Biographical Memoirs: V. 90

ISBN
978-0-309-12148-4

460 pages
6 x 9
HARDBACK (2009)

Office of the Home Secretary, National Academy of Sciences

Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES
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VOLUME 90

THE NATIONAL ACADEMIES PRESS

WASHINGTON, D.C.

www.nap.edu

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INTERNATIONAL STANDARD BOOK NUMBER 978-0-309-12148-4
INTERNATIONAL STANDARD SERIAL NUMBER 0077-2933
LIBRARY OF CONGRESS CATALOG CARD NUMBER 5-26629
Available from
THE NATIONAL ACADEMIES PRESS
500 FIFTH STREET, N.W.
WASHINGTON, D.C. 20001

PRINTED IN THE UNITED STATES OF AMERICA

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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.

JOHN I. BRAUMAN
Home Secretary

Biographical Memoirs

VOLUME 90

GERALD DONALD AURBACH

March 24, 1927–November 4, 1991

BY IRA PASTAN

GERALD D. AURBACH REVOLUTIONIZED OUR understanding of bone metabolism and calcium homeostasis.

Gerald D. Aurbach was born in Cleveland, Ohio, on March 24, 1927, but grew up in Washington, D.C. After spending one year in the U.S. Army-Air Force at the end of World War II, he entered the University of Virginia, where he received his bachelor's degree in 1950 and his M.D. in 1954. These years at the University of Virginia were some of the happiest days of Gerry's life and he remained devoted to the University of Virginia for the remainder of his life. He received the singular honor of being named the Centennial Alumnus of the university in 1988. Ironically it was during a visit to the university on November 4, 1991, that he was fatally injured in a senseless act of violence.

Gerry began his studies on parathyroid hormone early in his career while a medical student at the University of Virginia in the laboratory of Dr. William Parson. Dr. Parson was chief of medicine and a professor of medicine at the University of Virginia. A renowned endocrinologist, Parson had worked with Dr. Fuller Albright, a leader in the field of endocrinology and metabolism, while training at the Massachusetts General Hospital. After graduation from medical school, Gerry took an internship at the New England Medical

Center and a residency at the Boston City Hospital. In 1956 he joined the laboratory of Ted Astwood as a research fellow at Tufts Medical School, where he was able to continue his studies on parathyroid hormone. This area of research occupied most of his research career. At that time only a few polypeptide hormones had been characterized. It was known that injection of an extract of parathyroid glands into test animals raised calcium levels in the blood, but the nature of the active substance was not known. Many investigators had already tried to purify this factor without success. Gerry thought of the novel idea of using phenol extraction to remove the hormone from proteins with which it was associated and to inactivate proteases causing its degradation. This approach was very successful and a paper published in 1959 established Gerry as the leader in the field.

Despite his early success Gerry felt he needed further training in research and applied to a new, prestigious training program at the National Institutes of Health in Bethesda, Maryland. Gerry was selected for the Research Associate Training Program and in 1959 joined the laboratory of William Jakoby, where he received training in enzyme biochemistry. In 1961 he accepted a research associate position in the Metabolic Diseases Branch of the National Institute of Arthritis and Metabolic Diseases and established his own research group. He spent his entire career in the branch, eventually becoming its chief in 1973.

When Gerry started his own laboratory, he returned to the parathyroid hormone field. His first goal was to obtain sufficient amounts of highly purified protein to determine its sequence. This was a challenging problem because the amount of hormone in each parathyroid is very low; parathyroid glands are small and protein purification techniques were in their infancy. Aurbach and colleagues were able to overcome these obstacles and they obtained sufficient amounts of highly purified bovine and human hormone to fully characterize the protein. In addition, they were able to chemically synthesize a biologically active form of the protein. These studies opened up the field of parathyroid hormone research, eventually leading to the understanding and treatment of several important diseases, including osteoporosis.

