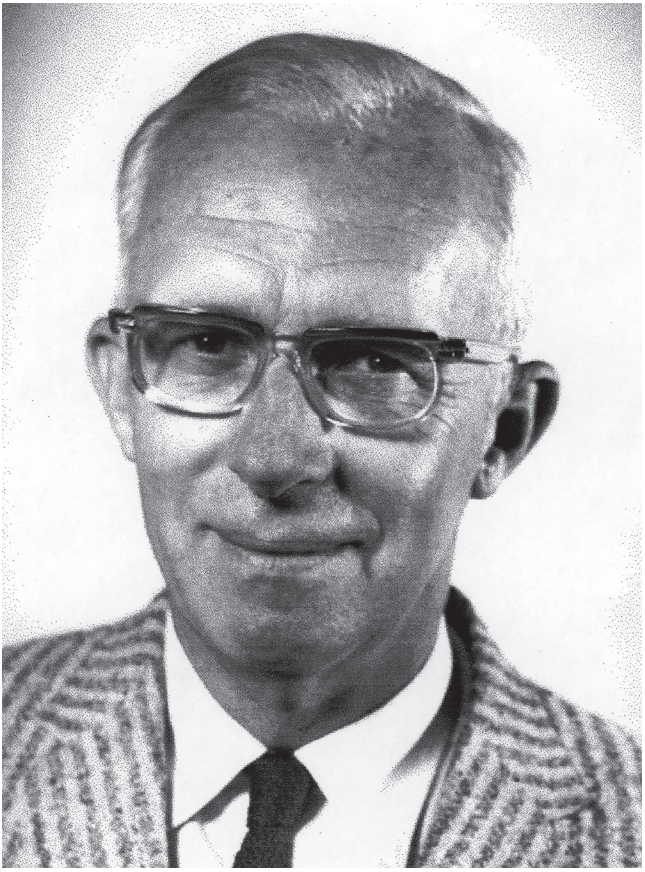

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ERWIN W. MÜLLER

June 13, 1911–May 17, 1977

BY ALLAN J. MELMED

THE FORTY-SEVENTH INTERNATIONAL Field Emission Symposium, which took place July 29 to August 6, 2001, was held in Berlin to commemorate the fiftieth anniversary of Erwin Müller's first publication on the invention of the field ion microscope there in the summer of 1951. The opening session of the meeting was devoted to historical accounts of the development of field electron microscopy (FEM), field ion microscopy (FIM), and atom probe mass spectroscopy (APMS, also known as APFIM for atom probe field ion microscopy)—all fields of scientific and technical endeavor originated by the late Erwin W. Müller. The achievements of these fields of study and their influence on other scientific fields stand as a tribute to the remarkable creativity and ingenuity of Professor Müller. Those of us who knew him remember with admiration his great ability as a scientist, an experimentalist, and a teacher. The history of the creation and development of FEM, FIM, and APMS is in large part the biography of Erwin Müller.

Erwin Wilhelm Müller was born in Berlin on June 13, 1911, the year the Kaiser-Wilhelm-Institute for Physical Chemistry and Electrochemistry (now the Fritz-Haber-Institute of the Max Planck Society) was founded. He died on May 17, 1977. A short time later his wife kindly reminisced with me

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about her husband, providing some insight into his early life. He was the only child of Wilhelm M. and Käthe Müller (nee Käthe M. Teipelke), a family of modest means. His father was a construction worker specializing in plastering ceilings in houses. Erwin Müller worked as a part-time research assistant at the Osram company in Berlin from 1932 to 1935; from 1935 to 1937 he was a research physicist at the Siemens company, also in Berlin, where he invented the FEM while continuing his education. He married Klara Thüssing in 1939, and their daughter Jutta, their only child, was born in 1940. He obtained his university education at the Technische Hochschule Berlin-Charlottenburg (now Technische Univerität Berlin), receiving an engineering diploma in 1935 and a doctor of engineering (physics emphasis) in 1936, working under the direction of the Nobel Prize-winning physicist Gustav Hertz. Those were stressful times for the young family because Erwin was not a National Socialist Party member and therefore had great difficulty trying to rise to a university post. Consequently, it was only after the war, in 1950, that he achieved his *Habilitation* from the Technical University Berlin (successor to the Technische Hochschule).

After Siemens he worked for the Stabilovolt company in Berlin, where he was director of research and development from 1937 to 1946, a critical time in German history. Of possible consequence to science, it is interesting that according to Klara Müller, he was protected from the party by his good research efforts. Michael Drechsler, a former coworker of Müller's has written[1] that the Stabilovolt laboratory in Berlin was destroyed by bombs in 1944 and that Müller attempted to rebuild it in Altenburg and in Dresden. He notes Müller's good fortune in managing to survive the firebombing of Dresden. Müller retained considerable resentment against the Allies for this late event in the Second

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World War, which was made clear in a 1951 conversation with Ralph Klein in Berlin. Klara, and earlier Erwin Müller, told me that immediately after the war they survived by picking up scraps from recently harvested fields and by learning to prepare baking powder for making bread from scraps of marble in the cemetery, maintaining a diet of about 900 calories a day. At this time, from 1946 to 1947, Müller was lecturer of physical chemistry at the Technical Institute in Altenburg, several miles from his home, to and from which he walked every day.

While Müller was working in Altenburg, I. N. Stranski invited him to come to the Kaiser-Wilhelm Institute in Berlin, where Müller went next and where he worked from 1947 to 1951. He began as an assistant to Stranski and later became a group leader and then a department head. Here he invented the field ion microscope. (Considering the extensive war damage in Germany, one can imagine that conditions for research and development at the German institutes were as bad as the food situation, and it required unusual inventiveness and experimental skill for Müller to obtain his excellent results.) With his *Habilitation* in 1950 he also became a teacher at the Technical University Berlin, and in 1951 he became a professor at the Free University Berlin. Then he moved to the United States and started a new laboratory at Pennsylvania State University. At the same time he maintained close contacts with the Fritz-Haber-Institute by way of a lively correspondence with his former students and coworkers as well as with I. N. Stranski and M. von Laue, and mutual visits. The Max Planck Society officially recognized these good relations by making him an external scientific member of the Fritz-Haber Institute, Berlin, in 1957, which he accepted as much as an obligation as an honor. At Penn State he began as professor of physics. In 1955 he became a research professor of physics, and was appointed to the

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prestigious Evan Pugh Professor of Physics post in 1968. Finally, he was named professor emeritus in 1976.

Erwin Müller's first publication was in *Zeitschrift für Physik* in 1935: "A Method for Photometric Measurement of the Intensity of Spectral Lines." His dissertation research, "The Dependence of Field Electron Emission on Work Function," was published in *Zeitschrift für Physik* in 1936. Overall, four papers and most importantly the invention of the field electron microscope resulted from his work with Gustav Hertz at Siemens. He went on to publish some 211 scientific papers over an active research career of 41 years.

The political circumstances in Germany during the 1930s strongly influenced Erwin Müller's scientific career, and it is remarkable that he was able to develop FEM at that time. Drechsler has written about some of the prevailing circumstances.[1] The cast of great scientists then working and lecturing in Berlin was certainly impressive: Einstein, Planck, Schrödinger, Debye, Nernst, Hertz, Haber, von Laue, Grotrian, Volmer, and Schottky. This made for an inspirational setting for Erwin Müller to begin his scientific career. The political climate, however, was far from nurturing with respect to the scientific community. Many of the internationally well-known scientists reacted to the growing political persecution by leaving their university posts, either because they were directly persecuted or in protest of the treatment of their colleagues. Müller's research professor, Gustav Hertz, felt compelled to leave his university chair in protest, and he moved to the Siemens company, where he became director of a new laboratory set up especially for him. Fortunately for Müller, Hertz brought him along to Siemens, where he was able to continue his research into field electron emission.

Müller has described[2] the situation when he began his dissertation research. In 1936 A. Wehnelt and W. Schilling

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had used a magnetic electron microscope to image the electron field emission from the edge of a sharp knife to find that the emission was coming from discrete and non-stable small points along the knife edge. In addition, in 1936 R. P. Johnson and W. Shockley published their description of a cylindrical field emission microscope.[3] Their images viewed on a phosphor screen also showed that the electrons were being emitted by tiny protrusions on the wire cathode surface.

Müller decided to view the electron emission distribution, or pattern, from the point cathodes he was studying, so he made the point equivalent of the Johnson-Shockley microscope. Next, he constructed a vacuum tube in which an ingenious electrically heatable tungsten tip was positioned a few centimeters away from a thin phosphor screen on the front inside surface of the tube. This tube allowed him to visualize directly the electron emission from the tip, prior to and following tip heating. He observed the patchy emission from as-etched tips, similar to what had been seen in the emission from edges and wires. Very easily, however, he was able to thermally smooth the protrusions and remove contamination from the W tip and to view the electron emission pattern of the clean surface on the screen. This tube was the first point projection field emission microscope.[4] He was then able to measure the electron emission characteristics of the clean W surface and to verify the high field necessary for field emission predicted by the Fowler-Nordheim equation. Later, after Müller had left the Siemens laboratory, R. Haefer[5] quantitatively confirmed the F-N equation in 1940. FEM became a powerful microscopy, however, far beyond the attempt to visualize the surface condition of a point field electron emitter.

The simplicity of design of Erwin Müller's FEM instrument is evident when compared to other microscopes. Consider that a 10^5-10^6 enlarged image of a metal surface,

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with resolution of 2.5 nm (and 1 nm in special cases) can be gotten with a small laboratory-built FEM. However, in the years before the advent of commercially available metal vacuum components considerable experimental expertise was needed to actually make a working FEM. Müller devised a host of experimental "tricks," that is, special techniques, to enable most students to construct his microscope. Pankow related to me that later, from 1951 to 1961, he and P. Wolf and later Ralf Vanselow were assigned by Müller the task of making FEM microscopes for commercialization by the Leybold company. These were various sealed tubes including a barium evaporation source, sold primarily as demonstration equipment for schools.

As director of research and development at the Stabilovolt company Müller managed to continue some studies of field emission even though the Second World War had begun. Drechsler has noted[1] that Stabilovolt manufactured glow discharge tubes that used Ba-activated cathodes, and this provided the opportunity for Müller to investigate surface diffusion of Ba on W using his FEM. Müller's study of Ba adsorption and perhaps more importantly his discovery of field desorption of Ba from W was published in 1941.[6] His pioneering measurements of the velocity distribution of field emitted electrons[7] and his study of the resolution of the FEM[8] were published in 1943. Due to the war Müller published no further scientific research until 1949.

By this time, as described above, Müller was at the Kaiser-Wilhelm-Institute. He continued to do FEM research, publishing papers on W surface self-diffusion,[9] the imaging of phthalocyanine molecules,[10] the visibility of atoms and molecules,[11] and (with M. Drechsler) the polarizability of atoms and molecules,[12] and other seminal experiments. His interpretation of the images of adsorbed barium and phthalocyanine molecules as atoms and molecules, respectively, met

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with considerable skepticism. However, these pioneering FEM experiments, especially the surface self-diffusion work, led to considerably more work by many researchers.

As important as his FEM results were, Müller's greatest contribution to microscopy and in fact to the scientific world was his invention of FIM. Let us consider the context in which this achievement took place. The electron microscope (TEM) had achieved Ruska's original aim of exceeding the resolution of traditional optical microscopy and had reached a resolution of about 2 nm. Müller's FEM had a resolution of about 2.3 nm in general and 1 nm in special cases. Ruska and Müller, both at the Kaiser-Wilhelm-Institute, were in friendly competition with each other, according to Gustav Klipping (private communication), to get the best results. Erwin Müller, however, aimed to make a great leap forward to achieve his dream of atomic resolution. After all, scientists had no direct proof that matter consisted of discrete atoms—only indirect evidence from X-ray diffraction and chemical experiments. No one had seen atoms to prove their existence.

Erwin Müller reached 40 years of age in the summer of 1951. Ten years earlier he had reported that atoms adsorbed on a W surface could be torn off, or desorbed, by the application of a large positive electric field,[7] and since then he had pondered a way to use the desorption phenomenon to image the tip surface. It was clear to him that simply desorbing a monolayer of Ba, for example, and accelerating the resulting positive ions to the screen would not provide sufficient image intensity. He recognized the need for a continuous supply of ions but did not immediately realize how to accomplish this. Finally in 1951 the solution occurred to him.

His assistant at that time, Gerrit Pankow, recently related to me (private communication) the circumstances surround-

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ing the first FIM experiments. One morning in the summer of 1951 when Pankow came into the laboratory, Müller was preparing to do an experiment. Pankow noticed that something was wrong, so he told Professor Müller that the tip voltage polarity was set to be positive instead of negative! Müller looked at him and simply said, "From now on, we work with positive tip voltage." The first FIM microscope was an FEM operated with positive tip voltage plus the addition of a palladium tube that when heated with a hydrogen flame, allowed the introduction of hydrogen into the microscope. (A small anode ring was added to minimize any field emission from the inside wall of the microscope but was later found to be unnecessary.) This microscope, primitive by our present scientific criteria, operating at room temperature enabled Erwin Müller to see that the surface did not have a continuous structure; rather he could clearly see rows of atoms.

The invention of FIM by Erwin Müller was a remarkable achievement, especially considering the utter simplicity of such a lens-less microscope, which achieves magnification of 10^6 or more and atomic resolution by radial projection of ions from the specimen point. In contrast to the somewhat stepwise development of FEM, with contributions by several people, it is not obvious that anyone else could have or would have invented FIM. Even after Müller had the concept of imaging by field desorption of a continuously renewed source of ions, it required his great experimental ingenuity to make the microscope an actuality. His earlier experience with gaseous discharges and his lifelong interest in optics and activities as an amateur astronomer were important, especially considering that the room temperature FIM image was extremely dim, and image intensifiers did not yet exist.

Müller proceeded with great efficiency to publish his

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historic first FIM paper,[13] describing the significant improvement in contrast and resolution brought about by imaging with positive (hydrogen) ions compared to imaging by FEM and presenting the first evidence that atomic resolution was achieved. Müller's original manuscript was submitted on August 27, 1951. Interestingly, in terms of the friendly competition between Müller and Ruska, in a 1954 conference in Milan Ernst Ruska presented a published lecture in which he stated that the theoretical limit of TEM is such as to permit proving the existence of atoms. This is remarkable because Ruska knew first-hand about Müller's FIM results. One has to wonder how Müller reacted, especially because TEM had not come close to that objective, which he had reached in 1951.

It is fascinating and somewhat ironic that knowledge gained through research using Müller's FEM was important in developing the present-day atomic resolution capability of the electron microscope. In a technical discussion tape-recorded at the first field emission symposium, in McMinnville, Oregon, in 1952 it was suggested that the use of a W point field electron emitter as the electron source in an electron microscope might lead to improved resolution. Then in 1959 the results of field-electron-emission energy distributions, mentioned above, revealed an unexpectedly narrow energy distribution, which is the basis of achieving atomic resolution with the electron microscope.

The decision for Müller to leave Germany must have been difficult. He had lived and worked most of his life in Berlin and had begun to raise a family. In fact, his daughter was now 11 years old. However, after the Second World War the U.S. Joint Chiefs of Staff invited him to spend six months visiting universities in the United States, with the hope of enticing him and other good scientists to move to the United States. In September 1951, only a few weeks after submitting

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for publication his now celebrated first paper on FIM, he accepted the invitation and went to New York City, staying at the Alamac Hotel, visiting various universities, and probably not yet decided definitely to leave Germany. However, according to Müller, when he visited the Pennsylvania State University in central Pennsylvania he and Klara were immediately reminded of rural Germany. This and no doubt the miserable conditions of postwar Berlin convinced him to accept the suggestion of Dean Hall to move there, and he arrived in about February 1952. He became an U.S. citizen in 1962. At first he did only a minimum amount of classroom teaching, but he did an appreciable amount of informal teaching in the laboratory. This was perfectly suited to Müller's preferred working mode, which was devoting as much time and effort as possible toward his dream of achieving what he considered the ultimate accomplishment of microscopy: the full resolution of the surface atomic lattice of a metal. Thus far his FIM operating at room temperature with hydrogen as the imaging gas could only resolve atoms along multiple step-height ledges formed, for example, by heating the W tip after carbon adsorption—a very special case.

By late spring or early summer of 1952 Müller had begun to attract students and to set up his new Penn State field emission laboratory, in the sub-basement of Osmond Hall, which housed the Physics Department. Two years later he moved his laboratory to more spacious and more pleasant quarters on the second floor of the building. Here he worked for the remainder of his scientific career. In the early years at Penn State the majority of students in his laboratory did research on issues related to field electron emission and FEM, and only one student worked, with Müller, on FIM matters. Müller's first few publications in this period were either papers written in collaboration with M. Drechsler,

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his former assistant at the Fritz-Haber-Institute laboratory or review articles, most notably Müller's 1953 review of FEM.[2] However, events significant to the development of FIM were taking place. I have written about the relevant historical details,[14] from the personal perspective of my years, 1954-58, as a student of Professor Müller. I will summarize here the key points and suggestions related to his thinking that may be of biographical interest.

Müller's first paper, in 1951, introducing FIM was remarkable. Of course, it provided the world's first view of the atomic nature of solid matter and began an entirely new field of study. It also presented Müller's ideas for several further developments of FIM, such as cooling the microscope, the use of helium for imaging, and the phenomenon of field-induced surface dissolution, later termed field evaporation. This phenomenon ultimately made the FIM and the APFIM (atom probe field ion microscope) uniquely powerful analytic instruments. He clearly believed that his success in achieving improved image contrast and resolution, compared to FEM, validated his hypothesis that operating the FIM with a low-pressure hydrogen-ambient-enabled image formation by positive ions desorbed from a layer of continuously replenished adsorbed gas atoms. He also believed that the factor limiting resolution of the FIM was diffraction. Although he later showed that these mechanisms were not strictly correct, his belief in them somewhat retarded the complete fruition of the FIM.

Another remarkable aspect of Müller's first FIM publication was the relatively short time between the conception of the experiment and the actual publication. This undoubtedly resulted from his genius for conceiving eloquently simple experiments, one of the defining characteristics of his scientific career. During the period 1954-58, while studying under Müller, I observed what I came to recognize as his

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awesome experimental talent, evident to all of his students and coworkers. The time span between an idea and setup of a new experiment was typically only a few days. This was true also for his later introduction of various low-temperature FIM microscope designs, and T. T. Tsong has related (private communication) that it was the case also for his invention of the atom probe.

During 1952-55 attempts were being made toward improving the resolution of the FIM both in Germany by Müller's former students and coworkers at the Fritz-Haber-Institute and in the United States at Penn State. Müller was striving to achieve what he considered the ultimate objective of microscopy, that is, the ability to see the atomic surface structure of a metal. However, before 1954 both theory and experiment seemed to agree that the FIM was not likely to succeed in improving beyond the 1951 room temperature image quality. In 1952 R. Gomer published a paper in which he theorized that no improvement in resolution of the hydrogen FIM would be expected by cooling the emitter, and at about that time Pankow reported to Müller that he had found no improvement in image quality by immersing the FIM in liquid air. Then in 1954 M. G. Inghram and R. Gomer found that most of the ions contributing to the FIM image intensity originated slightly away from the surface, which was contrary to Müller's original concept of image formation. In 1954 Müller and Bahadur tried imaging at liquid nitrogen temperature and again found that cooling the FIM specimen, even using helium as imaging gas, did not improve the resolution. This added to Müller's pessimism about achieving his goal. A fascinating breakthrough occurred in October 1955, and I have described and analyzed this event and its background in detail.[14] Müller, assisted by his student Kanwar Bahadur, once again cooled the FIM specimen with liquid nitrogen. This time, due to a

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fortuitously well-prepared specimen, they unintentionally discovered the phenomenon of surface smoothing by low-temperature field evaporation while imaging with helium. This immediately gave them the world's first view of the atomic surface structure of a metal—Erwin Müller's long sought goal.

The FIM had evolved somewhat from Müller's original design, now using helium for imaging, cooling the specimen by liquid nitrogen or liquid hydrogen and smoothing the tip by field evaporation, but it still was by all criteria a marvelously simple instrument, something that Müller never tired of reminding audiences. He had wonderful showmanship and frequently exaggerated this instrumental simplicity, to the delight of audiences.

During the period 1956-66 Müller increasingly emphasized further development of FIM techniques and conducted exploratory research into many areas of FIM applications, although he continued to make significant contributions to the applications of FEM. In 1960 he published a major review paper[15] that gave practical information so that others could more easily get started doing FIM, and this helped to spread the technique around the world. In addition, in this period his work with R. D. Young, a student of his, refining the method of measuring electron energy distribution[16] led to a new theoretical analysis[17] and experimental verification of field emission energy distributions, and revealed an unexpectedly narrow energy distribution. Müller published a paper in 1957[18] that motivated the later extensive FIM research by others, notably G. Ehrlich, T. T. Tsong, and D. W. Bassett, on the diffusion of single atoms on surfaces. Müller's final research papers in FEM were published in 1962, with Young on the electron work function of (011)-oriented W, and with W. T. Pimbley on their unsuccessful search for polarized field electrons. Müller reviewed the

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progress in his microscopies in his 1969 book with T. T. Tsong.[19]

Müller's final major contribution to science was his invention of the instrument he called the atom probe, in 1967.[20] It later also became known as the atom probe field ion microscope, recognizing that it incorporates an FIM capability to give an atomic map of the specimen surface, by means of which the user selects atoms for chemical identification by time-of-flight mass spectrometry. This uniquely powerful analytical instrument has made and continues to make important contributions to materials science. The introduction of the atom probe by Müller burst like a supernova, at least on the international field emission community. After all, he was already 56 years old, had created two microscopies, and had given the world its first view of atoms. It would still be some 13 years until any other microscopy could claim the capability of seeing atoms in a solid.

In retrospect it is fascinating that at least in principle other scientists came very close to inventing the atom probe. Inghram and Gomer, H. D. Beckey, W. A. Schmidt, and J. H. Block all designed, built, and worked with mass spectroscopic instruments using field ion sources that could have been adapted to analyze the composition of the tip itself, perhaps leading them to invent the atom probe FIM. They were dedicated, however, to using the instrumentation to analyze only the composition of field ionization or adsorbed species, while Erwin Müller was focused on trying to determine the composition of individual surface atoms.

Students of Müller who were present during the time of the invention of the atom probe have described some of the relevant events to me, and J. A. Panitz has recently published[21] his description of the historical development. Müller had been trying for a few years to find a way to chemically identify atoms for which the FIM image contrast

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was not understood. The immediate motivation for this effort was the uncertainty in interpretation of FIM contrast in some binary alloys, where one element imaged with bright contrast and the other with dark contrast (his student, T. T. Tsong was studying Co-Pt, for example). Müller was well aware of the techniques of field ion mass spectroscopy and had students working with them. In addition, his students, M. P. R. Thomsen and D. F. Barofsky, had shown that field-evaporated metallic species could be mass analyzed. However, Müller realized that there were two existing shortcomings for his purposes. The detectors did not possess single-ion sensitivity and there was no way to pre-select and localize the region of analysis to do single-atom identification. He conceived the idea of using a probe hole to limit the field of view, or field of analysis, to a pre-selected atom or atoms, and believed that improved detectors could be built to detect single ions. He asked Barofsky to assess the feasibility of doing single-ion mass spectroscopy using a magnetic sector instrument with a continuous dynode detector. A short while later Barofsky learned about the time-of-flight technique from a course he was taking and suggested its use to Müller, who directed him to determine the instrument parameters suitable for an atom probe using the contemporary timing electronics. His technicians, Gerry Fowler and Brooks McLane, were assigned to put together the hardware to make such an instrument and then his student J. A. Panitz was given the project for his Ph.D. research. The atom probe came to fruition in mid-1967, a matter of only several months from its conception by Müller.

Müller's steadfast, focused effort to improve the microscopy he had invented was the defining characteristic of his scientific career. He strove to be first in all aspects of FEM and FIM, so much so that the phrase "for the first time" became his mantra. In point of fact, until about 1960 Müller

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had personally discovered most of the aspects of the microscopies, and it was not uncommon for him to remind the author following a presentation, or as a manuscript reviewer, that he had done it first, usually years ago. This zeal sometimes caused resentment and certainly masked his warm personality. In private Erwin Müller was friendly, kind, and charming. But his public persona was something else—more like a lion defending his lair.

Müller retired from active research in 1976 and was named professor emeritus. He was suffering from the after-effects of treatment for cancer of the throat, which caused him difficulty in lecturing, but his condition seemed to be improving. Then, on May 17, 1977, at the age of 65 he died from a stroke while attending the annual meeting of the National Academy of Sciences in Washington, D.C.

Erwin Müller received a number of awards and honors during his lifetime. These were as follows:

1936	Bronze Medal for outstanding work, the Technische Hochschule Berlin-Charlottenburg
1952	C. F. Gauss Medal (laudatio by Max von Laue)
1957	External scientific member, Fritz-Haber-Institute of the Max Planck Society, Berlin
1960	Achievement Award, Instrument Society of America
1961	Fellow, American Physical Society
1964	H. N. Potts Gold Medal, Franklin Institute, Philadelphia
1968	Elected member, Deutsche Akad. d. Naturforscher, Leopoldina, Halle
	Dr. rer. nat. honoris causa, Free University, Berlin
1969	Honorary fellow, Royal Microscopical Society, Oxford
	Centenary Lectureship Silver Medal, Chemical Society, London
1970	M. W. Welch Gold Medal, American Vacuum Society
	John Scott Medal, City of Philadelphia (oldest scientific award given in America)
1972	Davisson-Germer Prize, American Physical Society

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1975 Dr. honoris causa, Claude-Bernard University of Lyon
Honorary member, Indian Vacuum Society
Elected member, National Academy of Engineering
Elected member, National Academy of Sciences

In addition, he was to have received the very prestigious National Medal of Science in 1976, but the award ceremony was postponed. It was awarded instead posthumously to Müller's daughter by President Jimmy Carter on November 22, 1977, at the White House.

Erwin Müller's career had an immeasurably large impact on science and technology. His invention and development of FEM clarified the physics of field electron emission from metals and led to important contributions to the progress of surface science. In recent years knowledge gained from FEM research has become important in product development for flat-panel image displays and vacuum electronics applications. His development of ultra-high vacuum techniques, from the pioneering use of barium and other metal vacuum getters to his early achievement of vacuum levels down to below 10^{-12} torr quietly advanced both surface science and vacuum technology. His invention of the FIM dispelled the intellectual myth that atoms were too small to be seen and began the age of atomic resolution metallurgy and materials research. With it Müller brought to surface science the ability to study surface phenomena, such as single-atom and cluster surface mobility on the atomic scale. Müller's atom probe (APFIM) transformed the FIM to a major analytical instrument. A few years after his death, as instrumental innovations extended APFIM capabilities even beyond Müller's concepts, the instrument began to have and continues to have wide impact on materials research.

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I AM DEEPLY grateful for useful information and manuscript comments from many people, especially Mrs. Klara Müller, Mrs. Jutta Moser (nee Müller), Kanwar Bahadur, Doug Barofsky, Paul Cutler, Norbert Ernst, Jerry Fowler, Gary Kellogg, Ralph Klein, Gustav and Ingrid Klipping, John Panitz, Gerrit Pankow, Walt Pimbley, Werner Schmidt, Tien T. Tsong, Ralf Vanselow, Nelia Wanderka, Russ Young, the Archives of the Max Planck Society, Berlin, and the Penn State Physics Department.

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Sarah Ratner

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SARAH RATNER

June 9, 1903–July 28, 1999

BY RONALD BENTLEY

TO A REMARKABLE EXTENT Sarah Ratner's career as a biochemist largely paralleled the development of her discipline. She became a graduate student in the early 1930s, when biochemistry was mainly rooted in physiology and organic chemistry; about all that organic chemists knew of proteins was that they contained amino acids. Nucleic acids were even more of a mystery, and the catalytic action of enzymes was an enigma. The chemical structures and modes of action of vitamins and hormones were unclear. When she published her last paper in 1987, biochemistry had come of age.

The discipline of biochemistry developed slowly as new and improved technologies became available after World War II. Beginning about 1950 and continuing for two to three decades there was an astonishing acquisition of knowledge; this period was the golden age of biochemistry. The suggestion of one possible structural arrangement for DNA in 1953 was followed by the development of a new discipline, or perhaps more accurately a collection of disciplines, under the rubric "molecular biology." To a major extent biochemistry was subsumed as a most important component of molecular biology. Beginning with an organic chem-

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istry problem for her Ph.D. thesis, Sarah Ratner grew into biochemistry. Her work contributed mightily to one important new technology, and she unraveled many details of important biochemical problems. She is the epitome of a classical biochemist.

In this memoir I take the liberty to call her Sarah. That she was commonly so known does not imply condescension; rather, it attests to the warm affection and deep respect with which she was universally regarded. Her own quotations are taken from an autobiographical article, "A Long View of Nitrogen Metabolism" (Ratner, 1977) and from an autobiographical résumé made available by the National Academy of Sciences. Quotations from the Festschrift for her eightieth birthday (Pullman, 1983) are identified by FSR and page number.

Sarah's productive life of 96 years encompassed both world wars as well as other major upheavals on the worldwide scene. In the year of her birth, 1903, the telephone and gasoline-powered automobile were relatively young inventions. It was in that year that the Wrights made the first successful airplane flight. When she died in 1999, the world was totally transformed by such developments as atomic energy, jet and manned space flight, gene cloning, and the Internet. Her family home library "included books on the great technical inventions: electricity, the telegraph and telephone, the incandescent light and the internal combustion engine." These works, part of her childhood reading, stimulated her interest in new technologies.

Her parents emigrated from a poor Russian village well before 1900. Her father, self-educated except for a few years of early Hebrew biblical studies, was an omnivorous reader and collector of Hebraic and late nineteenth-century classics (history, literature, philosophy). He was a strong influence on his children. Her "gentle and self-effacing" mother

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was much concerned with family care. After the birth of three sons came twins: a fourth son and only daughter, Sarah. Of these five children she was the only one to choose an academic education.

Sarah attended a "new and excellent" high school in New York City, preferring courses in science and mathematics. Her desire to attend a university open to women centered on Cornell, not only for its strong chemistry department but also for scholarship possibilities. In fact, it was the award of a scholarship that convinced her parents to withdraw their objections to a university career. When she entered Cornell in 1920, as a chemistry major, almost all of the students were men focusing on industrial careers. She was the only woman in many of her chemistry and physics classes, and while she would have liked to exchange impressions and ideas with fellow students, "co-eds were generally placed in the pale and I was easily discouraged, being innately very shy." The situation was not easy for her; her college friends were liberal arts students with whom chemical discussions were impossible.

On graduation in 1924 neither medical school nor an industrial position seemed possible for Sarah. She gained laboratory experience in New York City in two positions requiring analytical chemistry. One was the Department of Pediatrics of the Long Island College Hospital. In 1932, collaborating with C. A. Weymuller, she reported on the acid-base metabolism of a nine-year-old child on diets with different ratios of "ketogenic to antiketogenic substances." Seventeen different analytical methods were used for determination of a wide variety of parameters in blood serum and feces (Weymuller and Ratner, 1932). One can only shudder at the amount of routine work involved. In work with R. Kurzrok (Departments of Biochemistry and of Obstetrics and Gynecology, Sloane Hospital for Women) it was

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found that in cases of amenorrhea accompanied by genital hypoplasia, excretion of follicular hormone in urine was slightly greater than normal. In this work a biological assay system was used (Kurzrok and Ratner, 1932). Other work with Kurzrok is discussed later.

These analytical experiences fostered an interest in physiological chemistry. The decisive step was her acceptance in the early 1930s as a Ph.D. student by H. T. Clarke in the Department of Biochemistry, College of Physicians and Surgeons (P&S), Columbia University. Clarke, usually termed "H.T.," had a distinguished career as an organic chemist with Eastman Kodak (Vickery, 1975). In 1928 he was appointed head of the department at P&S, then termed "Biological Chemistry," with a mission to upgrade facilities and to appoint new faculty, a task at which he succeeded brilliantly. H.T. believed in a strong role for organic chemistry and, as a historian has noted, "a conscious effort was made to introduce ideas and techniques from organic chemistry into medical research" (Kohler, 1977). The Columbia medical school had moved uptown in 1928 to Washington Heights to form part of the Columbia Presbyterian Medical Center.

The only requirement for admission to graduate study in that department was to survive an interview with H.T., at the end of which the potential student was immediately informed of the outcome. No other extraneous factors entered into his decision. He apparently based his judgment on the student's chemical knowledge and "ability to recall and coordinate his chemical experiences" (Fruton, 1990). H.T. had an uncanny sense of quality and was almost never wrong in his choices. The interview was hard to prepare for. The aspiring student might be asked "how he would make sulfuric acid or something of similar import" (Chargaff, 1978, p. 65). In Sarah's case H.T. clearly excelled himself.

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H.T. provided Sarah a part-time job in the department so that she might complete course work and also involved her in the previously described work with Kurzrok. The latter and H.T. had collaborated on some work, and the Rube Goldberg-style apparatus diagrammed by Kurzrok and Ratner (1932) for continuous extraction of follicular hormone with ethyl acetate and subsequent evaporation could only have been constructed by H.T., a skilled glassblower.

A 1935 paper was the first for which she was the sole author. Also heavily dependent on analytical work, the iron content of the teeth of anemic animals was found to be about half that of the controls (Ratner, 1935). At this time she held a Wm. J. Gies Fellowship (1934-35); both Gies and H.T. were thanked "for their generous interest and advice."

Sarah was also involved in work for which she did not receive authorship. She stated that in 1930, working with Kurzrok, she discovered a low molecular mass compound in human semen that could produce uterine contractions. Much later, and in retrospect, she noted that this work "constituted the first recorded description of the uterine contracting properties of a prostaglandin." Her "List of Publications" (provided by the National Academy of Sciences) contained no paper describing this work. Examination of Kurzrok's publications shows that Sarah was indeed involved. In a 1930 paper (Kurzrok and Lieb, 1930) the authors are listed in large capital letters as RAPHAEL KURZROK and CHARLES C. LIEB. Immediately following, in parentheses and in very much smaller type, is the brief statement "With the assistance of Sarah Ratner." This 1930 paper is notable, being the very first in which her name appears in connection with research in the scientific literature. Perhaps Sarah singled out this work in her autobiographical material since she felt that her "assistance" deserved coauthorship or at least a much warmer acknowledgment. In later papers

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Kurzrok and his colleagues made no further mention of Sarah.

Her Ph.D. thesis work was straight organic chemistry: a study of the reaction of cysteine with formaldehyde to form a thiazolidine-4-carboxylic acid (Ratner and Clarke, 1937). Much later Sarah took pride in the fact that this work was of great interest in connection with the structure of penicillin. A penicillin degradation product, penicillamine, reacted smoothly with acetone. Penicillamine was found to be ßß-dimethylcysteine, and the reaction product was a thiazolidine carboxylic acid structure formed exactly as in the reaction of cysteine with formaldehyde. The thiazolidine ring is an important component of the ß-lactam structure for penicillin.

In late 1936 Sarah sought postdoctoral research positions but again encountered problems, apparently as a result of her gender. She notes that "other students finishing up at that time were men, and they had been well placed." A research position distant from New York City was accepted reluctantly in view of her father's ill health. When he died, she returned to New York to assume responsibility for her mother's care.

In 1937 Sarah had "the professional good fortune to be invited back to P&S to work with Rudolf Schoenheimer." Finally she was on the right track, and she never looked back. H.T.'s humanism and lack of prejudice had opened his department to many scholars who were refugees from the nightmare policies of the Nazis. Schoenheimer was one such individual. Together with David Rittenberg (Bentley, 2001) he had developed the use of the heavy isotope of hydrogen as a tracer of metabolic processes. When Sarah was asked to join the group, similar work was just beginning with the heavy isotope of nitrogen. Tracer isotope techniques with both stable and radioactive isotopes came to

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play a most vital role in the new technologies exploited in biochemistry. The leadership role of Schoenheimer in this development has been extensively described, notably by Kohler (1977).

A special social organization was necessary for the tracer work: an interdisciplinary group (Kohler, 1977). The separated isotopes were provided by a physicist, labeled organic molecules were synthesized by an organic chemist, instrumentation for assay required a physical chemist, and finally a biochemist was needed to define metabolic problems and to interpret results. The group at P&S was a very early example of exemplary interdisciplinary research. In this group Sarah initially played the role of the organic chemist and in 1939 coauthored a paper with Schoenheimer describing the synthesis of amino acids containing ^{15}N. She was, moreover, rapidly "growing into biochemistry" and specifically the biochemistry of nitrogen compounds. From 1937 to 1939 she was supported by a Macy research fellowship and from 1939 to 1946 had titles of instructor and assistant professor.

There was, however, an unhappy development; Schoenheimer took his own life in 1941. He was to have delivered three lectures at Harvard's Medical School later that year, and Sarah and Rittenberg assisted H.T. in preparing Schoenheimer's drafts for delivery (by H.T.) and publication as *The Dynamic State of Body Constituents* (1942). This book is a landmark in biochemical writing. The isotope group had also begun work on the synthesis of antibody protein before Schoenheimer's death. Sarah felt this work did not receive enough acclaim and she later expanded on it in a lecture (Ratner, 1979).

Beginning in 1942 she worked with D. E. Green on amino and hydroxy acid oxidases and on a peptide form of *p*-aminobenzoic acid. A purified L-amino acid oxidase from rat kidney was shown to have broad specificity and to be a

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flavoprotein. Also present in liver, this enzyme did not have a major role in amino acid deamination. While the overall outcome was disappointing, this research was instrumental in stimulating her interest in enzymology. She became "very eager to pursue new facets of nitrogen metabolism."

Sarah was recruited by S. Ochoa as an assistant professor of pharmacology at New York University's School of Medicine in 1946. She was 43 years old and finally was able to strike out on her own. One year later she published a brief but most important paper on the mechanism of the formation of arginine from citrulline. It marked the beginning of her comprehensive study of urea biosynthesis (see later) that occupied her over the next four decades. It was a late start to be sure but one leading to splendid conclusions with observations of great significance.

When Ochoa became chair of the New York University Medical School's Department of Biochemistry and moved to a new building, Sarah "was left in a dilemma which was happily solved by the return of Efraim Racker to New York." Racker was to head and reorganize the Department of Biochemistry at the Public Health Research Institute of New York. Sarah joined the institute that finally became her scientific home; it was her last move. She remained as a staff member until retirement in 1992, when she was close to 90 years old.

The urea story began in 1932 when H. A. Krebs and K. Henseleit studied urea formation in respiring liver slices in the presence of oxygen, discovering a cyclic process. Ornithine, CO_2, and NH_3 reacted to form citrulline. With a second NH_3 the latter produced arginine and finally arginine was decomposed with the enzyme arginase to form urea and ornithine; the latter could initiate the cycle again (see Figure 1). Sarah demonstrated that the citrulline → arginine reaction was more complex than it appeared. The ni-

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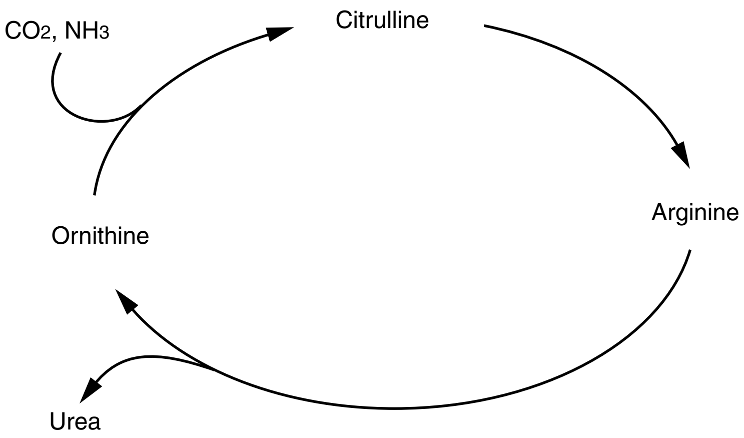

FIGURE 1 The original Krebs/Henseleit proposal for urea synthesis.

trogen donor was not NH_3 but aspartic acid, and the reaction with citrulline led to formation of a previously undiscovered amino acid, argininosuccinic acid. Two further enzymes were necessary for urea formation: argininosuccinate synthetase catalyzing the formation of argininosuccinate and argininosuccinate lyase catalyzing argininosuccinate decomposition (see Figure 2). A major part of her achievement was to purify these enzymes from various sources, to study their molecular structures, and to determine the catalytic mechanisms by which they acted.

Also of great significance was the finding that ATP was necessary for the urea cycle operation, thus explaining the need for O_2 in the Krebs-Henseleit experiments; argininosuccinate formation required ATP. Indeed, the ATP-generating activity of the citric acid cycle was metabolically related to urea synthesis. In addition, two ketoacids of the citric acid cycle, oxaloacetic acid and α-ketoglutaric acid,

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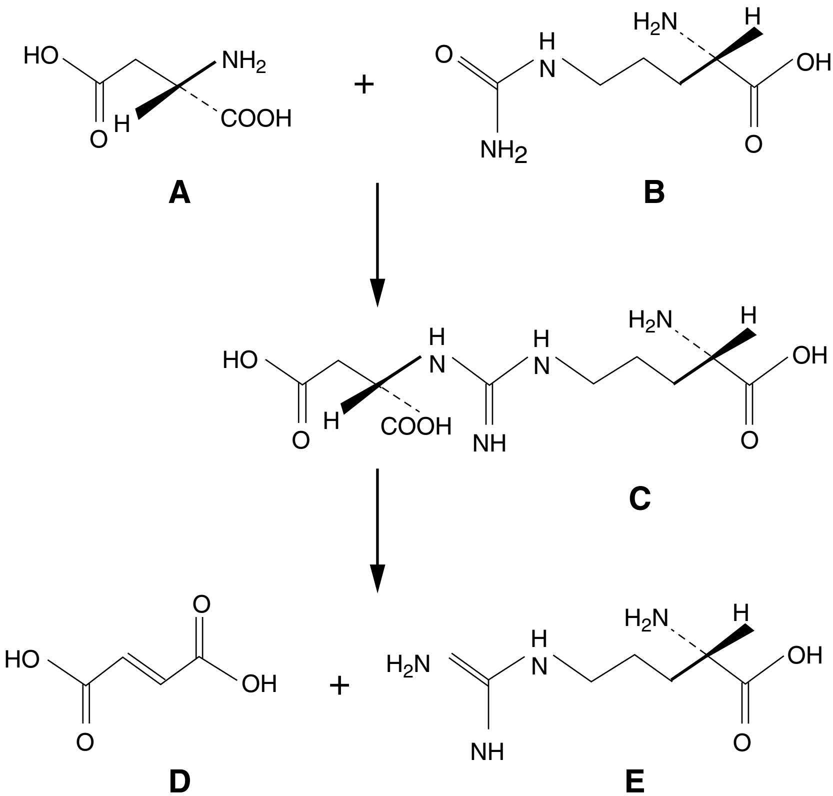

FIGURE 2 The Ratner portion of the urea cycle. *A* = aspartate, *B* = citrulline, *C* = argininosuccinate, *D* = fumarate, *E* = arginine. The formation of *C* from aspartate and citrulline is catalyzed by argininosuccinate synthetase and requires ATP. Its decomposition is catalyzed by argininosuccinate lyase.

played important roles. The interrelationships between the two cycles (citric acid and urea) are best visualized by consulting Figure 3. An excellent summary by Sarah herself is also available (Ratner, 1976).

In 1958 Sarah learned of an unfortunate child with

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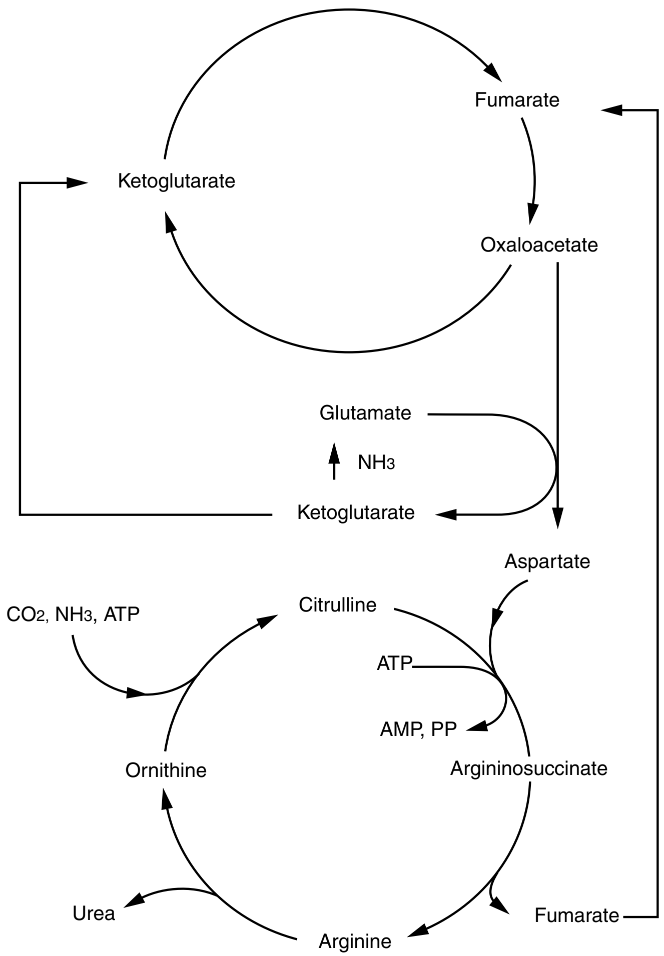

FIGURE 3 Interrelationships between the Krebs/Henseleit/Ratner cycle and the citric acid cycle. The top circle shows a much abbreviated form of the citric acid cycle emphasizing the three components of most interest. Coupled with the reactions of oxidative phosphorylation, this cycle leads to formation of ATP (required for argininosuccinate synthetase). CO_2 and NH_3 actually enter the cycle through carbamoyl phosphate; synthesis of the latter is also dependent on ATP.

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mental retardation and marked derangements of nitrogen metabolism who excreted some 3 grams per day of argininosuccinate. This was probably the earliest description of an inborn metabolic error involving the urea cycle. This particular problem is now known as argininosuccinate aciduria. Similarly, citrullinemia, an autosomal recessive genetic disease, involves a deficiency of argininosuccinate synthetase and produces excessive levels of citrulline in plasma and urine, and neurological damage and mental retardation. Few individuals have discovered two enzymes with such significant consequences.

Sarah's work on isotope tracer techniques contributed to the extensive flowering of biochemistry between 1950 and 1970. It is not easy in the twenty-first century to realize how few instrumental capabilities were available before World War II. As a Ph.D. student the major tools available to her were elemental analysis, melting and boiling point determinations as indications of purity, and the intellectual gift to interpret results, often by analogy with previous findings. Apparatus was very simple and only slowly did glassware with standard ground joints come into use. Her thesis advisor, H.T., was "a very good organic chemist of the old observance; one of those who liked to putter around in the laboratory with test tubes and small beakers and watch glasses and who was happy when crystals appeared" (Chargaff, 1978, p. 67). A snapshot of that time is provided by Sarah (personal communication, letter, December 3, 1989, from Sarah Ratner). She was "reminded how primitive our instruments were at that time. For example, all the pH measurements were carried out with a very thin glass membrane drawn out by the department's technician guided by a description which had just been published by someone at another university."

Although placing a high value on teaching for student

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and lecturer alike, Sarah was uncomfortable as a lecturer. Her manner of speaking was deliberate and thoughtful. Colleague H. Waelsch remarked, "Sarah, I always know when you are calling because the phone rings more slowly" (FSR, p. 243). She taught pharmacology at New York University after spending part of a summer reading "the monumental pharmacology text by Goodman and Gilman." She regretted that she had not taught biochemistry in a lecture course. She was too modest to claim that at a different level she was, in fact, a most gifted teacher who excelled in mentoring and guiding those associated with her. Her research was accomplished by herself or by a rather small group. Her style "tended toward longer papers and less frequent publications."

Sarah had a relatively late start as an independent investigator and later in life stated that "my career as a biochemist has been a more difficult one because of my sex." She believed that she received relatively few applications for postdoctoral positions in her laboratory for the same reason. Incredible as it may seem today, when she went to Cornell in the 1920s there were many universities that did not admit women; Cornell was a happy exception. T. C. Stadtman has noted that "the obstacles that had to be overcome to achieve scientific recognition when Sarah started her career were of an entirely different magnitude than they are now. Every woman biochemist who has followed Sarah Ratner has benefited from her example of excellence and perseverance and owes her a debt of gratitude" (FSR, p. 233). A revealing interchange between mentor, H.T., and student, Sarah, has been noted. "When 'H. T.' [Clarke] (bless his heart!) sincerely intending to be complimentary, remarked, 'Sarah thinks like a man,' Sarah's response was a somewhat disdainful, 'hrumph'" (FSR, p. 3).

Sarah received several awards: William J. Gies fellow in

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biological chemistry (1934-35), Schoenheimer lecturer (1956), Carl Neuberg Medal (1959), Garvan Medal of the American Chemical Society (1961), and Freedman Award in Biochemistry, New York Academy of Sciences (1975). She was one of the relatively few women to have been elected to the National Academy of Sciences (in 1974). From 1978 to 1979 she was a Fogarty scholar in residence at the National Institutes of Health. She served on boards and committees at various times: member of the Editorial Board and Editorial Committee, *Journal of Biological Chemistry*; Editorial Board, *Analytical Biochemistry*; Executive Committee of the American Chemical Society, Division of Biochemistry; and member of the Extramural Science Advisory Council, National Heart and Lung Institute, National Institutes of Health. She received an honorary D.Sc. degree in 1984 from the State University of New York, Stony Brook. The citation read in part as follows:

Known to your colleagues as a scientist's scientist, you have pursued a long and successful career in unravelling the complexities of amino acid metabolism. Your way of going about scientific investigation combines determination to arrive at a complete molecular description of a biochemical process with clarity of vision and exquisite attention to detail.

Since I never had the privilege of working with Sarah Ratner, knowing her mainly as a result of my wife's abiding affection for her, I may not be her best biographer. It is appropriate, therefore, to let her colleagues remedy my deficiencies by quoting at length from the 1983 Festschrift in her honor.

M. L. Blanchard: "She was an effective teacher and what I learned from her helped to shape my life" (FSR, p. 4).

M. J. Coon: "I became impressed with her dedication to science, the meticulous care she devoted to experimentation, and her thoughtful approach to problems both scientific and nonscientific" (FSR, p. 41).

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M. E. Jones: "Her stature is great as an innovator and as a scientist who has made critical and important contributions to the study of nitrogen and amino acid metabolism. Her personal attributes may be less well known but are cherished by those of us who have had the opportunity to know her and to discuss science with her" (FSR, p. 81).

C. J. Lusty: "To me Sarah Ratner represents scientific excellence. She is a scholar, mentor and a friend" (FSR, p. 103).

H. S. Penefsky: "I am speaking of her intense commitment to the highest standards of scientific excellence and the natural way in which these standards are expressed and communicated by example. Those of us privileged to know her could not, I think, fail to absorb and be influenced by those lessons. Thank you, Sarah" (FSR, p. 145).

B. Petrack: "My respect for Sarah continues to grow, not just for her considerable scientific achievements, but also for her many admirable personal qualities, among which integrity, kindness, and courage stand out" (FSR, p. 147).

F. Lipmann: "A fascination with science was surely the center of her life's interest, but I would like to mention that it still left time for her to become, for example, an expert in the fine art of bookbinding, to name only one of her side interests" (FSR, p. 167).

M. E. Pullman: "[Sarah] has been a wise and generous friend and colleague for many years. Her uncompromising devotion to the highest standards of scientific excellence has been and continues to be an inspiration to those of us privileged to know her" (FSR, p. 181).

E. Racker: "Sarah's interest in music and the arts is manifest in her writings which are compositions of a very individualistic scientist. Her contributions are recognized all over the world. Her laughter and warmth are cherished by all who know her" (FSR, p. 185).

O. M. Rochovansky: "She tried to instill in me the qualities of a good researcher: an unprejudiced mind, logical thinking, experimental care and the need of that extra dash—the insight that makes a difficult problem suddenly become beautifully clear. These are qualities that Sarah has in abundance" (FSR, p. 187).

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S. Gluecksohn-Waelsch: "I remember visiting you in your parents' house on Shakespeare Avenue in the Bronx—a home notable for its high standards of tradition and hospitality, standards that you have maintained and continue to do through the years" (FSR, p. 243).

The portrait is very clear: extraordinary insight, uncompromising standards and integrity, a most distinguished biochemist especially concerned with nitrogen metabolism, gifted mentor to many, and above all a very warm and vibrant human being. Unhappily, her health declined somewhat following retirement; Sarah Ratner's long and most productive life ended on July 28, 1999.

J. W. BENNETT kindly read a draft of this memoir, and I am grateful to her for valuable suggestions.

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Sarah Ratner published close to 100 articles. Of these, 25 were book chapters, review articles, and contributions to *Methods in Enzymology* and are not listed below. Her series on "Biosynthesis of Urea" ran to 15 papers in the *Journal of Biological Chemistry.* She was an important contributor to several papers in the classic series "Studies in Protein Metabolism" authored by R. Schoenheimer and collaborators, and also published in the *Journal of Biological Chemistry.*

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ABRAHAM ROBINSON

October 6, 1918–April 11, 1974

BY JOSEPH W. DAUBEN

Playfulness is an important element in the makeup of a good mathematician.