The availability of purified parathyroid hormone enabled Gerry to investigate several new areas of research. One of these was to develop a radioimmunoassay that could measure parathyroid hormone levels in the blood and other body fluids. This assay became a valuable tool in the diagnosis of hyperparathyroidism. A second was to radiolabel parathyroid hormone and determine its pharmacokinetic behavior. And a third was to investigate the mechanism of parathyroid hormone action. Influenced by the classic studies of Earl Sutherland, Gerry reasoned that parathyroid hormone acted on its target tissues, kidney and bone, by raising cyclic adenosine monophosphate (AMP) levels. To study this process in animals and humans he and his fellow, Lew Chase, developed a highly sensitive assay for cyclic AMP. They then used this assay to show that parathyroid hormone activated adenylate cyclase and elevated cyclic AMP levels in target cells and in the urine. The cyclic AMP in the urine could be used to assess the parathyroid hormone status of patients, an invaluable clinical tool. He was also in the position to investigate some hereditary diseases of calcium metabolism. Fuller Albright had described pseudohypoparathyroidism as the first example of a disease of hormone resistance rather than hormone deficiency. Gerry and Lew Chase discovered that patients with pseudohypoparathyroidism failed to elevate cyclic AMP levels in the urine in response to parathyroid hormone and showed that the biochemical basis of this was a defect in the ability of parathyroid hormone to activate renal adenylate cyclase. And later, with Allen Spiegel, Gerry showed that the specific defect in adenylate cyclase was a deficiency in the guanine nucleotide binding protein Gs.

Gerry's contributions extended outside the parathyroid hormone field and had broad implications for the entire field of hormone action. One very important contribution was the design and synthesis of radiolabeled iodohydroxybenzylpindol. This compound was used as a marker of specific binding to the beta-adrenergic receptor, and enabled Aurbach and colleagues to study in detail a critical initial step in hormone action.

Gerry was a fine physician and his groundbreaking research in the parathyroid hormone field resulted in many patients being referred to the National Institutes of Health from community hospitals as well as large medical centers in the United States and abroad. Gerry being a compassionate person and physician kept in close touch with his patients over the years. In collaboration with John Doppman, a radiologist from the Clinical Center, and many endocrine surgeons, including Sam Wells, Murray Brennan, and Jeff Norton, Gerry's team achieved an extraordinary success rate in treating patients with hyperparathyroidism who had previously had unsuccessful neck exploration; thus, making the NIH Clinical Center the leading referral center for the treatment of hyperparathyroidism.

Additionally, studies by his group, and particularly with Steve Marx, led to the characterization of several hereditary forms of hyperparathyroidism (multiple endocrine neoplasia Type 1, familial hypocalciuric hypercalcemia, hyperparathyroidism-jaw tumor syndrome) and the eventual identification of the genetic basis for each of these diseases. One striking example of Gerry's impact on clinical medicine is the current excitement about using parathyroid hormone to treat osteoporosis.

Gerry's reputation as an outstanding researcher and a superb mentor and clinician encouraged young physicians to come to NIH to work and train in his laboratory. Many are now leaders in the endocrinology and biomedical communities; these include Lew Chase (chief of medical service, Veterans Affairs Medical Center, Washington University, St. Louis), Allen Spiegel (director of the National Institute of Diabetes and Digestive and Kidney Diseases [NIDDK]), Steve Marx (chief of the Metabolic Diseases Branch at NIDDK), Ed Brown (Endocrine/Hypertension Division at Brigham and Women's Hospital in Boston), Maria Luisa Brandi (professor in the Endocrine Section at the University of Florence in Italy), and John Bilezikian (chief of the Endocrine Division at Columbia University Medical Center in New York).

Gerry's contributions were recognized by many awards, some of which included the John Horsely Memorial Award from the University of Virginia in 1960, the Andre Lichiwitz Prize from France in 1968, the William F. Neuman Award of the American Society for Bone and Mineral Research in 1981, the Gairdner Foundation International Award in 1983, the Edwin B. Astwood Award from the Endocrine Society in 1985; and the Distinguished Service Medal of the Public Health Service in 1988.

Perhaps one of the most meaningful awards was the Edwin B. Astwood Award of the Endocrine Society. This was because Gerry began his research career in Dr. Astwood's laboratory and was very active in the Endocrine Society, holding positions on various committees and serving as its president from 1998 to 1999. Since his death, both the Endocrine Society and the American Society for Bone and Mineral Research have established memorial lectures in his honor. The University of Virginia Medical School established the Gerald D. Aurbach Professorship in Endocrinology in 1998 in recognition of his scientific contributions and his service, and in 2002 the university named their new medical research building "The Gerald D. Aurbach Medical Research Building" in his honor.