—Abraham Robinson

Abraham Robinson was born on October 6, 1918, in the Prussian mining town of Waldenburg (now Walbrzych), Poland.[1] His father, Abraham Robinsohn (1878-1918), after a traditional Jewish Talmudic education as a boy went on to study philosophy and literature in Switzerland, where he earned his Ph.D. from the University of Bern in 1909. Following an early career as a journalist and with growing Zionist sympathies, Robinsohn accepted a position in 1912 as secretary to David Wolfson, former president and a leading figure of the World Zionist Organization. When Wolfson died in 1915, Robinsohn became responsible for both the Herzl and Wolfson archives. He also had become increasingly involved with the affairs of the Jewish National Fund. In 1916 he married Hedwig Charlotte (Lotte) Bähr (1888-1949), daughter of a Jewish teacher and herself a teacher.

[1]Born Abraham Robinsohn, he later changed the spelling of his name to Robinson shortly after his arrival in London at the beginning of World War II. This spelling of his name is used throughout to distinguish Abby Robinson the mathematician from his father of the same name, the senior Robinsohn. Abby's older brother, Shaul, always used Robinsohn, the traditional form of the family name.

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In 1916 their first son, Saul Benjamin, was born in Cologne, Germany. Robinsohn had been appointed the first director of the Jewish National Library in Jerusalem, but the family's plans to emigrate to Palestine were unexpectedly precluded when Robinsohn prematurely died of a heart attack in Berlin on May 3, 1918. Five months later their second son, Abraham (Abby), was born in Waldenburg, Lower Silesia, where Lotte Robinsohn had moved to live with her parents. In 1925 the family moved to Breslau, capital of Silesia, where Lotte Robinsohn worked for the Keren Hajessod, a Zionist organization devoted to the emigration of Jews to Palestine.

Both Saul and Abby were educated at a private Jewish school in Breslau, where Abby was very soon identified as "a genius." He liked to hike, and enjoyed writing short stories, poems, plays, and even a five-act comedy, "Aus einer Tierchronik" (From a Chronicle of Animals). Both brothers attended the Jewish High School in Breslau and looked forward to spending their summers in Vienna with their uncle Isak Robinsohn, a prominent radiologist.

In 1933, however, as Hitler and the National Socialists came to power in Germany, Lotte Robinsohn decided it was time to realize her lifelong dream of settling in Palestine. The family left Berlin by train on April 1, 1933, as Jewish businesses were being boycotted throughout the country. The trip south through Austria to Italy afforded the family a chance to see Rome, where Abby was greatly impressed by the Coliseum and found that he especially liked Italian pastries and espresso. In Naples they boarded the *Volcania*, a ship that sailed for Palestine via Greece. From Piraeus the Robinsohns were able to spend a day in Athens, and the Acropolis naturally made a lasting impression. A day later, when their ship docked in Haifa, as Abby recalled in his diary, everyone on board was singing the Ha-tikvah:

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"Our hope is not yet lost, the age-old hope, to return to the land of our fathers. . . ." But under the British Mandate refugees could not establish legal residence, and so the Robinsohns arrived in Palestine as "tourists," with ongoing tickets to Trieste, which they never used.

PALESTINE (1933-1939)

To support her family Lotte Robinsohn ran a small *pension* in Tel Aviv, but when Saul Robinsohn went to Jerusalem in 1934 to attend Hebrew University, within a year she and Abby also moved to Jerusalem, where Abby finished high school before going on to the university as well. To help meet family expenses he began tutoring students in various subjects, including Hebrew. The first evidence of his mathematical interests also dates from this time: a set of notes in German on the properties of conics (Seligman, 1979, p. xii).

Jewish immigration to Palestine grew dramatically in the late 1930s; simultaneously the Arab population increasingly rebelled against the mandate and Zionism. The Jewish response was the creation of an illegal organization for the defense of Palestine, the Haganah. Robinson joined the Haganah and often assumed night watches. From time to time there were also paramilitary exercises in the mountains near Jerusalem that would keep him away from his studies, sometimes for weeks at a time. When six students at Hebrew University were killed on Mt. Scopus in 1936, the immediacy of the danger was apparent. It was not long before Robinson was made a junior officer of the Haganah.

When Robinson entered Hebrew University in 1936, the Mathematics Department (added to the faculty in 1927) was barely a decade old. But, given the exodus of Jews from Europe, a number of impressive mathematicians had settled in Palestine, including Abraham Fraenkel, Michael Fekete,

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Jacob Levitzki—and Robinson studied with them all. The library was built around the collection of Felix Klein, whose books the university had obtained in 1926. Edmund Landau taught briefly in the newly founded Einstein Institute of Mathematics, and was followed by the appointment of Fraenkel. Robinson was among Fraenkel's first students, but within two years Fraenkel said that he had already taught Robinson, his brightest student, all that he could (Seligman, 1979, p. xv).

In addition to mathematics Robinson also took a number of courses in theoretical physics, an introductory course in Greek, as well as readings in ancient philosophy, especially the pre-Socratics and Plato. He also took a course devoted specifically to Leibniz. One of his fellow students Ernst Straus recalls, "When we did not understand something, we would ask him to explain it to us later" (quoted in Seligman, 1979, p. xvii). Robinson was also active in the university's mathematics club, which he had helped organize and to which he once gave a lecture on the zeta function.

Robinson's first publication appeared in 1939 in the *Journal of Symbolic Logic*. This showed that the axiom of definiteness (the axiom of extensionality, or the axiom that establishes the character of equality within the system) was independent of the axioms of Zermelo-Fraenkel set theory. The paper was reviewed by Paul Bernays, who recommended it with various revisions to Alonzo Church for publication. Another paper accepted in 1939 for publication in *Compositio Mathematica* offered a simple proof of the theorem that for rings with minimal conditions for right ideals, every right nil ideal is nilpotent. This work drew on ideas inspired by courses Robinson had taken with Jacob Levitzky, but when World War II broke out, *Compositio Mathematica* ceased publication, and Robinson's paper, though corrected

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in page proof, did not appear (although it is included in the edition of Robinson's *Selected Papers*).

At the end of 1939 Robinson was awarded a special French government scholarship. In his application he explained that he needed to broaden his mathematical horizons, especially with respect to "mathematical methodology" and that in France he hoped to be able to read the vast literature on the subject not available in Palestine. And so, despite the war that had already begun in Eastern Europe in 1939, Robinson set off in January of 1940 by ship from Beirut to Marseilles and then went on by train to Paris.

PARIS: JANUARY-JUNE 1940

In Paris Robinson lived in a small *pension* in the *Quartier Latin*, not far from the Sorbonne. There is no record of what Robinson may have done with respect to his mathematical studies in Paris, apart from an enthusiastic letter of introduction Fraenkel wrote on Robinson's behalf to the philosopher of mathematics Leon Brunschvicg. While in Paris, Robinson's diary records visits to the museums and galleries, public concerts, the opera, cinemas, and the theatre. He noted in particular a play he saw by Jean Giraudoux, *Ondine*, based on a German novella but presented in Paris with typical French "*esprit*" and "*clarté*," as Robinson said (Dauben, 1995, p. 65). He also commented specifically on an exhibition he had seen by the Belgian artist Frans Masereel, a member of the Association of Revolutionary Artists and Writers, an antifascist group. Masereel had been sympathetic to the Republican cause in Spain and stressed socially progressive and radical themes in much of his artistic work, which Robinson regarded as better known in Palestine than in France.

When the Germans invaded Holland, Belgium, and Luxemburg in May of 1940, Robinson first thought about

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trying to make his way back to Palestine. But, when Mussolini sided with Germany on June 10, declaring war on France and England, any easy route back to Palestine was effectively blocked. The next day Robinson left Paris, having learned that the German army was only some 30 kilometers northwest of the city. Relying on a combination of suburban trains, trucks, and often making his way on foot, he headed for Bordeaux. Fortunately, traveling on a British passport, Robinson was able to secure a place on a coal tender, one of the last to carry refugees across the channel from France to England. The trip took four days, slowed by intermittent shelling from a German ship and occasional strafings by enemy planes overhead. Everyone slept on the open deck until the boat reached Falmouth in Cornwall. From there Robinson was taken to a holding facility in London for processing along with thousands of other refugees.

LONDON (1940–1946)

Thanks to the Jewish National League in London, Robinson eventually found a place to stay in Brixton, "a quarter of ill repute," as his diary put it. Soon the Germans were bombing London, and the Battle of Britain was underway. For weeks on end the *blitzkrieg* was relentless. One morning, returning from a night's refuge in one of the underground stations, Robinson found his quarters destroyed by a bomb, and for nearly two weeks he was homeless. By November of 1940, however, he had enlisted in the Free French Forces under the command of General Charles de Gaulle.

Despite the war Robinson did his best to keep his mathematics alive and even wrote a short paper on a generalized distributive law for commutative fields. M. H. Etherington, in reviewing the paper for the *Proceedings of the Royal Society of Edinburgh*, described the results as "entirely new,"

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"interesting," and "could not be of any assistance to the enemy," whereupon the article, "On a Certain Variation of the Distributive Law for a Commutative Algebraic Field," was published in 1941.

Meanwhile, Robinson had by a fortuitous set of circumstances been asked to help in writing a report on aircraft design for the Ministry of Aircraft Production. The results were sufficiently impressive that the British government requested Robinson's transfer from the Free French to the Ministry of Aircraft Production at the Royal Aircraft Establishment in Farnborough, just southwest of London. Immediately Robinson began to study aerodynamics in earnest and soon passed a special examination administered by the Royal Aeronautical Society, whereby in June of 1942 he was made an "associate fellow."

In January of 1943 Robinson was visiting friends in London when he met Renée Kopel, a refugee from Vienna who was working in London as an actress and fashion designer. The two soon found that they both enjoyed the theatre, art galleries, nature, walking, and above all, music. Exactly a year after they met, Renée and Abby were married at Temple Fortune in Golders' Green. At first they lived in West Byfleet, nearly equidistant between Farnborough and London, and later Surbiton, somewhat closer to London. In the meantime, Robinson had joined the Home Guard, to have a more active, physical involvement with the war.

At Farnborough Robinson's research was devoted in part to a study of the merits of single- versus double-engine designs for planes on aircraft carriers, but soon he was transferred to the aerodynamics department, where he began research on supersonic aerodynamics. One of the last of Robinson's projects at Farnborough was the reconstruction of a German V-2 rocket from bits and pieces of debris the

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Royal Air Force had managed to collect from test firings from Peenemünde that landed in Sweden and Poland.

Once Allied forces had made their beachhead at Normandy in June of 1944, it was another two months before Paris was liberated, on August 25. Within months Mussolini was dead in Italy, Hitler had committed suicide in Berlin, and Churchill finally proclaimed an end to the war in Europe, V-E Day, May 8, 1945. Abby donned his Royal Air Force uniform and went to London, where he and Renée listened to Churchill address the nation from Whitehall, after which they joined the crowds celebrating in central London.

With the war in Europe finally at an end Robinson was assigned to an intelligence reconnaissance task force sent to Germany to debrief scientists in hopes of learning what they had accomplished in aerodynamical research. While in Frankfurt, he also made a special side trip to Breselenz to see the house where Riemann was born.

LONDON (1946–1951)

One of the first things Robinson did after the war was to make a brief return visit to Jerusalem, in part to take his examinations for his diploma from Hebrew University, and to see his mother, brother, and friends whom he had not seen for six years. Robinson was subsequently awarded his M.S. degree, with minors in physics and philosophy. He also used the month he was there to work on a paper with his former instructor Theodore Motzkin, the result of which was "Characterization of Algebraic Plane Curves," published in the *Duke Mathematical Journal* the following year.

Robinson returned to London, having accepted a position as senior lecturer at the newly founded College of Aeronautics at Cranfield (northwest of London), where he taught mathematics in the Department of Aerodynamics. At

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Cranfield Robinson spent considerable amounts of time conducting experiments in the wind tunnels and also learned to fly in order to gain the practical experience many theoreticians never acquired. Among Robinson's continuing research interests at this time was the design of delta wings for supersonic travel.

Wanting to further his own mathematical studies, Robinson enrolled as a graduate student at Birkbeck College, University of London, where he studied with Richard Cooke and Paul Dienes. He originally thought to devote a doctoral thesis to the syntax of algebra, but this eventually became "On the Metamathematics of Algebra." He reported some of the early results of his thesis in a brief abstract he sent to the *Journal of Symbolic Logic* in 1949: "Analysis and Development of Algebra by the Methods of Symbolic Logic." Even from this very concise note it was clear that his interests were much more mathematical in a strict sense than were those of other pioneers of the subject like Alfred Tarski and Leon Henkin. Robinson's major interest was algebra, and he regarded logic as a means of obtaining new and more general results—not as an end in itself.

Robinson first came to the attention of a worldwide audience at the International Congress of Mathematicians held in 1950 in Cambridge, Massachusetts. Based on the strength of a proposal he had submitted, Robinson was invited to give a lecture, "On the Applications of Symbolic Logic to Algebra," which presented further results from his just completed Ph.D. thesis (in 1949). Here he was in excellent company; the other invited lecturers in the section on logic, in addition to Tarski, were Stephen Kleene and Thoralf Skolem.

Both Robinson's congress lecture and his thesis dealt with models and algebras of axioms, in which his introduction of diagrams and transfer principles was especially in-

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novative—and where he established a variety of results concerning algebraically closed fields. Philosophically, at this point Robinson was committed as he said to a "fairly robust philosophical realism," meaning that he accepted the full "reality" of any given mathematical structure. The formal languages he drew upon were simply constructs to describe structures, and these he took for granted. His methods above all made it possible to establish results "whose proof by conventional means is not apparent" (Dauben, 1995, p. 175). Later, as a mature mathematician he would adopt a more formalist position with respect to the foundations of mathematics.

Robinson's thesis from Birkbeck College was published by North-Holland in 1951 as *On the Metamathematics of Algebra.* Robinson was also made deputy head of the Department of Aeronautics at Cranfield. However, in February 1951 he received an invitation to accept a position as an associate professor at the University of Toronto. There he would replace Leopold Infeld, the Polish physicist whose presumed Communist sympathies had created certain difficulties that eventually persuaded him to leave Canada and return to his native Poland.

UNIVERSITY OF TORONTO (1951-1957)

Robinson always tried to write at a constant pace, "three good pages" a day (Dauben, 1995, p. 185). As Abby and Renée sailed from Liverpool to Montreal on a Cunard liner in August of 1951, they not only went first class but in the course of the trip Robinson also completed the 25-page manuscript "On the Foundations of Dimensional Analysis." (The original manuscript dated "RMS *Franconia,* August/September 1951" is preserved among Robinson's papers in the Yale University archives.) Dimensional analysis, as he explained, was an especially useful tool for engineers and

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physicists, with important implications for the relation between a model and its full-scale counterpart. In theoretical applications dimensional analysis was useful, because it allowed the transfer of results from an experiment performed under one set of circumstances to another comparable set for which the experiment had not been performed. This all had strong affinities to the work for which Robinson would soon become noted, namely model theory and nonstandard analysis.

Robinson's wife was intent upon returning to the theatre, and she not only did some television work but also made a number of recordings, including a dramatic reading of *Medea*. In addition to working with the Canadian Broadcasting Corporation, Renée was a regular performer on a dramatic radio series, "The Craigs," in which she and the actor Josef Fürst (with whom she also made several films) played an Estonian couple, the "von Hohenfelds." At home the Robinsons would often entertain a very mixed group of mathematicians, actors, and producers.

At the University of Toronto Robinson worked in the Department of Applied Mathematics. The National Research Council took advantage of Robinson's arrival in Canada and invited him to give a short course of lectures on "Supersonic Wing Theory." While at the university his teaching included aerodynamics, fluid mechanics, and differential equations, the sort of courses he had been teaching at Cranfield. He sometimes taught a graduate course on supersonic wing theory and also taught basic introductory courses as well, including calculus and analytic geometry. Robinson also had a number of graduate students who worked on various aspects of mathematical physics.

Most of Robinson's publications during his first few years at Toronto dealt with such applied topics as supersonic airfoil design, especially for delta wings, a subject he pioneered

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during the war. One of his major efforts at Toronto was a book he wrote with one of his former students at Cranfield, John Laurmann. The book covered both subsonic and supersonic airfoil design under conditions of both steady and unsteady flow and was presented with "that heightened sense of structure and unity that was a characteristic of Abby's work" (Young, 1976, p. 311). Following the appearance of *Wing Theory,* Robinson's interests began to turn increasingly to mathematical logic and the results he had achieved in his dissertation at the University of London.

In August of 1952 Robinson participated in the second *Colloque de logique mathématique*, devoted to "Scientific Appreciation of Mathematical Logic." This was held at the Institut Poincaré in Paris, and Robinson's lecture, delivered in French, was later published as "L'application de la logique formelle aux mathématiques." The basic aim of the paper was to show how the generalized completeness theorem could be applied to algebraically closed fields of characteristic zero, a subject to which Robinson would return in a number of subsequent papers.

In connection with the International Congress of Mathematicians held in Amsterdam in 1954, Robinson also participated in a special independent symposium devoted to the "Mathematical Interpretation of Formal Systems," chaired by Arend Heyting. Robinson was invited to give a special lecture, which he devoted to "Ordered Structures and Related Concepts." Inspired by Tarski, Robinson showed in this paper how metamathematical principles could be used to prove the completeness of a real-closed ordered field without having to use Tarski's elimination procedure. Much of the work Robinson was doing at this time was related to algebraically closed fields, real-closed ordered fields, and model completeness, all of which were developed in a book he published in 1955: *Théorie métamathématique des ideaux.*

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This actually proved to be a transitional work for Robinson, as his interests evolved from his thesis on the *Metamathematics of Algebra* to another book that he published in 1956, *Complete Theories*. This later was recognized as "a milestone in the development of model theoretic algebra" (Keisler, 1977, p. vi). The book included such important concepts as model completeness, model completion, and the prime model test. Among the succinct, elegant results Robinson presented in this book was a proof of the completeness of real-closed fields. Although not all theories are model complete, for those that are (for example, the theory of algebraically closed fields may be regarded as a model completion of the theory of integral domains), the model completion of a given theory is unique.

Among the more important results Robinson published while at Toronto was a paper he submitted to *Mathematische Annalen*, "On Ordered Fields and Definite Functions." This provided a model theoretic proof of Hilbert's seventeenth problem, that a positive definite real rational function is a sum of squares of rational functions. Although Artin had proved the theorem in 1927, Robinson not only gave better bounds on the number of squares and their degrees, but as Simon Kochen later described the paper, "the main interest of the model theoretic proof lay in its extreme elegance and simplicity" (Kochen, 1976, p. 314). Robinson came back to this problem a few years later, and in "Some Problems of Definability in the Lower Predicate Calculus" he applied the idea of model completeness to field extensions, again studying uniform bounds on the number of squares in Hilbert's seventeenth problem, which led Robinson to a relativization of the concept of model completeness.

Although Robinson had been hired at Toronto to teach applied mathematics, he nevertheless managed to attract a small coterie of students interested in logic, including Paul

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Gilmore and Elias Zakon, both of whom went to Toronto specifically to work with him. Of these, A. H. Lightstone wrote his thesis with Robinson on "Contributions to the Theory of Quantification."

Impressed by the amount and quality of Robinson's work, the University of Toronto promoted him to the rank of professor in June of 1956, but this was not enough to keep him in Canada. Later that same year an offer came from the Hebrew University of Jerusalem. Would Robinson accept the chair in mathematics held by his former teacher Abraham Fraenkel at the Einstein Institute? Robinson could not resist the opportunity to live and work in Israel and promptly accepted the offer.

HEBREW UNIVERSITY, JERUSALEM (1957-1962)

When the Robinsons arrived in Israel in 1957, the country was barely a decade old. The War of Independence fought in 1948 had left Jerusalem divided between Jordan and Israel, with Hebrew University atop Mt. Scopus stranded in a demilitarized zone accessible only by convoy once every two weeks. As a result, until the 1967 Six Day War, which would reunite all of Jerusalem, Hebrew University was scattered throughout West Jerusalem in a patchwork of buildings. The administrative offices were in rented space in a former college attached to the Franciscan monastery of Terra Sancta, while the department of mathematics was located in a building at the northern edge of the King David Hotel. Robinson taught several undergraduate courses on linear algebra and hydrodynamics, as well as an advanced course in logic that he taught with his old teacher Abraham Fraenkel.

Among Robinson's first graduate students was Azriel Levy, who had just finished his master's thesis on "The Independence of Various Definitions of Finiteness" (directed by Fraenkel). Robinson joined Fraenkel as Levy's disserta-

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tion advisor, and in 1958 Levy completed his thesis on "Contributions to the Metamathematics of Set Theory." By then, coinciding with the tenth anniversary of the founding of Israel, a new campus of Hebrew University was officially opened on Givat Ram, where Manchester House served both the departments of mathematics and of theoretical physics.

In addition to his teaching at the university, Robinson was invited to teach a course on fluid dynamics at the Weizmann Institute (in Rehovot) in the spring of 1959. Robinson still maintained a strong interest in applied mathematics, and while at Hebrew University he also contributed an article on "Airfoil Theory" to Wilhelm Flügge's *Handbook of Engineering Mechanics*. Over the course of his career, nearly one-half of his publications, including one book, were devoted to aerodynamics and the mathematics of structures. As Simon Kochen has suggested, "I believe that the thread that runs through all his work lies precisely, in fact, in this aspect: that also as a mathematical logician, his viewpoint was that of an applied mathematician in the original and best sense of that phrase." By this Kochen meant that the problems set by physical phenomena naturally inspire new ideas in the mathematician, or as Kochen put it, "To logicians, it is the world of mathematics which is the real world" (Kochen, 1976, p. 313).

Among the theoretical areas on which Robinson was working while in Israel was local differential algebra. He was especially interested in expanding upon earlier work of Joseph Ritt, in particular on matters of initial and boundary values. Again, Robinson's successes were due to his model theoretic approach, about which he talked at a meeting of the Union of Italian Mathematicians in Naples in the summer of 1959. Following the meeting in Italy he attended an international symposium on foundations of mathematics in Warsaw, a meeting devoted to infinitistic methods in the

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foundation of mathematics. There Robinson spoke on "Model Theory and Non-Standard Arithmetic." This meeting also afforded Robinson a chance to return to his hometown of Walbrzych and visit the grave of his father, who was buried in the Jewish cemetery.

When Fraenkel retired as chairman of the Mathematics Department in 1959, it was Robinson who assumed his position. Robinson was especially interested in curriculum reform and was instrumental in adding a new B.S. degree that had not previously been offered. He was also serious about replacing the old European system of evaluations of students at the end of their studies with course-by-course examinations. He likewise helped to abolish the tradition whereby faculty members worked on their own with one or more teaching assistants, and instead emphasized greater cooperation between faculty with different specialties and in different departments. This was by no means an easy matter, "since there were so many opposed to it . . . and its eventual success was due in large measure to Robinson's efforts as the 'living spirit' behind the new curriculum" (see Dauben, 1995, p. 272).

Meanwhile, Robinson was at work writing up his results on differentially closed fields, which he published in the *Bulletin of the Research Council of Israel.* Using Seidenberg's elimination techniques, Robinson showed how it was possible to give a model completion for the axioms of differential fields. The differentially closed fields could then be taken as models of the "closure" axioms associated with the completion. As George Seligman has said, reflecting the views of Angus Macintyre, "it would be appropriate to say that he *invented* differentially closed fields" (Seligman, 1979, p. xxiv).

In 1960 Alonzo Church was on sabbatical leave from

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Princeton, and Robinson was invited to spend the year as a visiting professor in the Department of Mathematics. The most remarkable result of that year was Robinson's creation of nonstandard analysis. He had been thinking about Skolem's approach to nonstandard arithmetic for some time, when one day as he walked into Fine Hall at Princeton the idea of nonstandard models for *analysis* suddenly flashed into his mind. A plenary lecture he had agreed to give at the silver anniversary meeting of the Association for Symbolic Logic in January of 1961 proved a suitable occasion to make public his new idea, and in the course on "Non-Standard Arithmetics and Non-Standard Analysis" he outlined how it was possible to provide a rigorous foundation for the calculus using infinitesimals. He communicated this almost immediately to Arend Heyting, and soon Robinson's first paper on the subject was published in the *Proceedings of the Netherlands Royal Academy of Sciences.*

In his paper Robinson explained how Skolem had shown the existence of proper extensions of the natural numbers *N* that possessed all of the properties of *N* formulated in the lower predicate calculus. Such extensions provided nonstandard models for arithmetic, and Robinson had the bright idea of taking the same approach to the real numbers *R*. He soon provided a much fuller account of nonstandard analysis in his book *Introduction to Model Theory* (see below).

While in the United States, Robinson spent several months working on an appropriate nonstandard language for nonstandard arithmetic at the University of California at Berkeley. He was also invited to Southern California by the Philosophy Department at the University of California at Los Angeles, where the Robinsons were impressed by both the university and the climate. As Angus Taylor recalls, it was about this time that the idea of offering Robinson

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a joint appointment in philosophy and mathematics began to take shape. Robinson was especially interested in the "concentration of good people" working in logic at UCLA and elsewhere in California (Robinson to DeLury, April 18, 1961, cited in Dauben, 1995, p. 292). The idea of inheriting Rudolf Carnap's chair was also a powerful factor in the decision to go to UCLA.

Robinson had one last year to spend at Hebrew University before moving to California, and he used it in part to work with several of his graduate students who were finishing their dissertations, including Shlomo Halfin's on "Contributions to Differential Algebra" and Amram Meier's on "Analytic Continuation by Summability and Relations Between Summability Methods." He also finished a revised version of his first book, *On the Metamathematics of Algebra,* to which he added many of the latest model theoretic developments produced in the decade following its publication in 1951. He was especially unhappy that his most basic insight—that important concepts of algebra possess natural generalizations within the framework of model theory—had not gained wider acceptance, something he hoped the new book would rectify. When *Introduction to Model Theory and to the Metamathematics of Algebra* appeared in 1963, the second half was almost entirely new and included a substantial section on nonstandard analysis.

Leaving Israel was not an easy decision for Robinson, for it meant not only leaving his colleagues and students at Hebrew University but also the country to which he was so deeply attached both intellectually and emotionally. Being in Israel had also given him the chance to reconnect with his brother, Saul. When Abby and Renée arrived in Israel in 1957, Saul and his wife, Hilde, were living in Haifa, and the two brothers enjoyed being able to talk again at length about philosophy and education, in which they were espe-

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cially interested. From time to time they would also make trips together with their wives to interesting archeological sites like Roman Caesarea.

UCLA AND NONSTANDARD ANALYSIS (1962-1967)

Robinson's appointment at UCLA was unusual in that he was a full member of two departments, mathematics and philosophy, which meant that he found himself serving on committees and working with students in both departments. Philosophy, logic, and mathematics had a significant prior history at UCLA, including a year Bertrand Russell spent there in 1939-40. A year earlier (1938) Hans Reichenbach had joined the faculty. Rudolf Carnap followed Reichenbach in 1953, and Robinson succeeded Carnap little less than a decade later.

At UCLA Robinson taught a seminar in logic in the Mathematics Department and an introduction to philosophy of mathematics in the Philosophy Department. He also taught an introductory course on modern logic in the Philosophy Department, and even a general course on the philosophy of science. In the Mathematics Department he also taught a one-year course on axiomatic set theory and another on applications of logic to analysis. From the beginning he was active in the Logic Colloquium at UCLA, founded by C. C. Chang and Richard Montague, which alternated its meetings between the philosophy and mathematics departments. Robinson especially impressed David Kaplan, one of his colleagues in philosophy, because "he talked philosophy the way philosophers did" (David Kaplan, quoted in Dauben, 1995, p. 316).

In 1963 the annual meeting of the Association for Symbolic Logic was held at the University of California at Berkeley, where Robinson presented a paper "On Some Topics in Nonstandard Analysis." This was devoted in part to estab-

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lishing both old and new results in the theory of functions of a complex variable, including a definition of summability that linked Banach-Mazur limits with the theory of Toeplitz matrices. These results and more were elaborated in a paper Robinson published the following year in the *Pacific Journal of Mathematics*.

Shortly after his arrival at UCLA Robinson's book *Introduction to Model Theory and to the Metamathematics of Algebra* appeared in the spring of 1963, and was described by one reviewer as "the first attempt to write a connected exposition of the new subject of model theory" (Engeler, 1964). The most obvious innovation of the book was the material devoted to Robinson's new creation, nonstandard analysis, and this was the avenue by which many mathematicians first came to appreciate the strength and potential of what he had accomplished. As Ernst Straus put it, nonstandard analysis "reached its next flowering in those years at UCLA." Robinson was quite excited about the potential of his new ideas and often quoted Fraenkel, who said, "Nobody achieves a good mathematical result after the age of 30." Robinson was very pleased, according to Straus, "that his best ideas came to him, at least by Fraenkel's standard, in an advanced age" (Ernst Straus, quoted in Dauben, 1995, p. 319).

Robinson spent a part of the summer of 1963 again at Berkeley, participating in an international symposium on the theory of models. He had helped to organize the meeting, which was intended to offer not only a comprehensive overview of model theory but also to achieve some preliminary consensus about uniformizing terminology and notation for the newly developing field. Robinson's contribution was devoted to "Topics in Non-Archimedean Mathematics," in which he explained how, building on the ideas of Thoralf Skolem, he had succeeded in producing a

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model of the real numbers that included both infinitesimals and infinite nonstandard numbers. Among applications he offered were theorems devoted to the analytic theory of polynomials, complex functions, topological spaces, normed linear spaces, and spectral theory.

In the summer of 1964 Robinson presented one of the most remarkable early results of nonstandard methods. At a NATO advanced study institute held at the University of Bristol in July of 1964, Robinson explained how he and Allen Bernstein, one of his graduate students at UCLA, had just solved the invariant subspace theorem in Hilbert space for the case of polynomially compact operators. As C. C. Chang notes, this result "instantly rocked the mathematical world, so to speak, although many said a standard proof would soon turn up" (C. C. Chang, quoted in Dauben, 1995, p. 327). Indeed, Paul Halmos shortly thereafter found a standard proof of the theorem by distilling the basic property that "did the trick" for Bernstein and Robinson, which Halmos called "quasitriangularity" (Halmos, 1985, p. 204).

Later that summer Robinson also participated in the International Congress for Logic, Methodology, and Philosophy of Science, held in Jerusalem. Robinson spoke on "Formalism 64" and offered his most recent views on the foundations of mathematics, explaining how he had matured from the Platonic realism he had adopted as a student to favor a more formalistic point of view. The title of his paper was meant to indicate a connection with the formalism of David Hilbert but with revisions that updated the subject to 1964, largely in light of Robinson's experience as a logician and especially in terms of model theory. As Robinson pointed out, when Hilbert advanced the elements of his own particular version of formalism in the 1920s, he could not have known that it was "doomed to failure," which only became apparent thanks to the work of Kurt Gödel in

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the following decade. Paul Cohen's results in the 1960s, however, which showed the independence of the continuum hypothesis (just a year earlier, in 1963), also influenced Robinson's views considerably, as did ongoing discussion over various axioms of infinity among logicians.

Although Robinson rejected any reference to infinite totalities as meaningless, he nevertheless believed mathematicians should "continue the business of Mathematics as usual, i.e. we should act as if infinite totalities really existed." Here Robinson was clearly comfortable adopting a very Leibnizian position, accepting infinite concepts as "*fictiones bene fundatae*" (Körner, 1979, p. xiii). By this Robinson meant that any statement about an infinite totality was "meaningless" in the sense that "its terms and sentences cannot possess the direct interpretation in an actual structure that we should expect them to have by analogy with concrete (e.g., empirical) situations." He also held that the rules of logic were not arbitrary and that the laws of contradiction and the excluded middle, for example, were "basic forms of thought and argument which are prior to the development of formal Mathematics."

In the fall of 1964, as chair of the University of California's Educational Policy Committee, Robinson was automatically a member of the university-wide Academic Council, chaired by the mathematician Angus Taylor. Primarily responsible for advising the university's president, Clark Kerr, one of the council's major concerns that year was the Berkeley Free Speech Movement, set against the background of both the Civil Rights Movement and the Vietnam War. This eventually pitted faculty and students against the administration, which also had to report to the Board of Regents, which in turn was answerable to then governor of California, Ronald Reagan. Although Robinson was distressed by the anti-intellectual tendency he perceived

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in the Free Speech Movement, he nevertheless opposed any restriction upon lawful speech or advocacy on any of the University of California campuses. (Robinson in a letter to Theodore R. Meyer, chairman of the Special Committee to Review University Policies, February 3, 1965, cited in Dauben, 1995, pp. 339-40).

Despite the political tensions that dominated the University of California that year, Robinson finished a new book on *Numbers and Ideals*, a slight work of barely 100 pages, yet one he hoped would stimulate interest among students in the simplicity and beauty of abstract algebra. This was followed a year later by *Nonstandard Analysis*, which offered the most powerful demonstration yet of Robinson's basic premise: that mathematical logic could benefit mathematics proper, especially model theory and nonstandard analysis, which provided, as he noted in the book's preface, "a suitable framework for the development of the Differential and Integral Calculus by means of infinitely large numbers."

Infinitesimals, Robinson insisted, "appeal naturally to our intuition." Using nonstandard analysis, he proved a wide variety of results, including basic theorems from the calculus, differential geometry, nonmetric topological spaces, Lebesgue measure, Schwartz distributions, complex nonstandard analysis, analytic theory of polynomials, entire functions, linear spaces (including Hilbert space), along with nonstandard spectral theory of compact operators, topological groups, and Lie groups. He also suggested applications to theoretical physics, and he even suggested that the discovery of nonstandard analysis required a rewriting of the history of mathematics, at least where the history of the calculus was concerned.

A year earlier, in June of 1965, Robinson had been invited to deliver the keynote address at an international collo-

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quium devoted to philosophy of science held at Bedford College in Regent's Park, London. There he presented his views on "The Metaphysics of the Calculus," in which he argued that limits were not the best foundation for the calculus but that infinitesimals were. Robinson included a critique of weaknesses in Cauchy's approach to the calculus based on limits, all of which promoted considerable discussion.

By 1965 Robinson had decided that trying to work effectively in two departments at once was too much of a strain and demanded too much of his time. He thus gave up his position in philosophy, and moved full-time to the Mathematics Department. Meanwhile, he had been invited by John Crossley to visit St. Catherine's College, Oxford, for the fall semester of 1965, where logic as Crossley put it was "booming" (John Crossley in a letter to the author, July 5, 1991, cited in Dauben, 1995, p. 369). In the course of the term Robinson gave a regular series of lectures on nonstandard analysis. As John Bell recalls, "The lecture hall was packed—the audience included Moshé Machover, Alan Slomson, Peter Aczel, John Wright, Frank Jellett, John Crossley, and Joel Friedman. These lectures were very absorbing—it was obvious that Robinson was presenting something of fundamental importance—and were delivered with, what I can only describe as, an endearing lack of slickness" (John Bell to the author, March 12, 1994, cited in Dauben, 1995, pp. 370-71).

Following his term at Oxford, Robinson lectured on nonstandard analysis at the University of Paris, where he was appointed a *Professeur associé* at the Institut Henri Poincaré. Interest in nonstandard analysis was growing in France, especially in Strasbourg, where an active group of logicians formed around Georges Reeb, who was known to proclaim "in every corridor that nonstandard Analysis was 'something really new, an actual revolution'" (Lutz and Goze,

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1981, p. vi). Robinson also lectured on nonstandard analysis at the Castelnuovo Institute at the University of Rome, which resulted in two papers in the *Proceedings of the Accademia dei Lincei*, both devoted to Dedekind domains in the theory of algebraic numbers. He also published "On Some Applications of Model Theory to Algebra and Analysis" in the *Rendiconti de Matematica e delle sue Applicazioni*.

Back at UCLA, the administrative turmoil and constant department meetings began to take their toll, as did the continuing financial woes of the entire university. Enrollments at UCLA had increased to nearly 30,000 students. Demonstrations against the Vietnam War continued, and Robinson joined Donald Kalish for one of the silent vigils organized by UCLA's Vietnam Day Committee. Financially the university was facing severe cutbacks, and faculty salaries were no longer competitive on a national scale. While still at Oxford Robinson had received a letter from Nathan Jacobson at Yale. A position in logic was being transferred from philosophy to mathematics, and on the advice of an ad hoc committee (consisting of Church, Kleene, Wang, and Montague), Jacobson wrote to say, "we feel very strongly that you would be the ideal person to help us to develop in this new direction for our Department" (Nathan Jacobson to Robinson, November 29, 1965, cited in Dauben, 1995, pp. 371-72). After some initial hesitation Robinson eventually decided to accept Yale's offer.

In the summer of 1966 Robinson returned to Tübingen where Peter Roquette had arranged a guest professorship. This resulted in a month-long course on the fundamentals of model theory, which included applications to both algebra and nonstandard analysis. When he returned to UCLA for the fall term, Robinson taught a course on set theory, another on lattice theory and Boolean algebras, and a three-

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quarter survey of model theory, decidability and undecidability, and recursive functions.

The highlight of Robinson's last year in California was an ambitious international symposium on applications of model theory to algebra, analysis, and probability, organized by W. A. J. Luxemburg as the culmination of a semester-long seminar on nonstandard analysis at Caltech. This meeting brought together for the first time a large number of mathematicians, all of whom were making use of nonstandard analysis, and the list of participants was impressive. Robinson used the occasion to talk about "Topics in Nonstandard Algebraic Number Theory," which concentrated on the further development of class field theory of infinite algebraic number fields, which he related to earlier classical results of Chevalley and Weil. Robinson finished his final summer in Los Angeles with an institute on axiomatic set theory at UCLA, which he co-organized with Paul Cohen and Dana Scott. This was actually the fourteenth in a series of annual summer research institutes sponsored by the American Mathematical Society and the Association for Symbolic Logic, with financial support from the National Science Foundation. The meeting was dedicated to Kurt Gödel, who had been invited to attend but declined for reasons of ill health.

While at UCLA Robinson had a diverse group of graduate students, and of those who wrote their dissertations with him, the ones who obtained the most significant results were Allen R. Bernstein, "Invariant Subspaces for Linear Operators" (1965); and William M. Lambert, "Effectiveness, Elementary Definability, and Prime Polynomial Ideals" (1965). Subsequently, Robinson worked with Larry E. Travis on "A Logical Analysis of the Concept of Stored Program: A Step Toward a Possible Theory of Rational Learning" (1966); Joel Friedman, "A Set Theory of Proper Classes" (1966); and Lawrence D. Kugler, "Nonstandard Analysis of Almost

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Periodic Functions" (1966). Robinson's last three students at UCLA were Peter G. Tripodes, who wrote on "Structural Properties of Certain Classes of Sentences" (1968); Robert G. Phillips, "Some Contributions to Non-Standard Analysis" (1968); and Diana L. Dubrovsky, "Computability in *p*-adically Closed Fields and Nonstandard Arithmetic" (which she defended in 1971).

Robinson's decision to leave UCLA for Yale after the spring term of 1967 was influenced by many factors, but among them certainly were issues of prestige—and style:

> A smaller private university offered him close contact with all members of a distinguished senior faculty in mathematics, a regular flow of talented postdoctoral researchers and graduate students, and the opportunity for easy contact with scholars in fields other than his own. No doubt his awareness of tradition lent appeal to the case of a university nearly twice as old as the oldest of his previous affiliations (Seligman, 1979, pp. xxvii-xxviii).

YALE UNIVERSITY, 1967-1974

At Yale Robinson joined a distinguished faculty in the Department of Mathematics, which at the time included, among others, Walter Feit, Nathan Jacobson, Shizuo Kakutani, William Massey, George Mostow, Oystein Ore, Charles Rickart, and George Seligman. Michael Rabin had prepared the way for Robinson by teaching a course the previous semester on model theory. With Robinson now at Yale he served as a magnet for other logicians, and soon he had established a strong group of postdoctoral students, including Jon Barwise, Paul Eklof, Manuel Lerman, James H. Schmerl, Stephen Simpson, Dan Saracino, and Volker Weispfenning. Established mathematicians also came to Yale for visits of a semester or longer, like Azriel Levy, Gerald Sacks, and Gabriel Sabbagh. As George Seligman has described Robinson's influence at Yale: "Graduate students were charmed and excited by the

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promise held out for model theory and nonstandard methods in analysis and arithmetic by his courses, and many sought him out as adviser. He refused none and gave generously of his attention to all" (Seligman, 1979, p. xxviii).

It was not long after his move to Yale that Robinson was elected to a two-year term as president of the Association for Symbolic Logic (1968-70). One of the major responsibilities of the Association was overseeing publication of its *Journal of Symbolic Logic*. Robinson was also interested in ways the Association could serve to promote logic in parts of the world where it had not as yet been established. Among the efforts Robinson championed during his presidency was development of mathematical logic in Japan and Latin America, where the Association, with support from the National Science Foundation, sponsored a number of special logic seminars and summer schools. Thanks in part to Robinson's efforts and these early meetings the subject began to win a progressively stronger foothold in Japan and throughout Latin America.

In the summer of 1968 Robinson spent a month at the University of Heidelberg, where he offered a series of lectures on "Model Theory and its Applications." This also gave him an opportunity to work with his colleagues Gert Müller and Peter Roquette. By then the Six Day War had reunited Jerusalem, and Abby and Renée were pleased to spend most of August back in Israel. At the end of the month Robinson presided over a two-day meeting of the Association for Symbolic Logic in Warsaw, during a week's conference devoted to "Construction of Models for Axiomatic Systems." From Poland they went on to Italy for a meeting at Varenna, where Robinson gave a one-week introduction to model theory. Building on the work of Ax and Kochen with respect to Artin's results on p-adic zeros of forms over p-adic fields, Robinson used nonstandard analysis to produce

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"one of the most striking applications of model theory to date" in order to prove in a direct way the Ax-Kochen theorem.

Back at Yale one of the first courses Robinson taught was "Chapters in the History of Mathematics," offered in the spring of 1968. Subsequently, in a Festschrift to honor Arend Heyting he chose to consider in partly historical terms the "ultimate foundation" for mathematics. Just as non-Euclidean geometry destroyed faith in Euclidean geometry as the one true geometry of space, so too, Robinson held, did the results of Gödel and Cohen destroy any faith one might have had in the existence of a single, absolutely true set theory. Thus both standard and nonstandard versions of arithmetic and analysis were possible, which served to reinforce the reasonableness of a formalist foundation for all of mathematics.

The following summer, 1969, Robinson was back in Heidelberg, to work again with Peter Roquette on nonstandard number theory. By now the two had been collaborating for some five or six years and had found that they could greatly simplify parts of Siegel's work, which the nonstandard approach made "manageable" (G. D. Mostow, quoted in Dauben, 1995, p. 419).

In the spring of 1970 while on leave from Yale, Robinson was invited to give three Shearman lectures back at his alma mater, the University of London. These he devoted to "Logic as the Science of Mathematical Reasoning." He also gave a second series of lectures for the mathematics department on nonstandard analysis. At this time the Mathematical Association of America released a film it had made with Robinson, an hour's introductory lecture on nonstandard analysis. Following a straightforward account of formal languages and mathematical logic, whereby he introduced the nonstandard, non-Archimedean continuum R* of nonstand-

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ard real numbers, Robinson went on in the second part of the film to show how derivatives, integrals, and limits of sequences could all be expressed in terms of R*.

Early in June 1970 the Robinsons flew to Norway, where the Second Scandinavian Logic Symposium, sponsored by the Association for Symbolic Logic, was being held in Oslo. Robinson was among the 30 participants, and talked about recent developments due to "Infinite Forcing in Model Theory." What Robinson did was to develop Paul Cohen's method of forcing in set theory within the context of model theory. What was new in his own approach, Robinson explained, was that he was able to introduce "a kind of compactness theorem for forcing," along with an axiomatization of classes of generic structures by infinitary sentences. This was related to work he had been doing with his colleague at Yale, Jon Barwise. Somewhat later Jerome Keisler summarized the significance of all this as follows:

> Robinson showed that the generic models constructed by his forcing are closely related to model completions. If a theory has a model completion, then it must be the set of sentences true in all generic models of the theory. Whether the theory has a model completion or not, all generic models are existentially closed in the theory. These results created new interest in model completeness and suggested many questions in particular areas of algebra. As a result there has been a substantial increase recently in research activity in the whole area of model-theoretic algebra (Keisler, 1979, p. xxxv).

From Oslo Robinson went on to Chile, where he was a major figure in the first Latin American meeting on logic. From Latin America he returned to Europe to spend a few days in Heidelberg seeing Peter Roquette and Gert Müller before going on to the Mathematical Research Institute in Oberwolfach for the first international meeting on nonstandard analysis. This had been organized by W. A. J. Luxemburg and Detlef Laugwitz, and brought together

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virtually all of the pioneers of the subject, including Robinson, of course, as well as Larry Kugler, Jerome Hirschfeld, Moshé Machover, Keith Stroyan, and Peter Loeb, who found Robinson's performance at the meeting "charismatic" (Peter Loeb, quoted in Dauben, 1995, p. 434). Following Oberwolfach, Robinson was in Nice for the international congress of mathematicians, where he gave an invited hour-lecture on "Forcing in Model Theory."

Among Robinson's graduate students at Yale, Gregory Cherlin was working on infinite forcing, developed from the point of view of generic hierarchies that resulted in his thesis, "A New Approach to the Theory of Infinitely Generic Structures." Cherlin's thesis also drew upon results of another of Robinson's graduate students, Carol Wood, who was also working on forcing but in terms of higher-order languages rather than first order logic. Her thesis, "Forcing for Infinitary Languages," dealt with differentially closed fields of characteristic $p \neq 0$, for which she established the existence of a model companion.

Tarski's seventieth birthday was celebrated at Berkeley in June of 1971 with a special two-day symposium, where Robinson talked about model theory in relation to an early paper of Tarski's, "A Decision Method for Elementary Algebra and Geometry," published in 1948. As a tribute to Tarski, Robinson wanted to show how "the successive widening of our model theoretic point of view has shed new light on Tarski's result." Robinson was able to do so through the introduction of the theory of existentially complete and generic structures.

At the end of the summer Robinson not only delivered the twentieth series of Hedrick lectures at the summer meeting of the Mathematical Association of America (on "Nonstandard Analysis and Nonstandard Arithmetic") but he also attended the Fourth International Congress for Logic,

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Methodology, and Philosophy of Science in Bucharest, where he spoke on "Nonstandard Arithmetic and Generic Arithmetic," which presented work he and his graduate students at Yale had been doing on model companions and related topics.

When Robinson returned to New Haven in the fall of 1971, he had been made a Sterling Professor, a prestigious named professorship at Yale. Among the applications of nonstandard analysis that had begun to interest Robinson in the early 1970s were results he and his colleague at Yale Donald J. Brown had obtained for "nonstandard economies," where the effect of any individual in an infinite economy of traders might be taken as infinitesimal. This led to several joint papers, including "A Limit Theorem on the Cores of Large Standard Exchange Economies" and its sequel "Nonstandard Exchange Economies," which appeared in *Econometrica* and summarized virtually all of the results Robinson and Brown had achieved.

Among those whom Robinson invited to Yale as a visiting professor was Gabriel Sabbagh, who spent the fall of 1972 in New Haven. Angus Macintyre had also accepted a position at Yale that same year as an associate professor, which meant that Yale was fast becoming one of the most stimulating centers for mathematical logic in the world. Robinson was away for a part of the year at the Institute for Advanced Study.

When Robinson retired from his presidency of the Association for Symbolic Logic in 1973, he was asked to give an hour lecture at its annual meeting, which he devoted to "Metamathematical Problems." In his lecture Robinson sought to pose 12 open problems or areas in mathematics that he believed would require logic and model theory for their solutions. Meanwhile, he was meeting with Gödel at Princeton, where they discussed their mutual interests in mathematics and logic. Gödel was especially impressed by nonstandard

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analysis and its potential applications in other parts of mathematics. He had suggested in fact that Robinson come to the Institute for an extended period of time, and even hoped that Robinson might one day be his successor (Robinson to Gödel, April 14, 1971; Gödel papers #011957, Princeton University archives; cited in Dauben, 1995, p. 458). On the subject of nonstandard analysis Gödel had the following to say:

> In my opinion Nonstandard Analysis (perhaps in some non-conservative version) will become increasingly important in the future development of Analysis *and* Number Theory. The same seems likely to me, with regard to all of mathematics, for the idea of constructing "complete models" in various senses, depending on the nature of the problem under discussion (Gödel to Robinson, December 29, 1972; Gödel papers #011962, Princeton University archives; cited in Dauben, 1995, p. 459).

While at the Institute, Robinson was working on a paper dedicated to Andrzej Mostowski for his sixtieth birthday. Of the "metamathematical problems" Robinson had raised in his Association for Symbolic Logic presidential address, he now turned to answer some of the questions he had posed on the "emerging field" of topological model theory. He also wrote another commemorative article that he dedicated to his colleague A. I. Mal'cev, "On Bounds in the Theory of Polynomial Ideals."

The most significant honor of Robinson's entire career was conferred in April of 1973 when he was awarded the L. E. J. Brouwer Medal by the Dutch Mathematical Society. The first recipient, three years earlier, had been the French pioneer of catastrophe theory, René Thom. Robinson's Brouwer lecture was devoted to "Standard and Nonstandard Number Systems" and constituted a mathematical tour of the major highlights of his best-known results, showing the power of nonstandard approaches to mathematics in general. In his Brouwer lecture Robinson also articulated

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further his views on the foundations of mathematics, which now reflected his experience with nonstandard number systems in particular:

> [Nonstandard analysis] does not present us with a single number system which extends the real numbers, but with many related systems. Indeed there seems to be no natural way to give preference to just one among them. This contrasts with the classical approach to the real numbers, which are supposed to constitute a unique or, more precisely, categorical totality. However, as I have stated elsewhere, I belong to those who consider that it is in the realm of possibility that at some stage even the established number systems will, perhaps under the influence of developments in set theory, *bifurcate* so that, for example, future generations will be faced with several coequal systems of *real numbers* in place of just one.

Robinson spent part of the summer of 1973 back in Heidelberg. Gert Müller had invited him to spend a week with the model theoreticians in Müller's seminar, but this also gave Robinson a chance to meet with Roquette's group, which took advantage of his visit as well. This also allowed Robinson and Roquette to continue their collaboration on nonstandard number theory. Together they were working on nonstandard approaches to diophantine equations, in particular C. I. Siegel's theorem on integer points on curves. Kurt Mahler had generalized the theorem, allowing for certain rational as well as integer solutions. By exploiting the idea of enlargements, specifically of an algebraic number field in a nonstandard setting, Robinson and Roquette hoped that nonstandard methods would help them to go beyond the results Siegel and Mahler had obtained. As Roquette later explained, "These ideas of Abraham Robinson are of far-reaching importance, providing us with a new viewpoint and guideline towards our understanding of diophantine problems" (Roquette and Robinson, 1975, p. 424).

From Heidelberg Robinson flew to Bristol for a European meeting of the Association for Symbolic Logic, where

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he discussed problems of foundations. Despite the great advances made in contemporary mathematics, where technical developments in particular had been "spectacular," he was concerned about how little the essential nature of mathematics itself had been illuminated with respect to the problem of infinite totalities ontologically. His own response was basically a formalist one:

> I expect that future work on formalism may well include general epistemological and even ontological considerations. Indeed, I think that there is a real need, in formalism and elsewhere, to link our understanding of mathematics with our understanding of the physical world. The notions of objectivity, existence, infinity, are all relevant to the latter as they are to the former (although this again may be contested by a logical positivist) and a discussion of these notions in a purely mathematical context is, for that reason, incomplete.

Robinson ended the summer of 1973 back in Princeton with a brief visit, again to see Gödel. When the fall term began at Yale, Robinson offered a course with the philosopher Stephan Körner on the "Philosophical Foundations of Mathematics." He later confided to Körner that he doubted if the students were enjoying the seminar, "because we are enjoying it too much" (Stephan Körner in a letter to Renée Robinson, cited in Dauben, 1995, p. 471).

In the end Robinson expressed his philosophy of mathematics in his usual light-hearted way in an account he wrote for the *Yale Scientific Magazine* (47, 1973), "Numbers—What Are They and What Are They Good For?"

> Number systems, like hair styles, go in and out of fashion—its what's underneath that counts.

He explained this in part as follows:

> The collection of all number systems is not a finished totality whose discovery was complete around 1600, or 1700, or 1800, but that it has been and still

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is a growing and changing area, sometimes absorbing new systems and sometimes discarding old ones, or relegating them to the attic.

Nonstandard analysis and Robinson's nonstandard real numbers were just another step in the continuing evolution of mathematics, which served to broaden and deepen the number systems available to mathematicians and logicians alike.

When Robinson returned to Yale in the fall of 1973, he had been experiencing stomach pains and finally underwent a series of tests at the end of November. "These gave abundant grounds for suspecting cancer of the pancreas, and an exploratory operation revealed that the disease was beyond surgical remedy" (Seligman, 1979, p. xxxi). Robinson began to cancel commitments, lectures he had agreed to give, and meetings he had hoped to attend, but he continued to meet with his students.

The class was removed to his modest office, where a dozen or so hearers crowded in. The disease and the drugs forced him to struggle to concentrate, but his wit still could flash out, and his listeners' laughter would then fill the narrow corridor outside his office (Seligman, 1979, p. xxxi).

Robinson was not able to withstand the progressive advance of the cancer, and in April he was forced to cancel his one class and return to the Yale Infirmary. Shortly thereafter he died quietly in hospital on April 11, 1974. A few days earlier he had just been elected a member of the National Academy of Sciences.