Gerry was a member of many prestigious medical societies, including the American Society for Clinical Investigation, the American Society for Biochemistry and Molecular Biology, and the Association of American Physicians. He was a founding member of the American Society for Bone and Mineral Research. He served as president of the Endocrine Society. In 1986 he was elected into the National Academy of Sciences.

Gerry was a fourth generation Washingtonian. His mother's family arrived in Washington in 1859. He attended the Lafayette Elementary School and graduated from Wilson High School in 1945. Part of his reason for joining the NIH staff was to return to the city he loved. Gerry was a devoted husband to Hannah, whom he married in 1960, and he was a wonderful father who set a fine example for their daughters, Elissa and Pamela. Yearly family vacations at North Shores in Rehoboth Beach, Delaware, and in Virginia Beach, Virginia, were always looked forward to. Winter trips to Colonial Williamsburg, Virginia, through New Year's Eve were also a favorite, especially since Hannah was from the Tidewater area.

A lover of classical music, Gerry was also an accomplished pianist who enjoyed sitting down at his Steinway Grand (which he had played since age 6) upon returning home from work no matter what the hour. He could often be seen jogging in Great Falls Park close to his home in Potomac, Maryland. He was an avid Redskins fan through both the good and bad times. He and his dad seldom missed attending the Senators baseball games. He was a collector of old maps, and any John Wayne movie was his favorite. He and Hannah visited all new art exhibitions in Washington and found local art museums when traveling; the Smithsonian National Air and Space Museum was Gerry's favorite because of his love of flying. Gerry also enjoyed traveling. Trips to Asia (especially Singapore), Israel, and Europe, often combined with meetings and speaking engagements, were always very special to both Gerry and Hannah.

I met Gerry in Boston when he was a fellow in the laboratory of Ted Astwood and I was a medical student. We came to the National Institutes of Health the same year and were both interested in the mechanism of action of polypeptide hormones. Because we lived in the same general area, we often drove home from work together. He was a wonderful friend and colleague, as well as a gentle, wise, and supportive person loved and respected by his colleagues and family.

SELECTED BIBLIOGRAPHY

1959

Extraction of parathyroid hormone with phenol. *Arch. Biochem. Biophys.* 80:467-468.

Isolation of parathyroid hormone after extraction with phenol. *J. Biol. Chem.* 234:3179-3181.

1962

With W. B. Jakoby. The multiple functions of thiooxidase. *J. Biol. Chem.* 237:565-568.

1963

With S. A. Berson, R. S. Yalow, and J. T. Potts Jr. Immunoassay of bovine and human parathyroid hormone. *Proc. Natl. Acad. Sci. U. S. A.* 49:613-617.

1965

With R. A. Melick and J. T. Potts Jr. Distribution and half-life of 131I-labeled parathyroid hormone (bovine) in the rat. *Endocrinology* 77:198-202.

With J. T. Potts Jr., L. M. Sherwood, and A. Sandoval. Structural basis for biological and immunological activities of parathyroid hormone. *Proc. Natl. Acad. Sci. U. S. A.* 54:1743-1751.

1966

With L. M. Sherwood, A. D. Care, G. P. Mayer, and J. T. Potts Jr. Evaluation by radioimmunoassay of factors controlling the secretion of parathyroid hormone. I. Intravenous infusions of calcium and EDTA in the cow and goat. *Nature* 209:52-55.

With A. D. Care, L. M. Sherwood, and J. T. Potts Jr. Evaluation by radioimmunoassay of factors controlling the secretion of parathyroid hormone. II. Perfusion of the isolated parathyroid glands of the goat and sheep. *Nature* 209:55-57.

With J. L. H. O'Riordan, A. H. Tashjian Jr., P. L. Munson, and P. G. Condliffe. Thyrocalcitonin: Ultracentrifugation in gradients of sucrose. *Science* 154:885-886.

1967

With R. A. Melick, J. R. Gill, S. A. Berson, R. S. Yalow, F. C. Bartter, and J. T. Potts Jr. Antibodies and clinical resistance to parathyroid hormone. *N. Engl. J. Med.* 276:144-147.

With J. T. Potts Jr. Parathyroid hormone. Am. J. Med. 42:1-8.