CONCLUSION

Robinson once said that "playfulness is an important element in the makeup of a good mathematician," and he was certainly a mathematician who enjoyed his work to the fullest. He was also happy to remind people that his own

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career was the perfect counter-example to the old myth that mathematicians do their best work before they are 30, at the beginning of their careers. Robinson had indeed produced excellent work at the beginning of his own career, but his best-known and most often discussed work was done well after he was 40. It can even be said that Robinson was only just beginning to develop the potential of nonstandard analysis and model theory when he died so prematurely at the age of 55.

Robinson was in many respects a universal mathematician, at home in many fields and thus able to exploit the power of model theory in many different areas. As one sympathetic to the work of applied mathematicians as well as the most theoretical, he was also interested in finding applications of nonstandard analysis in a host of disciplines, from quantum physics to economics. And yet as the work he did at Yale clearly shows, he was not only aware of its powerful applications in certain contexts, but he appreciated the fact that it was historically revolutionary as well. As a tool, however, it required an experienced hand, and he was among the few who knew other parts of mathematics well enough to know where nonstandard analysis might be most helpful, or even essential. As he once told Greg Cherlin, "At first it was easy to get results—now you have to do more" (Gregory Cherlin, cited in Dauben, 1995, p. 492).

In the course of his 55 years Robinson accomplished more than most can claim to have accomplished in far longer lifetimes. Indeed, he was a man who made mathematics a thing of beauty, and equally important, he had the remarkable ability to reveal that beauty to all who wished to learn from his example.

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Donald C. Shreffler

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DONALD C. SHREFFLER

April 29, 1933–August 8, 1994

BY CHELLA DAVID

DONALD SHREFFLER'S STUDIES ON the mouse H2 system played a major role in shaping immunology. Today the major histocompatibility complex (MHC) genes are the focus of study in many areas of immunology. The MHC molecules shape the T cell repertoire in the thymus by positive and negative selection, and in the periphery they generate the CD8 and CD4 T cells restricted by the MHC molecules to play a critical role in immunity. Specific MHC molecules appear to predispose the individual carrying them to develop certain autoimmune diseases. Structure/function studies on the MHC genes, their products, and on their role in human immunity and disease are widespread. In the 1960s the MHC genes were only an obscure curiosity studied by a handful of scientists interested in tumors and transplantation. The pioneers of the mouse H2 system played a critical role in the genetic fine structure studies and in developing inbred, congenic, recombinant strains of mice and reagents that were essential in developing the field. Donald Shreffler was one of those pioneers who paved the way for the explosion that occurred in this field during the 1960s and 1970s. He also provided precious mouse strains and reagents, as well as advice, to many scientists initiating these experimental studies. His work in the mouse also enabled the

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rapid progression of studies on the human MHC genes, the HLA system. His place in the "Immunology Hall of Fame" is secure.

Shreffler sprouted from German roots that were transplanted to rural Illinois. His ancestors toiled and cleared the land and established a traditional German farm. Both the family and their farm flourished, and the Shrefflers became leading citizens in that area. They contributed to various community activities such as building churches, schools, and recreational centers. Don was born in the ancestral Shreffler home built in 1846. His father, Cecil, branched out into dairy farming and Don, who was the only boy in the family, became his father's right hand from the time he could walk. His farm chores included milking the cows in the morning and working in the fields in the afternoons. Sunday was a family day. They would spend the day with church activities, family gatherings, and for Don, playing with his cousins, especially cousin Norman. All of his aunts and uncles remember Don as a shy, serious young man who always did the right things and never got into trouble.

He attended a grade school built by his grandfather and after finishing his elementary education went to high school in Reddick, 15 miles away. That meant getting up even earlier in the morning to finish his chores before taking the bus to school, finishing his evening chores later, and then staying up to finish his homework. Nevertheless, he not only did very well in his schoolwork, but he also participated in extracurricular activities, especially the 4-H club and the Future Farmers of America group. He did not have time for organized sports, but did play clarinet in the school band and the piano accompaniments for the church choir. He graduated at the top of his class of 32 students. His cousin Norman remembers him as a humble person who

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preferred to be a face in the crowd at school rather than be recognized as the smartest one in his class.

Don loved farming and planned to join his father as a dairy farmer. In order to be closer to home he enrolled at the University of Illinois College of Agriculture. Besides studying and working part time to defray his college expenses he also found time to participate in intramural sports. Don graduated in 1954 with honors, but before he could rejoin his father on the farm, Uncle Sam called. He spent two years in the service, mostly in Japan, where he learned to appreciate foreign cultures and probably acquired his international outlook on life. After his service he returned to do graduate work in dairy science at the University of Illinois. During that time he met his future lifetime partner, Dorothy, a fellow student at the University of Illinois. It was love at first sight, and they soon married. Dorothy worked as a bookkeeper at the University bookstore while Don continued his graduate work. His master's thesis project was in hemoglobin variants in dairy cattle, and he published three papers from his research.

Don's thesis work gave him an appetite to learn more about biochemical genetics. He realized that state-of-the-art biochemical genetics research was being done by scientists at the California Institute of Technology (Caltech). He planned to stay at Caltech for one or two years to gain sufficient expertise and then to return to Illinois to complete his graduate work. But the longer he stayed, the more excited he became about the science going on at Caltech in the laboratories of professors Owen, Beadle, and Pauling. Finally, Don decided to stay at Caltech to complete his Ph.D. thesis in Ray Owen's laboratory. He obtained a National Science Foundation predoctoral fellowship for his thesis research on mouse plasma proteins. Ray Owen recalled that

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Don speedily settled into the research and teaching in our lab, and revised his intention of a limited stay. Turning mainly to mice as experimental animals, he began to define electrophoretic differences in plasma proteins among inbred lines, using Smithies' starch-gel techniques. Beginning with antisera that had been prepared by students in the immunology course lab, he defined repeatable genetic patterns in gel diffusion serology, especially in globulins. He also looked at the hemoglobins of monkeys and participated in much that went on in the lab. Our interest in the early days of bone marrow transplantation, immunological tolerance, and the cellular antigens of mice was enriched by extensive working visits from such leaders as Elizabeth Russell of Bar Harbor on the genetics of blood cell formation in mice, David Nanney and his student Eduardo Orias, and Jean-Marie Dubert, Eleanor Brandt, and John Loeffer on immunogenetics and mating in Tetyrahymena. Bill Stone, on leave from Wisconsin, looked for blood factors deriving from gene interaction, for human blood typing reagents from immune sera produced in cattle, and for effects of irradiation on the state of immunological suppression. Don worked with Prof. Jerry Vinograd on molecular hybridization of sheep hemoglobins, and studied serum protein types in mouse bone marrow chimeras. Sei Tokuda and Margaret MacGillivray were analyzing "parabiotic intoxication" and 'isoimmune anemia" in rats and mice. Much was going on in other labs at Caltech as well, and Don interacted with many of them, to [their] mutual benefit. He was an excellent teaching assistant, devoting time and attention to students and cultivating creativity and substance in wide samples of biological science.

As part of his duties as a teaching assistant for an undergraduate immunology class, Don had to demonstrate immunoprecipitation. He made antisera against mouse serum in rabbits for this demonstration. These antisera gave very strong precipitation lines against most inbred strains of mice, but he noticed a unique reaction with sera from a few mouse strains. He designated these as being high or low in a serum substance (Ss) protein. Mating of Ss high mice with the Ss low mice gave intermediate levels in the F1 and segregated for the high and low phenotype in the F2 population. He realized that all the strains that had the Ss low genotype carried one particular MHC-H2 gene, and eventually showed

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that the gene coding for the Ss protein was linked to the H2 locus. This finding was published in *Genetics* in 1963. Don and Dorothy enjoyed their time in Pasadena and also celebrated the birth of their first son, Doug. The group in Ray Owen's lab became part of their family, and they enjoyed the wonderful hospitality at Ray Owen's home.

After completing his Ph.D. at Caltech, Don had the option of either going back to the University of Illinois to continue his work on the immunogenetics of cattle or accepting a position at the University of Michigan to continue his work on the mouse Ss protein. His new discoveries in the mouse system convinced him to go to Michigan, where he was appointed research associate in the Department of Human Genetics. His studies on the mouse Ss protein revealed that the Ss gene mapped within the H2 gene complex in the seventeenth chromosome of the mouse. He established a close relationship with George Snell at the Jackson Laboratories in Bar Harbor, Maine. George, the "father of H2," became his mentor, and he visited with him frequently to exchange ideas. With the help of Snell and Jack Stimpfling, he established a mouse colony at Michigan consisting of inbred, congenic, and recombinant strains of mice. This was before the days of lab animal medicine, and his mice were housed in a small building that was referred to as the "Mouse House." These mice were bred and maintained by his students and technicians. As a member of the human genetics department he also collaborated with several of the faculty in the department (Neel, Brewer, Gershowitz, Weitkamp, Tashian, Rucknagel, and Sing) on various aspects of human biochemical genetics. During those early years at the University of Michigan he published several papers in these areas. In 1965 he was appointed assistant professor in the department. Shortly after coming to Michigan the Shrefflers welcomed their second son, David. Dorothy became

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a full-time mother and a solid home support for Don as he climbed the academic ladder.

One of Don's first two graduate students was Jane Schultz. Jane's Ph.D. research was on the human serum lipoprotein. Jane Schultz had finished her undergraduate work and had started a family. By the time she started her graduate work, she already had three small children. Don, who was the youngest faculty member in the department, accepted Jane into his laboratory and gave her all the support she needed to complete her research within a reasonable time. Jane remembers Don as magnanimous, allowing her to interact with people outside the laboratory and even outside the department, and providing her with constant advice. Don was a perfectionist, so Jane had to work very hard to come up with an outstanding thesis. Her studies were published in the *Proceedings of the National Academy of Sciences* (PNAS). Jane also remembers Don as being a very social person, joining the graduate students for beer at the local pubs and at impromptu parties at society meetings, and enjoying the hospitality at their home.

The thesis of Don's other graduate student, Howard Passmore, involved determination of structural differences between the Ss high and the Ss low protein. Howard generated alloantisera by cross-immunizing Ss high and Ss low mouse strains. He identified the presence of an antigen controlled by the Ss locus, expressed only in males, of the proper genotype. He designated this as "sex limited protein" (Slp). Immunological evidence suggested that the Slp antigenic sites resulted from structural variations of the Ss component of the mouse serum, either controlled by the same gene or by closely linked genes.

I met Don for the first time at a genetics meeting in 1968. I was a graduate student and because he looked so young, I assumed he was also one. I was amazed the next

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day when I realized he was the keynote speaker, and he gave a masterful presentation. After I finished my Ph.D. and started exploring postdoctoral opportunities I contacted Don through a mutual friend Bill Stone. After visiting him there was no question in my mind about where I wanted to do postdoctoral work. His hospitality, kindness, and humility touched me very much. I was his first postdoctoral fellow, and since I was only three years younger than he and had two children the same age as his boys, he treated me more like a colleague that a trainee. My Ph.D. research had been in the area of chicken immunoglobulins and my knowledge of mouse genetics and MHC was minimal.

Don took hours of his time to painstakingly teach me the intricacies of the H2 system as it was known during the late 1960s. During that first year I had a very hard time understanding the system, and my productivity was nil. Don continued to be patient with me and continued to guide and advise me during that difficult time. At laboratory meetings, even if I made a stupid comment, he never ridiculed or embarrassed me in front of the others. He would talk to me privately in a very nice way, suggesting that maybe what I had said was not correct. Don took me to an H2 meeting in Bar Harbor organized by George Snell. About 20 people involved in H2 research participated. Finally, I was beginning to understand this system. My assigned project was to map conclusively the exact location of the Ss gene within the H2 complex. By undertaking an extensive classical gene mapping study and producing several recombinant strains of mice within the various H2 genes, I mapped the Ss gene to the middle between the major H2 loci, H2K and H2D. During this process several recombinant strains of mice were also identified that were identical at H2K and H2D antigens but differed in the Ss protein.

In 1969 Jan Klein joined our group from Prague. Jan

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had been involved in extensive research on the H2 genes in Prague for several years at the Czech Academy of Sciences. He came to our group with a gold mine of knowledge in this area. His studies and his incredibly imaginative analysis of the data with Don resulted in a new hypothesis for the organization of the H2 genes. This was termed the two-gene model for the H2 system, with only two H2 loci, H2K and H2D with the Ss gene mapping in the middle. All the controversies regarding other minor H2 genes in the complex were determined to have resulted from cross-reactions between H2K and H2D antigens. This new model proposed that the H2 gene complex was much simpler than had been previously thought, and made the regulation and function of these genes and their products more comprehensible. This important paper was published in the *Journal of Experimental Medicine* in 1972.

During this time Hugh McDevitt at Stanford University had discovered immune response genes. He had found that the immune responses against synthetic polypeptide antigens were controlled by genes that he designated as Ir genes, and more importantly he found that these Ir genes were linked to the H2 genes on the seventeenth chromosome. This resulted in a major collaboration between Hugh McDevitt, Donald Shreffler, Jan Klein, Jack Stimpfling, and George Snell to map the location of the Ir gene. Studies using the various recombinant strains showed that the Ir gene mapped inside the H2 gene complex, probably between H2K and the Ss protein. This major breakthrough was also published in the *Journal of Experimental Medicine* in 1972. Until then the H2 genes had been a curiosity worked on by a small number of immunogeneticists and investigators interested in transplantation. The finding that genes controlling immune responses mapped within H2 attracted a much wider group of immunologists. Don and Hugh McDevitt

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respected and admired each other, which led to a strong scientific interaction and close friendship. Hugh remembers Don as "a gentle, thoughtful, and rigorous investigator who was extremely generous with his many MHC recombinant mouse strains, as well as with advice." Hugh remembered that "during our Ir gene mapping studies Don was extraordinarily helpful. In discussions both here at Stanford and [at] the University of Michigan, I was always impressed by Don's gentle approach to a scientific problem, and his very strict and rigorous application of genetic principles to experiments and experimental results."

In 1971 Don went to the Basel Institute of Immunology on a sabbatical leave. While there he made two important findings. In collaboration with Bernice Kindred he found an H2 dependence on the cooperation between T and B cells in vivo, a precursor to the MHC restriction studies. In collaboration with Tommy Meo he found that differences in the Ir region were sufficient to stimulate a mixed lymphocyte reaction suggesting an antigen controlled by the Ir genes. During this time Hugh McDevitt and Baruch Benacerraf had speculated that the product of the Ir gene might be the elusive T-cell receptor. This started the race to identify the Ir gene product and possibly the T-cell receptor.

At this time I finished my postdoctoral work and was appointed assistant research scientist in the human genetics department working with Don Shreffler. We were fortunate to have two recombinant strains of mice that were identical for the H2K and the H2D antigens but different in their Ir genes. We decided that cross-immunizations of these two strains of mice (A.TL and A.TH) could potentially generate antisera that could identify the Ir gene product. Similar studies were also initiated in the laboratories of Hugh McDevitt, Jan Klein (who now had his own laboratory at Michigan), and David Sachs at the National Institutes of

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Health. The antiserum we produced was unlike any other antisera we had made for the H2 genes. In general it reacted only with 40-50 percent of lymphoid cells, but it had a much higher titer than any other antiserum. It reacted predominantly with B cells, but its reactivity with T cells was controversial. During this time Jeff Frelinger, who like Don had been a graduate student with Ray Owen at Caltech, joined our laboratory and assisted us in this project. Extensive serological, genetic, and immunological studies using this antiserum with the different mouse strains proved that it was indeed detecting a product from the Ir gene itself or from a closely linked gene. The antigen was designated "Ia" for immune-associated antigen. Our first paper was published in the PNAS in 1973. Klein and his associates also made similar observations in their laboratory. The identification of the Ia antigens was a major breakthrough showing a second MHC-linked antigen associated with the genetics of the immune response. Further studies showed that the Ia antigens were expressed predominantly on antigen-presenting cells and not T cells.

Jeff Frelinger fondly remembers many parties at Don's house, especially the annual Christmas parties.

> Dorothy collected little mouse figures that the whole family would hide around the house. Hundreds of these little mice (maybe there were thousands!), wooden ones, plastic ones, ceramic, and glass blown mice [were] hidden everywhere: on the book shelves, hanging on the [Christmas] tree, hidden in the intricate carvings of the fine old-world furniture that Don's father or grandfather had hand-carved, [and] on Don's desk. [There were] mouse refrigerator magnets, [and some] even sitting in the bottom of the fish aquarium. We'd have a scavenger hunt, and there would be a prize for the person who found the most mice. All of this occurred with a traditional Midwesterner's openness and friendliness; a sense of honesty and good family values prevailed.

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Jeff credits Don for serving as a model for his own career.

> Don always gave us freedom. Even when he was really involved with a project, such as the beginning of the class II discovery, I never felt that he was trying to manage the project, but rather trying to guide all of us through the process of discovery. Certainly it was an exciting time, every day brought a new result. Don remained calm and supportive throughout the whole process. Don also let us be the young and excitable scientists we were.

Don was a very generous person. He could have kept all of the antisera and the recombinant mouse strains for himself to pursue all the interesting studies. Instead, he freely gave his sera and mouse strains without any strings attached to anyone who wanted them. This was an enormous boon to many laboratories around the world exploring the role and function of the Ir gene products. In a major collaboration between our lab and Stan Nathenson's laboratory at Albert Einstein University, Susan Cullen showed that Ia antigens consisted of two chains of 33,000 and 31,000 molecular weight whereas the molecular weight of the subunits of the H2K and the D molecules were 45,000. Her findings showed that the Ia antigens were a new class of MHC antigens, and were published in the PNAS in 1974. Jan Klein designated the H2K and the D molecules as class I antigens and the Ir-associated Ia antigens as the class II antigens. The Ss/Slp was designated as class III antigens.

Despite the excitement about the discovery of the class II antigens, Don had not forgotten his first love, the Ss protein. A new graduate student in the laboratory, Ted Hansen, took over the project and made an important observation. He found a significant correlation between the levels of Ss protein and hemolytic complement activity. The results suggesting that the Ss protein might be a component of the complement system were published in the *Journal*

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of Experimental Medicine in 1975. Tommy Meo from the Basel Institute of Immunology joined the lab for a sabbatical and got interested in the possibility that the Ss protein might be a component of the complement system. His studies led to another major finding that a component of human plasma and the Ss protein were cross-reactive. The human plasma component was identified as the fourth component of complement. The conclusion, that the Ss protein was probably the fourth component of complement in the mouse, was published in PNAS in 1975. Until then there had been a question of why there was no homologous gene for Ss in the human, while there was considerable homology in the class I and class II genes in human and mouse. Later the human C4 gene was also found to map within the human MHC region, showing there was complete homology between the human and mouse MHC.

Don had few activities apart from his work and his family, but he did enjoy fishing, where he could completely relax. He was a serious fisherman, allowing him to get his mind away from work in order to focus on what he was doing. Because his sons, David and Doug, were also avid fishermen, it provided a chance to spend time with them. The Shrefflers had a cottage on Little Silver Lake near Ann Arbor, where his family spent most of their summer weekends fishing for bass and bluegill. Don spent several weeks each summer teaching a mammalian genetics course in Bar Harbor, Maine, where he took his whole family. The main activity there was mackerel fishing. He not only loved catching fish but also loved eating them, and my family and I enjoyed many wonderful fish dinners at Don's house even though I never contributed to catching the meal. As a matter of fact, the one time I went fishing with Don I messed up all his lines so badly it took him an hour to untangle them. He never lost his temper or cool though, and kept smiling

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during the whole ordeal. David, his son, feels sad that his children won't have a chance to get to know their grandfather, but he plans to make sure that they know what a terrific guy he was: his sense of honesty and integrity, his passion for his work and his family, his patience and well-honed listening skills, his wonderful wit, his generosity, his humility, his quiet pride, and his farm-boy roots. The year he spent on sabbatical in Basel, Switzerland, was a special time for his family. They got to travel and see many wonderful things. His family will always remember his mischievous grin.

In 1975 an exciting proposal was offered to Don. Washington University in St. Louis wished to create a new department of genetics composed of several "superstars" in immunogenetics, such as Hugh McDevitt and Stan Nathenson. This was too exciting for Don to turn down. He accepted to move to Washington University, assuming that one of the others would be appointed chairman of the department. Unfortunately, the others decided not to make the move, but since Don was already committed, he agreed to be the acting chairman to start the department. I was recruited as one of the faculty in the department, and Don and I moved to St. Louis in the fall of 1975. The McDonnell Foundation was funding the department, and state-of-the-art laboratories and mouse-breeding facilities with all the amenities were constructed. Don had the opportunity to design one of the best mouse facilities in the country. In 1977 I moved my lab to the Mayo Clinic.

Despite the heavy responsibility of recruiting and building a new department of genetics, setting up the graduate program, and his teaching responsibilities, Don's lab continued to be very productive on the complement/Ss system. He set up a long-term collaboration with John Atkinson in the Department of Medicine in these studies. The next major breakthrough in the complement system came when Keith

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Parker, a graduate student in his lab, showed that the Ss protein and the Slp proteins were discrete components coded by closely linked genes within the S region. Subsequent studies by Parker and another graduate student, David Karp, thoroughly characterized the Ss and Slp proteins biochemically. Keith Parker remembers Don as one of those ideal mentors who was always available for discussion, listened to all sides of a question, had an open mind, and was uncompromising when it came to the integrity of the results and the data.

David Karp remembers Don as

> . . . a very supportive mentor whose scientific thought was both rigorous and creative. He taught us clarity and precision in our writing. He allowed us to pursue independent projects with what I felt was just the right amount of supervision. He was very supportive when it came to students attending and presenting at meetings. On one occasion, he wrote a generic abstract for a meeting that he was asked to present, saying that whoever had the most data could make the actual presentation. Although I am biased because I got to speak at the meeting, I think that was a very generous way to transfer recognition from a lab chief to the people doing the work. The Department of Genetics at Washington University is a lasting legacy to his organization and scientific foresight.

Don worked tirelessly to put together a top-notch group of interactive scientists working in a broad area of genetic model systems. Ted Hansen, a former graduate student in Don's lab, joined his department to continue his work on immunogenetics of the mouse MHC system. Don didn't just recruit individuals to his department who were interested in immunology; he engaged geneticists with widely different interests.

In 1980 Don was elected to the Institute of Medicine and in 1982 to the National Academy of Sciences. He also contributed to the American Association of Immunology and in the years 1982-88 served on its council, as vice-

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president, and finally as president. In 1984 he stepped down as the chairman of the Department of Genetics to concentrate on his research and other extramural activities. In 1986 the fiftieth anniversary of the discovery of H2 antigens by Peter Gorer was celebrated in Bar Harbor. The pioneers of the H2 field—Peter Grover, George Snell, Bernard Amos, Jack Stimpfling, Don Shreffler, and Jan Klein—were all honored at this happy occasion. In 1989 Don was honored as a distinguished alumni of the California Institute of Technology.

In the late 1980s Don's health deteriorated, and he was able to spend only limited time in the laboratory. All of his former students, postdocs, and colleagues got together to celebrate his sixtieth birthday in Bar Harbor in June of 1994. He appeared to be doing well both emotionally and physically, and we were all happy that he was on his way to a full recovery. He was looking forward to taking an early retirement to enjoy other things in life. None of us anticipated that he would die within a few months. He was laid to rest in a spot he loved most: his family farm near Kankkee, Illinois, next to four generations of Shrefflers.

Don Shreffler's death was a big loss to the field of immunogenetics, because we missed the intellectual contributions he had consistently provided. One way to judge the contribution of a person is by the human legacy he leaves behind. The students and the postdocs that Don trained hold important positions in the field and have themselves made major contributions. He gave full credit to his students and postdocs for all his accomplishments. I for one owe my career to Don Shreffler. I worked seven years with him, and credit him with teaching me to become a scientist. Another way to judge an individual is by the contributions he has made to his colleagues. Shreffler selflessly helped many laboratories flourish and succeed.

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I RECRUITED THE help of many individuals to write this memoir. I thank the members of the Shreffler family who provided me with the information on his childhood. To Don's wife, Dorothy, and his sons, David and Doug, for their input. I also appreciate contributions from Ray Owen, Jane Schultz, Hugh McDevitt, Jeff Frelinger, Keith Parker, and David Karp. I especially thank Henry Metzger for giving me the opportunity to write this memoir and for reading the manuscript and providing helpful suggestions. The material on Don Shreffler's early life came from the "In Memoriam" I wrote in 1995 for *Immunogenetics* (vol. 41, pp. 175-177, Springer-Verlag).

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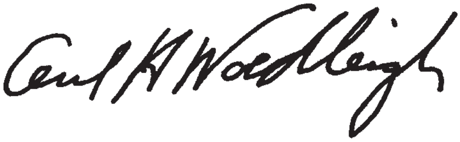

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CECIL H. WADLEIGH

October 1, 1907–February 18, 1997

BY WILFORD R. GARDNER

Cecil Wadleigh was a multi-faceted scientist. His youth on a farm taught him that pragmatism was a virtue but also aroused in him an interest in finding new and better agricultural practices and better crop varieties. He was a large man with a dignified demeanor that masked a sly sense of humor. His wit could easily deflate a colleague who showed signs of arrogance, but it was demonstrated in a good-natured rather than mean-spirited way. He could be charmingly persuasive and persistent, especially as an administrator.

Cecil was born in Gilbertsville, Massachusetts, on October 1, 1907, as the only son of Hazen Carl and Lucy (Whitehead) Wadleigh. He lived from 1909 to 1919 on his father's dairy farm, a not uncommon childhood and youth for agricultural scientists of his generation. His school bus was his father's milk delivery wagon, an arrangement that allowed him to work two to three hours before school. In the 1920s he moved to his father's 225-acre fruit and vegetable farm in Milford, Massachusetts. Characteristic of farm families, Cecil's father expected diligent work "only 99 percent of the time." Cecil preferred work in the orchards to that on the dairy farm if only because of the more pleasant byproducts. This preference may have influenced Cecil's later choice of

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botany rather than animal husbandry for a major. While still in high school he was assigned to supervise from 10 to 30 hired seasonal workers. Working alongside them engendered a mutual respect between supervisor and employee, which Cecil exhibited in his later years as a science administrator.

In 1925 Cecil graduated from Milford High School, where he excelled in mathematics, physics, chemistry, and biology. He disliked civics and ancient history. Cecil's description of his schoolteachers is little different from those that would have been given by almost anyone of his generation. He describes them as "peerless but stern and demanding, but they opened up new vistas to a run-of-the-mill farm boy." School was a pleasant diversion from the heavy schedule of farm work, and this contrast probably helped Cecil choose a career that required less from the back than from the head, but which was no less demanding in terms of time and effort.

In 1930 Cecil received a bachelor of science degree in pomology from the University of Massachusetts. That fall he married Clarice Lucille Bean in Petersburg, New York. This union was to produce in time three daughters and one son. The newlyweds went on to Columbus, Ohio, where Cecil received an master of science degree in horticulture from Ohio State University in 1932. This was followed three years later by a Ph. D. in plant physiology from Rutgers University. These institutions produced many outstanding agricultural scientists during the 1930s in large part due to the strength of their faculties. From 1933 to 1936 Cecil was a research assistant in plant physiology at Rutgers.

From 1936 to 1941 the "new" Dr. Wadleigh was an assistant professor of plant physiology at the University of Arkansas. It was while he was at Arkansas that Wadleigh showed the pragmatic side of his nature by working on a number of scientific problems with rather immediate practical applica-

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tion. His first contribution to science was a paper entitled "Better Quality in Sauerkraut" (1953). This study showed that the effect of potassium deficiency on carbohydrate synthesis in the cabbage plant was an impairment of the sauerkraut quality. It pleased Cecil to note that this paper was translated into German and published in a German technical journal. He felt that if anyone appreciated the quality of sauerkraut, it was the Germans. At Arkansas he increased his understanding of plant nutrients on crop quality through further studies on cabbage, chlorosis in corn, boron deficiency, and aspects of metabolism in cotton. The work at Arkansas was ideal preparation for the next phase of Cecil's career, since most of the work related in one way or another to the uptake of ions by plants.

CECIL'S YEARS AS A MATURE SCIENTIST

In 1941 Cecil joined the staff of the relatively new research facility, the Salinity Laboratory, in Riverside, California. This laboratory was one of seven regional laboratories established in 1938 in the U.S. Department of Agriculture to study agricultural problems common to more than one state. Cooperation between these regional laboratories and the state experiment stations within each region was intended to lead to more speedy and effective solutions to the problems identified by and common to each region. The state experiment station directors of the 11 (later 17) western states chose soil salinity as their most serious crop production problem.

Cecil was appointed senior chemist rather than a plant physiologist. He often expressed bemusement at the proclivity of the federal civil service to try to fit individuals into its own personnel classification scheme rather than adjust the scheme to fit the scientists. The laboratory had permission to hire a senior chemist but not a plant physiologist, and

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there was no question about his qualifications as a chemist. At some later period it was decided that everyone at the laboratory should be a soil scientist, so he was accordingly reclassified. He often found joy in pointing out that simply by an act of the government he was a soil scientist. Fortunately, Riverside was far enough from Washington so that no administrative types could get in the way of scientists using their abilities in the best possible way.

Wadleigh's arrival at the Salinity Laboratory was most fortuitous, both for Cecil and for the laboratory. The laboratory had been established in 1938 and was still seeking to find its scientific niche. This author's uncle (Willard Gardner) was one of the representatives from Utah on the committee to set scientific priorities for the laboratory. Gardner's proposal that the fundamental sciences underlying problems of irrigation, drainage, waterlogging, and salinity should have highest priorities. This was not received with any great enthusiasm by his colleagues, who preferred quick but empirical results. By a twist of fate and irony Gardner's student (M.S.) L. A. Richards had been hired away from Iowa State University to establish the physics program at Riverside in 1938 and by 1941 had developed the tools for which he became world renowned. These methods made possible basic studies on the osmotic and soil water relations of plants at the most fundamental thermodynamic level while generating valuable and immediately useful information for farmers.

Wadleigh first teamed up with Hugh Gauch in a series of studies on the effect of saline substrates on various metabolic steps in plants. In some ways this was a confirmation of the work that Wadleigh was doing at Arkansas, with salinity as an added variable. Cecil called upon the work that he had done previously in ion uptake to understand the effect of such uptake on plants in general.

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By 1943 Wadleigh was increasingly focusing his attention on the effect of salinity per se on plant response. There was a vigorous debate among plant and soil scientists about whether the deleterious effect of salinity upon plant growth was specific to each ion that contributed the total osmotic pressure of the soil (or nutrient) solution, or whether the effect was nonspecific and due almost solely to the osmotic effect. A related and no less important question was the combined or additive effect of osmotic stress and soil water stress. Nowhere was the division between the two schools of thought more pronounced and hotly debated than in Riverside. Wadleigh and Richards were on one side of the debate while some of the more important members of the Citrus Experiment Station, also in Riverside, were on the other.[1]

The laboratory staff was firmly of the opinion that while there might be some specific ion effects (e.g., boron), from a practical point of view the suitability of a water for agricultural use was largely dependent upon its osmotic pressure and hence to a good approximation the concentration of salts. The almost linear relation between soil solution concentration and electrical conductivity of the soil solution, conductivity measurements were soon used as a surrogate for osmotic pressure and/or concentration. Cecil Wadleigh and L. A. Richards largely share the credit for this simplification.

As a result of this thinking the plant research program at the laboratory under Cecil's supervision moved in two directions. It had been established from experiments at Torrey Pines (now the site of the University of California, San Diego) in the Imperial Valley as well as in Riverside that the relative effect of salinity upon plant growth was independent of climate. Therefore a series of field plot studies of the relative salt tolerance of a large number of field crops, tree crops, vegetables, and ornamentals was initiated. Salt tolerance between varieties of the same species were also initiated.

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Cecil Wadleigh had a major role in initiating these studies, but his principal interest was increasingly to center on the more fundamental question of the mechanism of plant stress upon growth. To this end he began the series of greenhouse studies for which he became best known as a scientist.

Following a series of papers, often in collaboration with other members of the Salinity Laboratory staff, Wadleigh showed definitively that not only was it osmotic stress that was important in determining the effect of soil salinity upon plant growth but also, to a very remarkable first approximation, osmotic stress and soil moisture stress were additive. This, combined with the knowledge that whatever the climate, the relative effect of total stress (osmotic plus moisture) was the same resolved once and for all the debate over the effect of salinity over plant growth. In recent years the concept had received fine-tuning, but as a general approximation it still stands.

A second debate also consumed those working on soil-water-plant relations. This also involved the Salinity Laboratory, but this time the other protagonist was F. J. Veihmeyer of the University of California at Davis. Veihmeyer had carried out a series of very important irrigation experiments in various parts of California. He concluded from these experiments that between the upper limit of available water (i.e., the field capacity) and the lower limit of water in the soil, known as the permanent wilting point, the water was equally available to the plant. That is, as the soil dried out it made no difference to the plant what the soil water content was until the plant failed to recover from temporary wilting even after stress was relieved at night. Wadleigh teamed with L. A. Richards in a landmark paper that reviewed the literature and added new insight and largely laid the issue to rest.[2] They showed that once a plant begins to wilt it reduces its rate of growth and continues to do so until it

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either dies or is irrigated. Veihmeyer had worked mainly with fruit trees on sandy soils, where the difference between initial wilting and permanent wilting was difficult to assess. He was responsible for the definition of the permanent wilting point, which was useful for a time, but his choice of sunflower as an experimental plant was unfortunate since lower leaves wilt first while higher and younger leaves wilt progressively later. Veihmeyer was to remain unconvinced to his death of the correctness of his view and his earlier work on irrigation is still significant and valuable. However, Richards and Wadleigh were correct in their view of soil water and further experiments by others only served to solidify this view.

Wadleigh also played a major role in contributing to the writing of Handbook 60 entitled "Diagnosis of the Improvement of Saline and Alkali Soils." This was published by the Bureau of Plant Industry of the U.S. Department of Agriculture in 1947 and was revised and published in hardcover in 1953. This publication was to become the "bible" of soil salinity for some 25 years and is now a collector's item.

WADLEIGH'S CAREER AS AN ADMINISTRATOR

In 1951, just prior to publication of his landmark paper with Richards, Cecil Wadleigh once more headed east, this time to Washington, D.C., to accept the position of head physiologist, Division of Sugar Plant Investigations in the Agricultural Research Service of the U.S. Department of Agriculture (USDA). In this position he was responsible for all sugar research in the United States. It was here that he honed his skills as an administrator and perfected his unique style.

In 1955 he moved up to become director of the Soil and Water Conservation Research Division of the Agricul-

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tural Research Service. In this capacity he oversaw, among other units, the U.S. Salinity Laboratory. It was on his tour of his domain that this author first met him. I had accepted a new position as a physicist with the Beltsville, Maryland, laboratory of the USDA, but until they had room for me I was sent to Riverside to learn some soil physics with L. A. Richards. Wadleigh came through Riverside about midway during my stay in Riverside. I explained to him that I had concluded that in order to do anything useful I either needed to spend a much longer time in Riverside or else move immediately to Beltsville. Wadleigh showed his ability to make immediate decisions when necessary. Rather that suggesting that he would think about it, he immediately promised that I could stay as long as needed. That state lasted 13 years, and when I left he offered me any location in the United States.

Cecil had an administrative style that could best be described as unique. He could be insistent in getting people to leave a perfectly happy research career and come to work for him in Washington. For example, he persuaded Jan van Schilfgaarde to leave North Carolina. Doral Kemper was called in Australia while on sabbatical from Ft. Collins, Colorado, and he agreed to move to Washington upon his return. Cecil had a way of convincing an individual that they were the only person in the world qualified to carry out a specific task.

Cecil was often called to testify before Congress, which he usually did himself, however, in negotiations with his superiors he often sent an underling. Afterwards he would complain that he would have handled the matter differently. When the subordinate sent would point out that he had had a chance to go himself and had chosen not to, he took this response in good humor, and one understood

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that he had sufficient confidence in his surrogate that he was quite prepared to accept the outcome.

Cecil was could be firm when firmness was called for. He once tried to close down a small research facility in the West. It so happened that the director of this facility was related to the local congressman. The word came down from above to Cecil that he was not to touch this facility. He promptly transferred the director to a very undesirable location "in the interests of the federal government." Sometime later he quietly closed the facility.

Cecil Wadleigh was one of the last of the administrators who had earned their stripes in research before moving to Washington to become an administrator. As such he understood what made scientists tick. He normally left a productive scientist alone to do what he thought best. However, he was not above using this knowledge to achieve his own ends if he felt they came ahead of the scientist's wishes.

Cecil Wadleigh retired from the Soil and Water Conservation Division in 1970. He served for a year as science advisor in 1971 and retired from that position at the end of 1971. From 1969 to 1971 he gave some 100 invited lectures at universities and technical societies on the general subject of agriculture's involvement in environmental pollution. This was soon after the publication of Rachel Carson's book *Silent Spring* and largely in response to the book. He did not always agree with Rachel Carson and was not bashful about expressing his views whether they agreed with her or not. Whatever the issue he always preserved his integrity, and this quality in him will always be remembered.

In retirement Cecil kept his interest in plant science. He maintained a large collection of named varieties of tall bearded irises and nurtured an orchard of over 100 different dwarf fruit trees. He also developed a competence in cooking,

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with an emphasis on Italian, Creole, and rural American cuisine.

HONORS AND AWARDS

1935	Elected to Sigma Xi
____	Elected president of the American Society of Plant Physiologists
1961-63	Selected a member of White House Panel on Waterlogging and Salinity Problems in Pakistan by President Kennedy
1962	Elected a fellow of the AAAS
1963-70	Selected a member of the Committee on Water Resources, Federal Council on Science and Technology, Executive Office of the President
1965	Elected a fellow of the American Society of Agronomy
1965-67	Selected a member of the U. S. National Committee for the International Hydrological Decade, National Academy of Sciences
1966-67	Selected a member of the Committee on Environmental Quality, Federal Council on Science and Technology, Executive Office of the President
1967	Presented the Distinguished Service Award, U.S. Department of Agriculture
1969	Elected a fellow of the Soil Conservation Society of America
1973	Elected to the National Academy of Sciences

NOTES

1. In 1943-44 this author's father, who represented Colorado on the Laboratories Board of Collaborators, was invited to spend a year on the staff of the laboratory to shore up the chemistry program. He felt it wise to stay out of this argument.

2. L. A. Richards and C. H. Wadleigh. Soil water and plant growth. In *Soil Physical Conditions and Plant Growth*. Agronomy Monograph 2(1952):73-251.

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Hermann Weyl

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HERMANN WEYL

November 9, 1885–December 9, 1955

BY MICHAEL ATIYAH

HERMANN WEYL WAS one of the greatest mathematicians of the first half of the twentieth century. He made fundamental contributions to most branches of mathematics, and he also took a serious interest in theoretical physics.

It is somewhat unusual to write a biographical memoir nearly 50 years after the death of the subject, and this presents me with both difficulties and opportunities. The difficulties are obvious: I had essentially no personal contact with Weyl, hearing him lecture only once at the international congress in Amsterdam in 1954, when I was a research student. His contemporaries are long since gone and only a few personal reminiscences survive. On the other hand the passage of time makes it easier to assess the long-term significance of Weyl's work, to see how his ideas have influenced his successors and helped to shape mathematics and physics in the second half of the twentieth century. In fact, the last 50 years have seen a remarkable blossoming of just those areas that Weyl initiated. In retrospect one might almost say that he defined the agenda and provided the proper framework for what followed.

I shall therefore take the liberty of connecting Weyl's own work with subsequent developments. This means that I

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shall say less about certain aspects of Weyl's work where my own competence runs out or where subsequent work may not have been so productive. In particular I shall omit any account of his important work on singular differential equations, number theory, and convex bodies. I shall also say little about his contributions to the foundations of mathematics. However, the important papers that he wrote in all these areas are included in the selected bibliography.

Hermann Weyl was born in the small town of Elmshorn near Hamburg, the son of Ludwig and Anna Weyl. In 1904 he went to Göttingen University and immediately fell under the spell of the great David Hilbert. As he described it later,

> I resolved to study whatever this man had written. At the end of my first year I went home with the "Zahlbericht" under my arm, and during the summer vacation I worked my way through it—without any previous knowledge of elementary number theory or Galois theory. These were the happiest months of my life, whose shine, across years burdened with our common share of doubt and failure, still comforts my soul.

In 1913 he moved to a chair at the Federal Institute of Technology in Zurich, where Einstein was developing his theory of general relativity. Sometime later this aroused Weyl's interest, and physics became and remained one of his central concerns.

When Hilbert retired in 1930, Weyl moved to Göttingen to take his chair, but the rise of the Nazis persuaded him in 1933 to accept a position at the newly formed Institute for Advanced Study in Princeton, where Einstein also went. Here Weyl found a very congenial working environment where he was able to guide and influence the younger generation of mathematicians, a task for which he was admirably suited.

At the time of his move to Zurich he married Helene

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Joseph, a talented translator of Spanish literature. They had two sons. Helene died in 1948, and in 1950 Weyl married Ellen Bär from Zurich.

Weyl published in a great variety of fields and he deliberately eschewed specialization. He explained his attitude as follows:

> My own mathematical works are always quite unsystematic, without mode or connection. Expression and shape are almost more to me than knowledge itself. But I believe that, leaving aside my own peculiar nature, there is in mathematics itself, in contrast to the experimental disciplines, a character which is nearer to that of free creative art.

As this quotation (and others) illustrate, Weyl was both a philosopher and a literary stylist. His interest in philosophy led him to become involved in the foundations of mathematics, one of the major interests of the time that saw great battles between the formalists led by Hilbert and the intuitionists under Brouwer. The essential difference was that the intuitionists only accepted as valid those results that could be established constructively in a finite number of steps. Weyl eventually and somewhat reluctantly sided with Brouwer. But his broader philosophical interests meant that he was always aware of the wider implications of his mathematical work and in particular of its relation to physics. He expounded his philosophical views on physics in a widely read book (1918).

His literary, almost poetic, style is highly unusual in a mathematician and only someone of his stature could expect to get away with it. Even the enforced transition from German to English resulting from his move to Princeton did not deter him. In his book on *The Classical Groups* (1939) his introduction recognizes this transition in typical form by asserting that "the gods have imposed upon my writing the

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yoke of a foreign language that was not sung at my cradle." Later in the text when discussing the rotation group he writes that "only with spinors do we strike that level in the theory of its representations on which Euclid himself, flourishing ruler and compass, so deftly moves in the realm of geometric figures. In some way Euclid's geometry must be deeply connected with the existence of the spin representation." Subsequent work on spinors only reinforces the power of these words, written though they were in a foreign tongue.

Despite the diversity of his interests it is analysis and geometry with their application to physics that provide the core of his work, though he could be an algebraist with style as in *The Classical Groups* and his tendency to unify mathematics makes nonsense of any simplistic divisions. His interest in the spectral properties of differential operators (their eigenvalues or frequencies) was an early love and persisted to the end. In fact, one of his first major achievements was to establish that the leading term in the growth of the eigenvalues (for the Laplace operator in a bounded domain) was given by the volume, a result that was predicted by physicists on the basis of the relation between classical and quantum mechanics. Weyl followed with interest the subsequent refinement of his work that gave more detailed information about the asymptotic behaviour of the eigenvalues, a subject popularized by Marc Kac under the title "Can You Hear the Shape of a Drum?" After Weyl's death the subject developed much further, leading among other things to the heat equation proof of the Atiyah-Singer index theorem (Atiyah, Bott, and Patodi, 1973) and to the regularized determinants that became a basic tool in quantum field theory.

In his Gibbs lecture to the American Mathematical Society (1950) Weyl set out his views on the eigenvalue problem in the following Delphic utterance:

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> I feel that these informations about the proper oscillations of a membrane, valuable as they are, are still very incomplete. I have certain conjectures of what a complete analysis of their asymptotic behaviour should aim at but, since for more than 35 years I have made no serious effort to prove them, I think I had better keep them to myself.

Whatever Weyl had in mind it is clear that he would have thoroughly appreciated the developments of recent times, particularly in the way the physics, analysis, and geometry have been interwoven.

Another early work was his now famous book on Riemann surfaces (1913). Here we see Weyl at his majestic best, imposing coherence, elegance, and order on a classical subject and thereby laying proper foundations for its future development. Already with the work of Riemann it was clear that the classical theory of functions of a complex variable could not be confined to the complex plane: branched coverings of the plane were necessary, but it was Weyl who put this into its proper form, getting away from the complex plane by introducing the notion of an abstract surface. Coming when it did in 1913 it was the right book at the right time, providing the model for all subsequent work on higher-dimensional manifolds. With its emphasis on vector spaces (which Weyl was the first to define) it provided the right language for both geometry and algebra. It also prepared the way for the topologists who followed.

Without Weyl's book on Riemann surfaces it is impossible to imagine Hodge's theory of harmonic forms (Hodge, 1941), which came 20 years later. Weyl was one of the first to recognize the importance of Hodge's work and he contributed an essential step for the analytical part of the proof. He described Hodge's theory as "one of the great landmarks in the history of science in the present century."

In 1954 at the International Congress of Mathematicians

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in Amsterdam Weyl, as chairman of the Fields Medal Committee, gave the speech describing the work of the two medallists: Kunihiko Kodaira and Jean-Pierre Serre. Kodaira had also independently completed Hodge's work and had gone on to apply it with great skill to prove concrete results in algebraic geometry. Serre had contributed through his work on the newly developed theory of sheaves. Despite his age (he was 69) Weyl gave a detailed and enthusiastic account of all this work, which by combining geometry and analysis in the spirit of his own earlier work was very close to his heart. This is clearly conveyed in his words addressed to Kodaira:

> Your work has more than one connection with what I tried to do in my younger years; but you have reached heights of which I never dreamt. Since you came to Princeton in 1949 it has been one of the greatest joys of my life to watch your mathematical development.

Turning to Serre, whose work in homotopy theory he had also described in detail, he said,

> I have no such close personal relation to you, Dr. Serre, and your research, but let me say that never before have I witnessed such a brilliant ascension of a star in the mathematical sky as yours. The mathematical community is proud of the work you both have done. It shows that the old gnarled tree of mathematics is still full of sap and life.

As a young member of the large audience on that occasion I was dazzled by Weyl's performance and inspired by his oratory.

If geometry and analysis were at the core of Weyl's interests, his urge to organize and synthesize made it perhaps inevitable that he would leave his mark on the theory of groups and their representations. These are the embodiment of symmetry, a topic that Weyl expounded on toward

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the end of his life in an elegant and popular book with that title (1952). The theory of continuous groups, developed by the nineteenth-century Norwegian mathematician Sophus Lie had been continued and extensively developed by Elie Cartan. Weyl took up the topic anew and brought his own point of view, with its emphasis on the global aspect of Lie groups. For his predecessors all the essential formulae were local (leading to the infinitesimal form, the Lie algebra), but Weyl emphasized the whole group, a manifold with, in particular, interesting topology. Here we see a link with his approach to Riemann surfaces: Weyl liked to see the big picture, the manifold or group in the round. This global view had many technical advantages and in particular for compact groups (such as the important group of rotations), one could average by integrating over the group. Essentially this made the theory very similar to that of finite groups, which was already well established. One famous consequence of this technique is the Peter-Weyl theorem, which decomposes the space of functions on the group into matrix blocks given by the irreducible representations. Here Weyl, as always, used his knowledge of differential equations in an essential way.

To pass from the compact groups to the usual linear groups of matrices Weyl employed what he described as the "unitarian trick," a simple but effective idea that has had a fruitful development beyond the narrow confines of pure group theory. In the hands of Simon Donaldson and others it has been a powerful tool in the study of moduli spaces, where it can be viewed as a geometric extension of Weyl's initial step.

One of the most elegant of Weyl's theorems was his beautiful explicit formula for the character of the irreducible representations. This formula has kept reappearing in subsequent work. For example, it appears as a fixed-point

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formula in the work of Atiyah and Bott (1966) on elliptic operators where it unites two of Weyl's main interests. It also appears in generalized form (Pressley and Segal, 1986) in the theory of representations of loop groups, infinite-dimensional groups of much interest in current physics.

Weyl was a strong believer in the overall unity of mathematics, not only across sub-disciplines but also across generations. For him the best of the past was not forgotten, but was subsumed and refined by the mathematics of the present. His book *The Classical Groups* was written to bring out this historical continuity. He had been criticized in his work on representation theory for ignoring the great classical subject of invariant theory that had so preoccupied algebraists in the nineteenth century. The search for invariants, algebraic formulae that had an intrinsic geometric meaning, had ground to a halt when David Hilbert as a young man had proved that there was always a finite set of basic invariants. Weyl as a disciple of Hilbert viewed this as killing the subject as traditionally understood. On the other hand he wanted to show how classical invariant theory should now be viewed in the light of modern algebra. *The Classical Groups* is his answer, where he skilfully combines old and new in a rich texture that has to be read and re-read many times. It is not a linear book with a beginning, middle, and end. It is more like an elaborate painting that has to be studied from different angles and in different lights. It is the despair of the student and the delight of the professor.

It is a tribute to Weyl's outlook that invariant theory has recovered from Hilbert's onslaught and is again a flourishing subject. But now it is firmly in the Weyl mold and has been given a fresh impetus by David Mumford under the heading of geometric invariant theory (Mumford, Fogarty, and F. Kirwan, 1994). This gives a systematic way of studying various important classification problems leading to moduli (or

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parameter) spaces, and many of these have turned up naturally in quantum field theory. Again, Weyl would have been delighted.

When theoretical physics was revolutionized by the advent of quantum mechanics in the 1920s, it was fortunate that there were then two outstanding mathematicians who were available to provide the mathematical underpinning and interpretation. John von Neumann put quantum mechanics into its now standard framework of Hilbert spaces and self-adjoint operators while Weyl expounded on the role of symmetry in his influential book on group theory and quantum mechanics (1928). In fact, the representation theory of Lie groups is tailor-made for quantum mechanics and Weyl's definitive work on representation theory together with his interest in spectral theory made him the ideal exponent of the new physics.

Von Neumann was some years younger than Weyl, but he was a prodigy with a formidable reputation. According to Armand Borel, who heard the story from M. Plancherel that whenever Weyl was going to give a lecture at Zurich, he approached the lecture room with trepidation in case von Neumann was in the audience. He was sure to ask penetrating questions that Weyl would be unable to answer! This fear did not prevent Weyl from arranging for von Neumann to be invited to join him later at the Institute for Advanced Study. As his speech at the Amsterdam congress showed, Weyl was always keen to identify talent and provide encouragement for the younger generation. Raoul Bott recalls (Bott, 1988) how kindly Weyl dealt with him on their first encounter, when Bott explained his latest result, only to find out that Weyl had done it all 25 years before. Bott also points out that Weyl as a person was not the Olympian figure that he appeared to be in print. He could be informal, amusing, and friendly.

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Quantum mechanics was not Weyl's first encounter with physics. He had already learned about Einstein's general relativity, which explained gravity in geometrical terms. Weyl had the idea of extending Einstein's theory to incorporate electromagnetism, so that Maxwell's equations would also acquire geometrical significance. Weyl's idea was to introduce a scale, or gauge, that varied from point to point and whose variation round a closed path in space-time would encapsulate the electromagnetic force. Almost immediately (in fact in an appendix to Weyl's paper) Einstein criticized the idea on physical grounds. If Weyl was right, then the size of a particle would depend on its past history, whereas experiments showed that all atoms of hydrogen, say, had identical properties. One might have thought that such a telling criticism from someone of Einstein's standing would have discouraged Weyl and that he might have withdrawn his paper. It is a tribute to his mathematical insight and self-confidence that he went ahead. The idea was too beautiful to discard, and Maxwell's equations came out like magic. As often happens, a good idea lives to fight another day and only a few years later, with the advent of quantum mechanics, a new physical interpretation was put on Weyl's calculations. Oscar Klein proposed that Weyl's gauge should be viewed as a phase and that space-time should be viewed as having a fifth dimension consisting of a very small circle. Mathematically Weyl's gauge variable gets multiplied by i (the square root of –1) and is periodic. This point of view, called the Kaluza-Klein theory (Theodor Kaluza made the first steps after Weyl) is now generally accepted. Moreover, it is just the first stage in the enlargement of ordinary space-time. To include the other nuclear forces we need even more dimensions and current research centres on a total space-time dimension of 10 or 11.

Independently of these extra dimensions Weyl's gauge

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theory description of Maxwell's equations is now applied to local symmetry groups other than the circle. This leads to the non-Abelian gauge theories, which are the basis of the standard model of elementary particle physics.

This gauge theory, the infant that was nearly thrown out with the bath water, has grown up into sturdy adulthood. Not only is it the framework of modern physics but it is also one of the most novel and exciting areas in modern mathematics. One notable example is the theory of 4-dimensional manifolds due to Simon Donaldson (Donaldson and Kronheimer, 1990), which emerged from physics but has turned out to be of profound importance to geometry. More recently, an alternative interpretation uses spinors coupled non-linearly to electromagnetism, a twist that would certainly have captured the imagination of Hermann Weyl and justifies his remarks about the geometrical significance of spinors.

The past 25 years have seen the rise of gauge theories—Kaluza-Klein models of high dimensions, string theories, and now M theory, as physicists grapple with the challenge of combining all the basic forces of nature into one all embracing theory. This requires sophisticated mathematics involving Lie groups, manifolds, differential operators, all of which are part of Weyl's inheritance. There is no doubt that he would have been an enthusiastic supporter and admirer of this fusion of mathematics and physics. No other mathematician could claim to have initiated more of the theories that are now being exploited. His vision has stood the test of time.

My thanks are due to Raoul Bott and Armand Borel for personal reminiscences. I have also relied on the obituary articles by Chevalley-Weil and by Newman cited in the references.

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R. L. Wilder

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RAYMOND LOUIS WILDER

November 3, 1896–July 7, 1982

BY FRANK RAYMOND

RAYMOND LOUIS WILDER, loved by his family, friends, students, and colleagues, was a pioneer in the emerging discipline of topology and attained international acclaim for his creation and development of generalized manifolds. He also had a lifelong scholarly interest in anthropology and the foundations of mathematics. This erudition resulted in many articles and two important books on the cultural origins and development of mathematics.

He was born in 1896 in Palmer, Massachusetts, and as a youth attended schools in that town. His family was musical, and he played the cornet in the family orchestra at dances and fairs. His flair at the piano resulted in employment at the local movie house to accompany the silent films. Love for music making never left him, although later he usually stuck with the classics.

Wilder entered Brown University in 1914. This was interrupted by World War I, and he served two years in the Navy as an ensign. He returned to Brown and completed his bachelor's degree in 1920 and his master's degree in actuarial science in 1921. In the meantime, he had married the charming Una Maude Greene. They had three daughters: Mrs. Mary Jane Jessop of Long Beach, California, Mrs. Kermit Watkins of Altadena, California, Dr. Beth Dillingham of

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Cincinnati, Ohio, and a son Dr. David E. Wilder of Pound Ridge, New York. At the time of Wilder's death in Santa Barbara in 1982 there were in addition 23 grandchildren and 14 great-grandchildren. His wife, Una, survived him for an additional 19 years, dying at the age of 100 in Long Beach.

The University of Texas at Austin was well known for its actuarial program, and Wilder decided to pursue further actuarial studies there. As he had enjoyed "pure math" as an undergraduate, he asked permission to participate in R. L. Moore's analysis-situs (the old name for topology) course. "No, there is no way a person interested in actuarial mathematics could do, let alone be really interested in, topology," said Moore. Wilder persisted and after Moore's extensive questioning and Moore's surprise to Wilder's answer to "what is an axiom?" he relented. He granted him admission but proceeded to ignore him. Moore's famous method of teaching was to begin with a few axioms and definitions. He would then state theorems, and it was up to the participants to find the proofs. Some of the propositions were quite difficult, and after Wilder had solved one of the more difficult ones, Moore began to take notice. He was also in the habit of posing unsolved research problems under the guise of homework. When Wilder found an elegant solution to one of the problems that had eluded J. R. Kline and Moore himself, Moore invited him to write this up as a Ph.D. thesis. So Wilder abandoned an actuarial career and became Moore's first doctoral student at Texas. Wilder and Moore maintained very cordial relations throughout the years.

He stayed an additional year at Austin as an instructor and in 1924 he moved with his family to Ohio State University to assume an assistant professorship. In the R. L. Moore archives at the University of Texas there is an exchange of

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letters discussing research and teaching along with Wilder's reluctance to sign a required loyalty oath at Ohio State University. Wilder's hostility to mindless patriotism and his predilection for liberal thought accompanied him throughout his life.

In 1926 he moved to the University of Michigan. Thus began a relationship that lasted 41 years, until his retirement from teaching in 1967. Wilder's first works in set-theoretic topology were well underway and had begun to gather international attention when a paper of J. W. Alexander was studied at the University of Michigan. This very important paper, in which Alexander proved his now famous duality theorem, was instrumental in turning Wilder's interest toward manifold theory and the use of algebraic techniques. For us, equipped with the systematic machinery of algebraic topology, Alexander's theorem does not seem so overwhelming today. However, we must remember at that time cohomology, relative homology, products, exact sequences, homotopy theory, etc., were still years in the future. Alexander had also produced his famous horned sphere, which meant the end of any hope for routine generalizations of plane topology to n-space. Yet the closed complementary domains of Alexander's sphere, while not both 3-cells, could not be distinguished from 3-cells by any homological means. It was this insight that led to Wilder's converse to the Jordan-Brower separation theorem in 3-dimensions.

In 2-dimensions the Jordan-Brower separation theorem says that a topological circle separates the plane into two uniformly locally connected complementary connected pieces of which the circle is the common boundary. The converse states that a connected closed and bounded subset of the plane that separates the plane into two uniformly locally connected pieces with the closed subset as the common boundary must be a topological circle. In 1930 Wilder in-

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troduced, in terms of homology, the analogue of uniform local connectedness in higher dimensions and determined when a closed subset of a 3-manifold was an embedded 2-manifold. Thus, the 2-manifold is determined by properties of its complement.

In 1933 the Institute for Advanced Study was founded in Princeton. Roaming the corridors of old Fine Hall were many topologists including Oswald Veblen, J. W. Alexander, Solomon Lefschetz, E. R. Van Kampen, A. W. Tucker, Leo Zippin, and Ray Wilder. Alexander, Eduard Čech, L. Vietoris, P. S. Alexandroff, and Lefschetz had invented or were inventing various homology theories that could handle general spaces and their subsets. The notion of a generalized manifold was not unknown in the polyhedral category (Van Kampen, in 1929), and the formulation for topological spaces in more abstract homological terms was done in the early 1930s. The first proofs of dualities for generalized manifolds were by Čech and Lefschetz.

In Wilder's famous paper in the *Annals of Mathematics* (1934), Wilder characterizes which closed subsets of a generalized n-manifold are (n-1)-generalized manifolds in terms of properties of their complements. This includes a generalization of the converse of the Jordan-Brower separation theorem in higher dimensions. That the setting in terms of generalized manifolds was appropriate can be discerned today because the analogous results for classical locally Euclidean manifolds are still not known in their full generality.

It is instructive to turn to Wilder's Symposium Lecture delivered to the American Mathematical Society (point sets in three and higher dimensions and their investigation by means of a unified analysis situs) in Chicago in 1932. Wilder had been concerned over the separation that had developed between the two schools of American topologists typified by the Texas (set theoretic or local) and Princeton

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(combinatoric or global) schools. Wilder was a successful "rebel" from the Texas school. It annoyed him to see criticism raised against unified methods. Actually, he was criticizing the dogmatism of both schools. By combining the methods of both schools he had been able to obtain generalizations of theorems of the plane whose extensions to n-space by means of set-theoretic methods alone had heretofore been unsuccessful. He was not alone of course in these successes, for Alexandroff, Čech, and Lefschetz, to name a few, had no qualms about combining methods. However, Wilder was brave to expose so much of his point of view to his contemporaries and to future generations, as he did in this Symposium Lecture.

Much of Wilder's work in topology can be said to center on placement problems and associated positional topological invariants. These essentially mean properties of a space M, in a space S, which are independent of M's embedding in S. For example, the uniform local connectedness of complementary domains of the (n-1)-sphere in the n-sphere is preserved by different embedding of the (n-1)-sphere in the n-sphere. These positional invariants manifested themselves in the plane as thoroughly investigated set-theoretic concepts. However, to obtain generalizations to higher dimensions required the introduction of homological (and later homotopical) concepts and techniques.

In 1942 Wilder delivered the American Mathematical Society Colloquium Lectures. World War II intervened and consequently it was not until 1949 that "Topology of Manifolds" was published by the society as volume 32 of its colloquium series. In the first portions of this 400-page book Wilder presents much of the topology of the plane that will be generalized to higher dimensions in the later portions of the book. Čech homology and cohomology theory and the lc^n and $colc^n$ properties are developed. At that time of

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writing and research, exact sequences and other functorial notions, which were to alter the point of view of topology, had not been introduced. Consequently, the reader will not find exact sequences, diagrams, and sheaves explicitly mentioned in the text. Wilder by that time had settled upon a modification (deletion of a superfluous lc^n axiom) of E. Begle's definition of generalized manifold as well as Begle's proof of the duality theorems. This definition is essentially equivalent to the one most popular today. The book contained much of Wilder's previously unpublished research and many generalizations of his previous research. It was a summing up of all that was known about generalized manifolds at the time.

Generalized manifolds are really the class of spaces for which Poincaré duality holds for every open subset. About 10 years later another explosion in the interest of generalized manifolds occurred. Powerful new machinery had been introduced into algebraic topology resulting in new proofs of the duality theorems. In 1957 C. T. Yang showed that Smith manifolds were really generalized manifolds, and so generalized manifolds began to play a very important role in topological transformation groups. In fact, they became the natural setting for the subject.

In 1957 Wilder in his monotone-mapping theorem gave sufficient homological conditions for a map of a manifold (or generalized manifold) to have a generalized manifold as an image. S. Smale, who was a student participant in Wilder's seminar at that time, was inspired to find the analogous homotopical setting. The results play an important role in modern research in generalized manifolds.

A generalized manifold cannot be distinguished from a classical topological manifold by purely homological means even though it may be far from a topological manifold. Today, finding the properties that will force a generalized

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manifold to be locally Euclidean is a major concern of geometric topologists. To avoid some of the pathology not detected by homology, one must assume that the generalized manifold is locally contractible. This enables homotopy theory and surgery methods to become active tools in this search. It is now known that these restricted generalized manifolds explain some of the mysteries behind topological surgery theory. Several homotopy theoretic ideas and the underlying basic technology for working with these more restrictive generalized manifolds goes back to works of Wilder and Eilenberg-Wilder in the 1930s and 1940s. Wilder's work and virtual creation of the theory of generalized manifolds has played a significant role in geometry and topology. Undoubtedly its influence will last long into the future.

Equal to Wilder's commitment to research in topology was his interest in teaching and in the foundations of mathematics. I shall quote from an article by Lucile Whyburn,[1] who wrote about Wilder's teaching.

> Thinking back to his student days he created his famous course, "Foundations of Mathematics." It is interesting that he began this course in the early thirties with a class of approximately thirty students whose central interest was actuarial mathematics. Much later he was to write a textbook for such a course and in the preface he says, "The reason for instigating such a course was simply the conviction that it was not good to have teachers, actuaries, statisticians, and others who had specialized in undergraduate mathematics, and who were to base their life's work on mathematics, leave the university without some knowledge of modern mathematics and its foundations."

The foundations course continued until his retirement at the University of Michigan in 1967. In the classroom Wilder went beyond topology and foundation of mathematics. He saw mathematics not only as a beautiful technical edifice but also as a product of the cultures that had created it. He

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felt that a knowledge of mathematics and its methods should be a part of the intellectual and cultural background of all well-trained people; whether they be teachers, businessmen, legislators, public servants, or housewives. Over the years I have met people from different walks in life who related to me that they had taken Wilder's Foundation of Mathematics course at the university. They remember it as one of the important things they did for themselves at the university.

Wilder had lifelong friendships with the leading philosophers and anthropologists at the university. Close to 40 of his publications concern themselves with mathematics' role in society and world cultures. He viewed mathematics as having a cultural basis and believed that recognition of this would clear the air of most of the mystical and philosophical arguments offered in support or defense of theories of the foundations of mathematics.

Wilder held that mathematics develops from two kinds of cultural stress. Mathematics arising from environmental cultural stress is a response to a perceived need to facilitate certain societal interactions, whereas inherited cultural stress is the response to internal mathematical problems. For example, development in an old culture of a symbolic nomenclature for recording numbers as in the number of bushels of wheat a farmer owes in taxes is an environmental stress. A response to one of Zeno's paradoxes would be an internal cultural stress. His book *Evolution of Mathematical Concepts* (1968) convincingly lays out his thesis in terms accessible to a layperson.

Wilder took a very active role in the development of research at the University of Michigan. In 1927 Wilder and G. Y. Rainich founded a somewhat secret research club called "The Small C." They felt that the Department Club, which met monthly, was not accomplishing very much in the development of interests in research. The Small C met every

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Tuesday evening to present a scientific paper by a member of the club. Usually it was on a member's own research, but sometimes it was a report on a new mathematical result of great importance. At the beginning there were eight members from the mathematics department, one from philosophy, and three from physics. Later, others active in research, including some research students, were invited to join. In 1947 the Small C was disbanded, because research now was expected of all faculty members. In 1981 Ray and Una Wilder endowed the G. Y. Rainich lecture series of the University of Michigan Mathematics Department to honor the memory of their friend who was an important figure in the development of mathematics at the university.

Wilder was very good at discovering and encouraging talent. He interested Norman E. Steenrod in mathematics in the 1930s. Steenrod did his first research under Wilder's direction. When Steenrod finished his undergraduate training he returned to Ohio and worked for a year and a half while Wilder arranged for him to study at Harvard and later with Lefschetz at Princeton. He managed to find a place at Michigan for the young Polish topologist Samuel Eilenberg in the late 1930s despite opposition from some quarters. The famous collaboration of Eilenberg and Steenrod began when Wilder was able to get Steenrod a position at Michigan.

Wilder directed 25 doctoral dissertations including those of Leon Cohen, Paul Swingle, Sam Kaplan, Ed Begle, Morton Curtis, Alice Dickinson, Joe Schoenfield, Tom Brahana, J. P. Roth, Kyung W. Kwun, and myself. His advanced graduate classes and seminars were intimate and stimulating. He enjoyed talking about the people, many of whom he knew personally, behind the ideas and theorems. I found myself often staying after his class. Our conversations would follow up some of the items in the classroom but would soon drift to other areas of his expertise. He was a devoted student of

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southwestern Native American culture. One day he told me that after retiring he would like to be a bartender in a rural area of Arizona or New Mexico, because he found the stories of the folk that he met in those bars so fascinating.

Among all the great mathematicians I have known, Wilder was the most approachable. He had a wonderful sense of humor and his wisdom made him a father confessor to many of his colleagues. With his wife, Una, they made their home a center of hospitality. My children called them—and still do—Grandpa and Grandma Wilder. Every year at Christmas we still hang up by the chimney the stockings that Mrs. Wilder made for the kids—over 30 years ago!

After retiring from Michigan in 1967 he moved in 1969 to Santa Barbara and joined the mathematical activities there as an emeritus researcher. However, when time and health permitted he did visit some of his favorite haunts in the West.

Wilder's accolades were many. He became the first person at the University of Michigan to hold a University Research Chair (1947-67). Respected throughout the university he used his influence to fight for intellectual integrity. The university honored him with the Russell Lectureship in 1958-59 and an honorary doctor of laws degree in 1980. He also received honorary degrees from Bucknell University (in 1955) and Brown University (in 1958).

He was president of the American Mathematical Society in 1955-56 and president of the Mathematical Association of America in 1965-66. For the American Mathematical Society he delivered a number of special lectures including the Josiah Gibbs Lecture in 1969 and received the award for distinguished service to mathematics by the Mathematical Association of America in 1973. He was elected to the National Academy of Sciences in 1963.

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NOTE

1. Lucille Whyburn (R. L. Moore's first doctoral student at Texas). Lecture notes in mathematics. In *Algebraic and Geometric Topology*, vol. 64, ed. K. C. Millet, pp. 33-37. Springer Verlag, 1978.

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OLIN CHADDOCK WILSON

January 13, 1909–July 13, 1994

BY HELMUT A. ABT

OLIN C. WILSON was a stellar spectroscopist who spent his entire research career (1932-82) observing at the Mt. Wilson and Palomar observatories. He is known for being the first person to derive activity cycles in other stars analogous to the 11-year solar cycle. He also showed that the widths of the chromospheric Ca II emission lines in late-type stars provide accurate measures of their luminosities—the Wilson-Bappu effect. He is known for showing the complex internal motions in planetary nebulae and the Orion nebula; the latter shows evidence of shock waves and turbulence that is non-Kolmogoroff. He also demonstrated that many Wolf-Rayet stars are members of double stars and that they are under-massive. He also showed that the chromosphere of the supergiant zeta Aurigae consists of sheets or clumps, not a smoothly varying density gradient.

Olin Wilson was born in San Francisco, California, on January 13, 1909. His father was a lawyer who had moved to California in 1904, and his mother came from Iowa. They lived just south of Golden Gate Park. Fortunately they were not seriously affected by the earthquake and fire of 1906. His father worked downtown and rode the streetcars; they did not have a car. Their income was only moderate. Olin

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was an only child and his parents were kind to him and were supportive of his interests.

Olin went to the San Francisco public schools and showed an interest in the physical sciences. In high school when the substitute general science teacher showed her lack of knowledge about astronomy, Olin developed the habit of learning by himself at the public library. He also attended the lectures in Golden Gate Park that were organized by the Astronomical Society of the Pacific. For instance, he remembered hearing Sir Arthur S. Eddington, probably in 1924, when Eddington received the Bruce Gold Medal of the Astronomical Society of the Pacific. Olin, at 15, remembered that the talk was all about relativity and he did not understand it, but Eddington later became one of Olin's heroes. Because it was foggy in San Francisco nearly every night, he never looked through a telescope until he was at the university.

There was not much doubt that Olin would attend the University of California in Berkeley, partly because his family supported his desire for more education, partly because he had heard that Berkeley had an excellent astronomy department and partly because he could commute there by ferry. In addition, the university was cheap then; there was no tuition and the fees were only approximately $25 per semester. Olin graduated from high school in December 1925 and started at the university in January 1926 at the age of nearly 17.

In Berkeley Olin became disappointed with the astronomy department's concentration on celestial mechanics, because he was much more interested in the nature of stars and in the applications of physics to astronomy. Therefore he majored in physics but he did take courses under C. D. Shane, A. O. Leuschner, William H. Williams, Raymond T. Birge, and Donald H. Menzel. He helped support him-

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self by grading mathematics papers (at 50 cents per hour). He also worked for a while for Birge, who was then determining the best values of the fundamental constants. That led Olin to write one of his first individual scientific papers (1932) on the constancy of the speed of light. He demonstrated that the reported constancy to six decimal places of the length of the meter was inconsistent with the reported variation in the third decimal place in the speed of light.

Donald Menzel came to Lick Observatory in 1926, having been a student of Henry Norris Russell at Princeton. He had a great influence upon Olin because both were interested in applying physics to astronomical objects, or what we now call astrophysics. Menzel first worked on measuring the Lick flash spectra of the Sun taken at eclipses from 1900 through 1908. Menzel employed Olin to measure the strengths of the chromospheric lines, and Olin received credit in the publication. That work was published in the *Publications of the Lick Observatory*, Vol. 17, 1931.

Olin's heroes in astrophysics were Ünsold, Menzel, Bowen, and Eddington. Bowen's explanation for the "mutilated multiplets" in nebular spectra in terms of a fluorescent mechanism impressed Olin and contributed to his selecting Caltech for graduate work.

Olin's father died in 1929, leaving little for his wife and son. His father's brother helped out for a half year until Olin could support his mother and himself. Olin visited Caltech in 1929 and talked with Bowen and J. A. Anderson, who was then in charge of the 200-inch project. Olin was strongly tempted to do his graduate work at Caltech, partly because of the facilities at Mt. Wilson and partly because of the caliber of the Caltech staff. He was offered a teaching fellowship that paid $75 per month for 10 months, and he and his mother managed to live on that amount. People like Olin who had to struggle financially during the

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depression never forgot that experience and it affected their outlook throughout their lives.

Caltech did not have an astronomy department in 1930; that did not occur until 1948 after the dedication of the Hale 200-inch, when Jesse Greenstein was brought from Yerkes to organize such a department. Therefore Olin was in the physics department; he took all the required physics courses or passed them by exam, but was allowed to do an astrophysics project for his thesis. In 1934 he received Caltech's first Ph.D. in astrophysics. His thesis was done under the supervision of Paul W. Merrill on the Mt. Wilson staff and was entitled "Comparison of the Paschen and the Balmer Series of Hydrogen in Stellar Spectra." Characteristic of the policies of the time, the lead author was Merrill.

In his second year at Caltech Olin was offered a full-time position, which had just been vacated by Nicholas U. Mayall at the Mt. Wilson Observatory. However, he had to give up his studies for one year. He worked for several of the staff astronomers mostly measuring plates for radial velocities. That position allowed him to buy his first car (for $200). Before the end of that year J. A. Anderson told Olin that he was going to teach an astronomy course the next year and asked Olin to be his teaching fellow, an offer that he readily accepted. With the approval of Director Walter S. Adams, Olin worked half-time for the Mt. Wilson Observatory, took courses at Caltech, and worked as a teaching fellow for Anderson. He also did some observing on Mt. Wilson that led to several of his first publications.

After graduation in 1934 Director Adams offered Olin a position at the Mt. Wilson Observatory as a computer. In those days computers were people, not machines. His duties were the same as before, namely to assist staff astronomers.

That same summer Donald Menzel invited Olin to come

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to the Harvard summer school. Olin drove east with David Thackeray, who was in this country as a commonwealth fellow, in Thackeray's car. Also in attendance were Otto Struve, Harlow Shapley, and graduate students Leo Goldberg and Jesse Greenstein. Thackeray then returned to Europe and Olin took the train west. Struve invited him to stop off at Yerkes Observatory, where Olin had dinner with the Struves before returning west.

After returning to Pasadena Olin received an offer from Struve to join the staff at Yerkes. Olin thought much about this, weighing his respect for Struve as a stellar spectroscopist against their lack of high-dispersion equipment. At that time the McDonald Observatory was under construction. Finally Olin showed the offer to Director Walter Adams. Adams, basically a very parsimonious person, hemmed and hawed, said he didn't want to lose Olin, and offered a staff position at $100 a year less than Struve's offer of $2,500. Olin decided that the difference was less than the increased home heating cost in the winters, and he became a staff astronomer.

His initial papers were often with astronomers Paul W. Merrill, Roscoe F. Sanford, and William H. Christie, but gradually they gave way to individual papers. He became well acquainted with and learned from the other astronomers, especially the stellar and solar ones who worked when the Moon was bright. Although he worked well with them, his political views were diametrically opposed to those of Merrill, and they avoided that topic. Merrill was staunchly Republican while Olin was liberal. In fact, Olin's political views were conservative on financial issues (due to his experiences during the Great Depression) and liberal on social problems, a combination that I have seen in others but for which I have not heard a label. More importantly, Merrill's philosophy for doing astronomy was to collect enough ob-

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servations until they made sense, emphasizing unusual stars, while Olin's was based more on a physical understanding, particularly of normal stars.

When America entered World War II in December 1941, Olin was disgusted, because he did not like war. However, he felt that he should do something to help, so he asked Ira Bowen at Caltech who referred him to William A. Fowler, who had a war-related project and welcomed Olin's help. Olin was not told, for secrecy reasons, what they were doing but he quickly surmised. They took him to Goldstone Lake, where they were testing solid-fuel rockets. The rockets were about 2.5 inches (6 cm) in diameter and a yard (1 m) long. Most of them exploded because they were designed incorrectly; hence the remote location.

The solid fuel (propellant) was a mixture of nitrocelluloid plus nitroglycerine. To fit it into the rockets, it was often turned on a lathe. One day Olin missed by one minute being killed in an explosion when an assistant was shaping the propellant on a lathe and it exploded.

In the course of his work Olin made an invention that is still used in rockets to this day. He saw that the support for the rocket fuel could be improved, so he designed a new one that was easier to install and increased the throughput by 5 percent. Olin also operated a school for Navy personnel, teaching them the construction and use of rockets. Other astronomers who worked on the rocket project were Bowen, Robert King (of later laboratory transition values fame), Horace Babcock, Franklin Roach, Gerald Kron, and Nick Mayall.

In 1944 Olin left the rocket project because he did not think it was progressing. Instead be went to work for the aircraft torpedo project on Green Street in Pasadena. The challenge was that an anti-submarine torpedo launched from a plane often had its steering mechanism disturbed upon

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impact with the water. Therefore they had a torpedo-launching tube aimed at a reservoir behind Morris Dam in Azusa Canyon east of Pasadena to simulate the airplane launches. It was known that the Japanese air-to-submarine torpedoes were far superior, which was one reason for their success in Pearl Harbor.

One important personal benefit came from his war work, and that was in meeting Katherine E. Johnson, a secretary who also worked at Caltech, mostly in publications and later in its radio astronomy group under John Bolton. She and Olin were married on September 3, 1943, and enjoyed 50 years of happy life together. They complemented each other. Olin enjoyed his work, his home life, and an occasional friend or two over. Katherine was far more outgoing and loved social occasions. She kept Olin "connected" with the outside world. She acquiesced to his wishes but sometimes guided him farther into the world of other people than he would have ventured himself. She loved to talk with people while he liked to read a book. They raised two wonderful children. Nicole, born on December 24, 1945, eventually married chemist Dave McMillan, who joined the Purdue University faculty in West Lafayette, Indiana. They have two children. After the children were somewhat self-sufficient Nickie worked as the secretary to the mayor of West Lafayette. Randy Wilson married Erin and they also have two children. Randy became a high school counselor in Encinitas, California, just north of San Diego.

Olin returned to the Mt. Wilson Observatory in January 1946 at the same time Bowen became director in place of Walter Adams. Olin relished the freedom of being able to talk with others about what he was doing. He became acquainted with Walter Baade, who joined the Mt. Wilson staff in 1932, a year after Olin came, and he was impressed with Baade's abilities and insights. Baade once invited Olin

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to join the nebular group, but after some thought Olin decided to stay where his training had led him.

Except for his work during World War II, he remained at the Mt. Wilson Observatory (later successively called the Mt. Wilson and Palomar observatories, the Hale Observatory, the Mt. Wilson and Las Campanas observatories, and the Carnegie Observatories) for the remainder of his career. He continued with high-dispersion stellar and nebular spectroscopy until his retirement in 1974. He aided Sinclair Smith, Ira S. Bowen, and others with some of the new instrumentation and had equipment built for his use, but generally he did not design it or do much laboratory work. He lived through the times of sole dependence upon prism spectrographs, then grating spectrographs, and finally electronic detectors.

In accord with Carnegie Institution policies at the time he had to retire at age 65. However, he obtained an emeritus position for several more years beyond that to complete work on stellar activity cycles. His last observing was done in 1980. Later he was persuaded to sell his house in Pasadena and move with Katherine to West Lafayette, Indiana, to be near their daughter and her family. I last saw the Wilsons in March 1994, where they enjoyed the frequent company of Nickie and Dave and their children. Olin bought a pool table; astronomers used to play pool on cloudy nights, but now most of them watch television. Olin missed his astronomical friends. After a short illness he died on July 13, 1994, at age 85. Katherine was increasingly bothered with retinitis pigmentosa (tunnel vision) during the last three to four decades of her life to the point that she could not walk unassisted. After Olin's death she moved to California to be close to Randy and his family. She died on December 2, 2000.

Olin rarely traveled, even during vacations. He loved

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his pipe, reading, watching football on television, playing poker, and long walks on Friday evenings. He hated pretense and egotism in people but respected honesty. He always spoke his mind, often in salty language. He epitomized a good scientist in being skeptical, honest, modest, generous, and never arrogant.

With regard to research, Wilson called himself an "opportunist." That label was realistic for him and others because with the new tools of astrophysics, astronomers could for the first time draw far-reaching conclusions from stellar spectra. For instance, the identities and strength of certain spectral lines told them, through the use of atomic theory and laboratory transition values, the atomic abundances and nuclear processes that have occurred in the stars. The widths and shapes of the spectral lines told them about stellar rotation and atmospheric turbulence. The presence of sharp non-stellar absorption lines in the spectra told them about the amounts, temperatures, and random motions in the foreground interstellar matter. The presence of emission lines told them about chromospheric matter and excited matter around the stars. It must have been a strong temptation to observe one spectrum after another and discuss each special case. It was, and still is, a great and productive time for stellar spectroscopists. Whereas a few spectroscopists concentrated upon one class of stars for decades, Olin's technique was to study a class of objects for several years until he had learned as much about them as seemed possible at the time.

Olin's initial research was governed partly by the interests of his collaborators at the Mt. Wilson Observatory. For instance, Christie got him interested in the nearly unique opportunity furnished by zeta Aurigae for exploring the nature of the chromosphere of a supergiant (or perhaps bright giant) star. Every two and two-thirds years a relatively

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small (5-solar-radii) B star is eclipsed by the 300-solar-radii K supergiant. The latter has a very extensive outer atmosphere or chromosphere such that it takes a week for the light from the B star to pass through successive layers of the K chromosphere. Each night for a week the atmospheric absorption lines superimposed on the B star spectrum yielded abundances, temperatures, pressures, turbulent velocities, and large-scale mass motions. Because of the period, not every eclipse is favorably placed in the sky for observation.

Christie and Wilson (1935) explored the 1934 eclipse and Wilson analyzed the spectra from the 1939-40 eclipse (1948). The weather during the 1947-48 eclipse was excellent, and Wilson obtained coude spectra every night during ingress (into totality) and egress (coming out of totality). By that time Olin obtained the assistance of Helmut A. Abt, one of the first four graduate students (with Allan Sandage, Morton S. Roberts, and James Parker) in the new astronomy department started at Caltech in 1948. The resulting analysis was published as the first *Astrophysical Journal Supplement* (1954). Of particular interest was the result that the whole chromosphere should have been ionized by the B star's radiation unless the material is concentrated in a dozen or so (along the line of sight) thin dense sheets or clumps. Thus, the assumption of a smoothly varying density gradient in the outer atmosphere is far from the truth.

Olin joined astronomers Adams, Merrill, Sanford, and later Deutsch, Kraft, and Preston in the study of the interstellar lines that appeared in the spectra of early-type (or distant) stars. In each star's spectrum the interstellar lines had their own strength and radial (line-of-sight) motions. They soon realized that there was a pattern of radial velocities that depended upon galactic longitude. Thus, they attached a large (2- x 4-foot) graph to the hallway wall and each person added the position on the graph of the radial

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velocity of the interstellar matter for each star newly observed. Gradually it was found that these interstellar velocities showed a double-wave pattern that yielded the constants of galactic rotation. Various members of the team published the results. Merrill and Wilson published lists of unidentified interstellar lines, and Sanford and Wilson discussed the doublet ratio for calcium and sodium. Wilson (1939), having the tools of radiative transfer, could discuss the abundances and significance of a doublet ratio that showed saturation with increasing line strength.

Observations of the spectra of novae (exploding stars that become about 100 times brighter within two days) persuaded Olin to compute the line profiles of an expanding star, again using the tools of radiative transfer (1935). Further, the recent work relating interstellar line strengths with mean distance for stars could be used to determine approximate distances to several novae. This allowed Olin to obtain luminosities (absolute brightnesses) of novae at maximum light (1936). This reasoning was also applied by Sanford and Wilson (1939) to obtaining luminosities of Wolf-Rayet stars.

Olin started to become intrigued with the nature of Wolf-Rayet stars, ones that show extremely broad emission lines characterized by high temperatures. He soon found that many of them are in spectroscopic binaries with normal O or B stars, allowing a determination of their masses. Actually, the discovery of the first Wolf-Rayet binary (HD 193576) was serendipitous. He was looking for objects in which both the Ca II H and K interstellar lines could be measured without interference from Hϵ of hydrogen that is found in most early-type stars. He looked at two plates taken of HD 193576 on a spectra comparator and was surprised to see that if the interstellar lines were aligned, the emission bands moved. That meant that he had discovered the

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first Wolf-Rayet binary, and he could therefore determine its mass. The Wolf-Rayet stars were found to be under-massive for their luminosities (1940). Another strange characteristic of such stars is that their mean radial (line-of-sight) velocities are consistently nearly 100 km s^{-1} larger than for the companions. Could this be due to an expanding atmosphere as in the case of novae?

Olin suspected that Wolf-Rayet binary HD 193576 was also eclipsing and persuaded Sergei Gaposchkin to find out. Gaposchkin found that it was eclipsing and Gerald Kron obtained a photoelectric light curve. This allowed Wilson (1942) to show that the Wolf-Rayet star is larger and brighter than the B star. He explored the expanding-envelope hypothesis for the broadened emission lines. That hypothesis led to a prediction of a transit time effect, a difference between the observed times of eclipses and the predicted times based on the spectroscopic orbit. The observations led to constraints on the ejection velocity, and he showed that the emission line widths could not be produced simply by an expanding envelope. No satisfactory final model was produced.

After the interruption due to World War II his observing concentrated on the spectra of planetary nebulae. He finished previous work on interstellar lines and Wolf-Rayet stars, but he started to explore the internal motions of planetaries with "slitless" spectra. Guiding on a nearby star, he allowed the light from the planetary to go through the spectrograph without being partitioned with a slit. Because the spectra of planetaries consist of a few emission lines with a very weak continuum from the central star, he obtained a velocity "picture" of the planetary at each emission line. In addition, the nuclear star has absorption lines whose velocities vary with excitation and ionization potentials. The highest excitation lines are formed deepest in the atmo-

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sphere and the lowest are formed farthest out. Meanwhile the nebular lines show the presence of previous ejections. However, not all the line widths were understood. Some of this work was done together with Lawrence Aller at the University of California, Los Angeles, and Aller is continuing this work to the present day.

Olin could not resist doing some "neat" projects for which the facilities allowed solutions. For instance, the coude spectrograph of the Hale 200-inch allowed him to obtain spectra of 15 red giants in the globular cluster M92. The scatter in the radial velocities for constant-velocity stars is a direct result of the mass of the cluster. Wilson and Coffeen (1954) obtained the reasonable mass of 3.3 million solar masses and a mass-to-luminosity ratio of 2.0 in solar units.

A more difficult project, funded by the Office of Naval Research, was to determine the internal motions in the Orion nebula (1959). To do so by setting the spectrograph slit successively on each of many strips in the nebula would have been too wasteful of observing time on the 200-inch. Instead Olin devised a multiple slit of 31 slits, each separated by 1.3 seconds of arc, which is approximately the seeing size. They measured thousands of points in the nebula, each in several lines of different elements on 25 spectrograms. The lines showed some asymmetries and occasional doubling, indicating "bubbles" of 25 km s^{-1}. The velocities indicated internal turbulent velocities averaging 5-7 km. However, the velocities are not characteristic of a Kolmogoroff law but rather of shock waves producing subsonic eddies.

Another "neat" project was to ask if the redshift of a distant galaxy is proportional to wavelength, as it must be if it is due to a Doppler motion. Minkowski and Wilson (1956) measured the redshift in Cygnus A from 3830 to 6472 angstroms and also compared the results with radio measures. They found the redshift to be constant within 3 parts in 10^9

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per 1000 angstroms, giving good support to the interpretation of the redshift in terms of motion.

In the late 1950s Olin did several projects to relate the spectral characteristics of late-type dwarfs with their photoelectric colors. In obtaining high-dispersion spectra he noted several unusual things. Some (1957) had emission lines in the bottoms of their deep broad Ca II H and K lines. He first saw that before World War II in the spectrum of Arcturus when he was just learning to use the 100-inch coude spectrograph. The H and K lines have such a high opacity that at their centers one is seeing only the outermost part of their atmospheres, namely their chromospheres. This is another example of a serendipitous discovery. He knew from solar research that the H and K emission was seen only in active regions and thus showed a strong variation during the 11-year solar cycle. He reasoned that if he followed the strength of the Ca II emission in other stars, he might discover "stellar cycles" in them too. Would they be 11 years long also, or shorter or longer? Would they depend on the age of the stars, their chemical makeup, or their temperatures?

In spectra placed on the spectra comparator he noticed that 61 Cygni (a dwarf) had narrow emission lines but the supergiant Betelgeuse (α Orionis) had very broad emission lines. He found that very interesting and baffling, but he couldn't do anything further about those observations, because he was preoccupied with planetary nebulae. An invitation to speak in a conference on stellar atmospheres in Bloomington, Indiana, in 1954 persuaded him to organize his material and announce the dependence of emission line widths upon luminosity (1954).

About that time Vainu Bappu was at Mt. Wilson Observatory as a Carnegie fellow, and Olin asked him if he were interested in getting some more spectra. He added about 5

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percent to the collection and then returned to India without becoming involved with the measurements or discussion. Olin found that when he plotted the logarithm of the emission line widths against the absolute visual magnitudes, which are also on a logarithmic scale, he obtained a straight line over 15 mag. of luminosity. This straight-line relation holds only for visual absolute magnitudes for stars of various spectral types, G to M, not for bolometric magnitudes or other systems. Olin was frustrated that theoreticians were unable to explain this drastic effect. He could only calibrate it for the values of the parallaxes available at that time for the Sun, Hyades, and Pleiades. However, he found that the calcium line widths in visual pairs agreed well with their magnitude differences.

Olin and Sir Richard Woolley also looked at calcium line strengths and found that they were strong in young stars, weakened with age, and showed that galactic orbital eccentricities and inclinations increased with age as stars suffered encounters.

The project that took most of his time until retirement was to measure the strengths of the calcium K emission line to get the stellar analogs of the solar cycle. The observations were started in 1966 and his last observations were made in 1978. He selected 91 main sequence stars between F5 and M2. The emission lines become more prominent in the later-type stars, because the photospheric continuum is decreasing. He found that nearly all stars show slow variations and in 12 years he found periodic cycles for about 10 of them. This work is being continued by Sallie L. Baliunas of the Harvard-Smithsonian Center for Astrophysics, using a spectrograph designed by Arthur Vaughan with NASA funds.

Olin Wilson published approximately 94 papers, primarily in the *Astrophysical Journal* and the *Publications of the*

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Astronomical Society of the Pacific. These extended for 50 years from 1932 to 1982.

Olin Wilson was president of the Astronomical Society of the Pacific during 1954-56. He received the society's Bruce Gold Medal in 1984 for a lifetime of outstanding astronomical research. He was elected to the National Academy of Sciences in 1960. He was chosen to give the 1977 Russell Lecture of the American Astronomical Society.

Olin assisted Ira Bowen in installing the coude spectrograph at the 200-inch Hale telescope in the early 1950s. He also made up the observing schedules for the light time on the Mt. Wilson and Palomar telescopes for more than 20 years until 1973. He did not teach courses at Caltech, feeling that his position at Mt. Wilson allowed him the freedom to do research, which should not be diluted by other activities.

Few people realized his effectiveness behind the scenes. Administrative members of the Carnegie Institution trusted him for his honest and carefully considered recommendations. For instance, he seems to have been the first person to persuade (in his letter to Merle Tuve of March 15, 1963) the Carnegie Institution to build a southern observatory. He pointed out that Mt. Wilson had lost its preeminence among Northern Hemisphere observatories, but the Southern Hemisphere was fertile ground for research.

I AM INDEBTED to Randy Wilson and David DeVorkin for use of a transcript of DeVorkin's long interview with Olin in July 1978. I thank Donald E. Osterbrock for his notes on a phone conversation in 1986 with Olin about his early life and for his review of an early draft of this memoir. George Preston's warm, sincere obituary for Olin (*Proc. Astron. Soc. Pac.* 107[1995]:97) was helpful and is a model for obituaries. The frontispiece, photographed in 1974, is reproduced with permission from the Carnegie Observatories.

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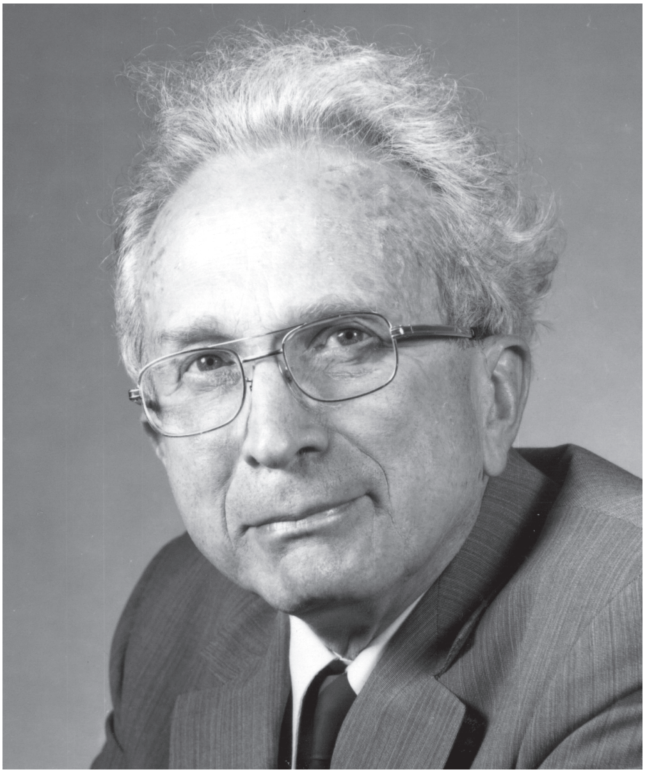

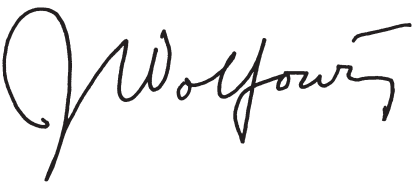

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JACOB WOLFOWITZ

March 19, 1910–July 16, 1981

BY SHELEMYAHU ZACKS

JACOB WOLFOWITZ, A GIANT among the founders of modern statistics, will always be remembered for his originality, deep thinking, clear mind, excellence in teaching, and vast contributions to statistical and information sciences. I met Wolfowitz for the first time in 1957, when he spent a sabbatical year at the Technion, Israel Institute of Technology. I was at the time a graduate student and a statistician at the building research station of the Technion. I had read papers of Wald and Wolfowitz before, and for me the meeting with Wolfowitz was a great opportunity to associate with a great scholar who was very kind to me and most helpful. I took his class at the Technion on statistical decision theory. Outside the classroom we used to spend time together over a cup of coffee or in his office discussing statistical problems. He gave me a lot of his time, as though I was his student. His advice on the correct approach to the theory of statistics accompanied my development as statistician for many years to come. Later we kept in touch, mostly by correspondence and in meetings of the Institute of Mathematical Statistics. I saw him the last time in his office at the University of Southern Florida in Tampa, where he spent the last years

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of his life. I regret that I did not have the opportunity to associate closer with this great man.

Jacob Wolfowitz was born in Poland on March 19, 1910. He immigrated to the United States with his parents, Samuel and Helen (Pearlman) in 1920. Jacob Wolfowitz got his education in New York City. Despite the depression he had the opportunity to get a college education in the City College of New York, and received his B.S. degree in 1931. The depression was then at its depth and jobs were scarce. Wolfowitz succeeded in becoming a high-school mathematics teacher and continued to work as a teacher until 1942. I was told that it was very difficult, especially for an immigrant candidate, to obtain a job as a high-school teacher in those years. People had to take very difficult certification exams. Wolfowitz achieved first place among hundreds of contestants. During these years he also studied mathematics as a part-time graduate student at New York University, from which he received his Ph.D. degree in mathematics in 1942. In 1934 Jack Wolfowitz married Lillian Dundes, who became his cherished companion for the rest of his life. Their children, Laura Mary and Paul Dundes, were born in 1941 and 1943. Laura is a biologist living in Israel with her family, and Paul is a political scientist serving as deputy secretary of defense in Washington, D.C.

In 1938 Wolfowitz met Abraham Wald, with whom he collaborated till the tragic death of Wald in 1950. The collaboration of Wald and Wolfowitz produced some of the most important results in theoretical statistics. Wald was a professor of statistics at Columbia University. During World War II Wolfowitz joined the Statistical Research Group at Columbia University, where the group worked for the war effort. The Wald sequential probability ratio test was developed there at that time. In 1945 Wolfowitz moved to the University of North Carolina at Chapel Hill as associate

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professor but stayed only one year. In 1946 he joined the faculty of the Statistics Department at Columbia and worked there till 1951. In 1951 he joined the Mathematics Department at Cornell University as professor and stayed until 1970. In 1970 Wolfowitz moved to the University of Illinois as professor of mathematics. Upon his retirement from the University of Illinois in 1978 he moved to Tampa, Florida, where he accepted the position of distinguished professor at the University of South Florida. Professor Wolfowitz died of a heart attack in Tampa on July 16, 1981.

Professor Wolfowitz spent sabbatical years at various places. As mentioned earlier, in 1957-58 he was at the Technion, Haifa, Israel. In 1967 he visited both the Technion and the University of Paris. In 1969 he visited the University of Heidelberg. In 1966-67 he was a fellow of the Guggenheim Foundation. In 1975 he received an honorary doctorate from the Technion. Professor Wolfowitz was elected to membership in the National Academy of Sciences in 1974. He was also a fellow of the American Academy of Arts and Sciences, Institute of Mathematical Statistics, Econometric Society, and the International Statistical Institute. The following are some of the celebrated meetings in which Wolfowitz delivered honorary lectures: International Congress of Mathematics, American Mathematical Society, German Mathematical Society, and All-Soviet Congress of Mathematics. He gave the Rietz lecture and the Wald lecture at the Institute of Mathematical Statistics meetings and the Shannon lecture at the Institute of Electrical and Electronics Engineers. Professor Wolfowitz also served one term as president of the Institute of Mathematical Statistics.

Up to the sudden death of Abraham Wald (a plane crash while visiting India) in 1950, Wolfowitz collaborated in research mainly with Wald. Starting in 1952 he collaborated with professors Arye Dvoretzky of Hebrew University, Jack

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Kiefer of Cornell and University of California at Berkeley, and Lionel Weiss of Cornell University. In the 1960s and 1970s Wolfowitz collaborated with R. Ahlswede in his research on coding theory. Lionel Weiss wrote an excellent summary of the research contributions of Wolfowitz, which can be found in the volume *Leading Personalities in Statistical Sciences.*[1] Another more comprehensive summary is given in the collection of Wolfowitz's papers compiled by Jack Kiefer.[2] Wolfowitz contributed in his research to the following areas of statistical theory: nonparametric inference, sequential analysis, statistical decision theory, asymptotic statistical theory, maximum probability estimators, design of experiments, probability theory, queuing and inventory theory, and information theory. I will give here a short nontechnical summary of these accomplishments.

Nonparametric statistical inference is an area of estimation or testing hypotheses that does not assume a particular functional form of the distribution of the observed random variables. The nonparametric procedures are also called distribution free. A nonparametric estimator of the distribution function (cdf), F(x), of a random variable, based on n iid random variables $X_1, X_2, \ldots, X_n$ is, for example, the empirical distribution function $F_n(x) = \sum_i I(X_i < =x)/n$. Procedures based on linear functions of the ranks of the observations within the samples are another familiar type of distribution-free, or nonparametric, procedure. A considerable number of papers published by Wolfowitz (some are with Wald or with Kiefer) discuss various properties of nonparametric procedures. They deal with confidence intervals for the cdf F(x) (1939); asymptotic minimaxity of the empirical cdf (with Dvoretzky and Kiefer, 1956; with Kiefer, 1959, 1976); convergence of the empirical distribution (1960); run tests (1943, 1944); and permutation tests (1944). The collaborations with Kiefer in this general area

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concentrated on procedures that are of the first of the two general types described above. They include the first general description of the so-called nonparametric maximum likelihood procedure that has become a popular modern technique. Wolfowitz's famous paper (1952) generalizing the Robbins-Monro stochastic approximation procedure also belongs to the nonparametric domain, as well as the sequential domain discussed below.

Sequential analysis is a branch of statistical experimentation in which observations are taken sequentially, one at a time or in groups. After each observation a decision is made based on all previous results whether to continue sampling or stop. At termination an inference is made, for example, an estimate or hypothesis test, concerning the distribution of the observed random variables or some parameter(s) or functional(s) of it. Wald and Wolfowitz were the pioneers of modern sequential analysis. Wald developed the widely applied sequential probability ratio test (SPRT). Wolfowitz collaborated with Wald in proving the optimality of the procedure. As written by Kiefer in the introduction to the *Selected Papers*,[2] the proof of Wolfowitz and Wald (1945, 1966) "is one of the strikingly beautiful results of theoretical statistics." The optimality result asserts that an SPRT with prescribed type I and type II error probabilities minimizes the expected sample size of all tests having error probabilities that do not exceed those of the SPRT. Wolfowitz was most proud of his contribution to Wald's sequential test. In later years Wolfowitz worked further on stochastic approximations of the Robbins-Monro type. He proved the strong convergence of the procedure in a more general setup than the original one. In 1972 Wolfowitz and Lionel Weiss published a paper on the sequential fixed-width confidence interval estimation of a translation parameter. This paper treats the problem in a nonparametric setup and proves its

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asymptotic optimality. They also published in 1972 on asymptotically efficient sequential procedure that is equivalent to the t-test.

The decision theoretic approach is found in many of Wolfowitz's papers. A loss function is assumed in general form and a procedure is chosen to minimize the risk (expected loss). Often Wolfowitz applied the minimax principle to overcome the dependence of the optimal procedure on the unknown parameters of distributions. Bayes solutions to a decision theoretic formulation are a minimization of the prior risk, which is an average risk over the space of unknown parameters according to some (prior) distribution. Given the observations the Bayesian procedure is the one minimizing the posterior risk. Together with Wald (1949, 1950) Wolfowitz characterized Bayes solutions to sequential decision problems, which were mentioned in the previous section.

In 1953 Wolfowitz, with the collaboration of Dvoretzky and Kiefer, generalized the results to sequential methods with continuous time in place of the discrete time formulation of the previous results. Many of the results of discrete time sequential analysis are carried over. These two early papers in the field showed clearly the advantage of solving discrete time problems by working first in continuous time, a technique that has become a contemporary staple of many areas of statistics and applied probability.

Asymptotic statistical analysis is concerned with the limiting behavior of sequences of procedures (estimators or test functions), each one corresponding to a sequence of increasing sample sizes. Two criteria of asymptotic behavior are prevalent: consistency (involving convergence in probability to the true value) and efficiency (involving the rate of approach). Wald's approach to the proof of consistency of the maximum likelihood estimator was extended in 1956

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(with Kiefer as coauthor) to a general class of estimators, including nonparametric models. Wolfowitz developed the minimum distance method (1957) for estimating parameters or functionals of distributions by minimizing the distance between the empirical distribution and the family of distributions for the model under consideration. The method yields consistent estimators in complicated problems. Wolfowitz's research on the asymptotic properties of the maximum likelihood estimator, with the collaboration of Weiss, led to the development (1969) of maximum probability estimators.

Let R be a specified region in the parameter space θ. Suppose that θ is the true value of a parameter. R could be a neighborhood set around this point. An estimator of θ, d_n, based on n observations is a maximum probability estimator (MPE) if $\lim P_\theta\{k(n)(d_n - \theta) \ \varepsilon \ R\}$ is maximal in a class of estimators. $k(n)$ is the rate of approach, usually $k(n) = \sqrt{n}$. Here the efficiency of an MPE is defined relative to R. Maximum likelihood estimators under the common regularity conditions are MPE. However, estimators could be MPE in more general models in which the common regularity conditions of the maximum likelihood estimator are not satisfied. The reader is referred to the Springer lecture notes (1974) on the subject, written jointly with L. Weiss.

In 1959, 1960, 1964, and 1965 Wolfowitz published a sequence of papers, written jointly with Kiefer, dealing with optimality of regression designs under least-squares estimation. The problem is to determine an optimal set of design points at which to make observations to attain certain optimality conditions. Several criteria of optimality are defined in these papers. Some of them are shown (surprisingly) to be equivalent under specified conditions. Some methods were also created for finding optimal designs. These papers created a sub-field of optimal design that is an active research area to this day.

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From time to time Wolfowitz published papers on problems in probability theory, which arose out of his research in mathematical statistics and applied stochastic processes. An early one is his paper on the notion of recurrence (1949). With K. I. Chung he published a paper in 1952 on a limit theorem in renewal theory. In this paper they generalized an earlier result of Erdös, Feller, and Pollard. In 1967 Wolfowitz published a paper on the moments of recurrence times. Wolfowitz's first paper in queuing theory (with Kiefer) appeared in 1955. They studied queues with many servers. In 1956 Wolfowitz and Kiefer published a joint paper on the characteristics of queuing processes, which yielded important results in random walk theory. Contributions to inventory theory were done in a series of three papers (with Dvoretzky and Kiefer) published in *Econometrica* in 1952 and 1953. These contributions were pioneering at the time. They proved, for example, the optimality of the celebrated (s, S) policy in inventory control.

The first paper of Wolfowitz's on the coding of messages subject to random errors appeared in 1957. From that time till the end of his life Wolfowitz published vigorously on this subject. In 1961 he published a classical book in this area under the title of *Coding Theorems of Information Theory*. The book immediately became a great success; a second edition was published in 1964 and a third edition in 1978. At the time it was the only book concentrating on the probabilistic aspects of noisy channels. The book became indispensable for specialists in the field but also served well as an introductory book because of its brief and simple explanation of the problems and their solutions. The second edition was translated into Russian. In the preface to the Russian translation the editor wrote that the "exposition is compact and elegant; the system of notation is complicated but logical." The reader is referred to the introduction of *Selected Papers*[2]

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for additional explanations and exposition of the problems treated in Wolfowitz's papers.

In addition to being prolific in research Jacob Wolfowitz was a very well read person. He was interested in current affairs and used to discuss issues of the day with his colleagues. He fought at the time for the liberation of Soviet Jewry. He was a friend and strong supporter of the state of Israel and had many friends and admirers there. We will always remember him as a great scholar, a principled person, and a charitable man.

I GRATEFULLY ACKNOWLEDGE the input and help of Professor Lawrence Brown.

NOTES

1. N. L. Johnson and S. Kotz, *Leading Personalities in Statistical Sciences: From the Seventeenth Century to the Present,* p. 215. New York: John Wiley, 1997.

2. J. Kiefer. *Jacob Wolfowitz Selected Papers.* New York: Springer-Verlag, New York, 1980.

3. The following selected bibliography is restricted to 25 papers. Wolfowitz wrote 114 papers in addition to books. It is difficult to choose the most important papers; this bibliography, therefore, has gaps. The reader is referred to Note 2 above for a more comprehensive partial list. In the article I wrote the year in which Wolfowitz published papers on the discussed topics. Many of these papers are not lsited in the selected bibliography. I should remark that since 1939 Wolfowitz published every year until his death in 1981. Anyone interested in the complete list of publications of Jacob Wolfowitz can get it from the author of this biographical memoir.

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