With L. R. Chase. Parathyroid function and the renal excretion of 3',5'-adenylic acid. *Proc. Natl. Acad. Sci. U. S. A.* 58:518-25.

1968

With L. R. Chase. Renal adenyl cyclase: Anatomical separation of sites sensitive to parathyroid hormone and vasopressin. *Science* 159:545-547.

With L. J. Deftos, A. S. Rabson, R. M. Buckle, and J. T. Potts Jr. Parathyroid hormone production in vitro by human parathyroid cells transformed by Simian virus 40. *Science* 159:435-436.

1969

With R. Marcus. Bioassay of parathyroid hormone in vitro with a stable preparation of adenyl cyclase from rat kidney. *Endocrinology* 85:801-810.

With J. L. H. O'Riordan and J. T. Potts Jr. Immunological reactivity of purified human parathyroid hormone. *Proc. Natl. Acad. Sci. U. S. A.* 63:692-698.

1970

With G. L. Melson and L. R. Chase. Parathyroid hormone-sensitive adenyl cyclase in isolated renal tubules. *Endocrinology* 86:511-518.

1971

With J. T. Potts Jr., G. W. Tregear, H. T. Keutmann, H. D. Niall, R. Sauer, L. J. Deftos, B. F. Dawson, and M. L. Hogan. Synthesis of a biologically active N-terminal tetratriacontapeptide of parathyroid hormone. *Proc. Natl. Acad. Sci. U. S. A.* 68:63-67.

With J. L. H. O'Riordan and J. T. Potts Jr. The isolation of human parathyroid hormone. *Endocrinology* 89:234-239.

With D. Desbuquois. Use of polyethylene glycol to separate free and antibody-bound peptide hormones in radio-immunoassays. *J. Clin. Endocrinol.* 33:732-738.

1972

With D. Powell, P. M. Shimkin, J. L. Doppman, S. Wells, S. J. Marx, A. S. Ketcham, and J. T. Potts Jr. Primary hyperpara-thyroidism: Pre-operative localization and differentiation between adenoma and hyperplasia. *N. Engl. J. Med.* 286:1169-1175.

With S. J. Marx and S. A. Fedak. Preparation and characterization of a hormone-responsive renal plasma membrane fraction. *J. Biol. Chem.* 247:6913-6918.

With S. J. Marx and C. J. Woodard. Calcitonin receptors of kidney and bone. *Science* 178:999-1001.

1973

With J. P. Bilezikian. A beta-adrenergic receptor of the avian turkey erythrocyte. I. Binding of catecholamine and relationship to adenylate cyclase activity. *J. Biol. Chem.* 248:5577-5583.

With S. J. Marx, D. Powell, P. M. Shimkin, and S. A. Wells. Familial hyperparathyroidism. Mild hypercalcemia in at least nine members of a kindred. *Ann. Intern. Med.* 78:371-377.

With J. L. Doppman, S. A. Wells, P. M. Shimkin, K. D. Pearson, J. P. Bilezikian, D. A. Heath, D. Powell, and A. S. Ketcham. Parathyroid localization by angiographic techniques in patients with previous neck surgery. *Brit. J. Radiol.* 46:403-418.

With J. P. Bilezikian, J. L. Doppman, P. M. Shimkin, D. Powell, S. A. Wells, D. A. Heath, A. S. Ketcham, J. Monchik, L. E. Mallette, and J. T. Potts Jr. Preoperative localization of abnormal parathyroid tissues: Cumulative experience with venous sampling and arteriography. *Am. J. Med.* 55:505-514.

With S. J. Marx, D. Powell, P. Shimkin, S. A. Wells, A. S. Ketcham, J. E. McGuigan, and J. P. Bilezikian. Familial hyperparathyroidism. *Ann. Int. Med.* 78:371-377.

1974

With H. D. Niall, R. T. Sauer, J. W. Jacobs, H. T. Keutmann, G. V. Segre, J. L. H. O'Riordan and J. T. Potts Jr. The amino-acid sequence of the amino terminal 37 residues of human parathyroid hormone. *Proc. Natl. Acad. Sci. U. S. A.* 71:384-388.

With A. M. Spiegel. Binding of 5'-guanylyl-imidodiphosphate to turkey erythrocyte membranes and effects on beta-adrenergic-activated adenylate cyclase. *J. Biol. Chem.* 249:7630-7636.

With S. A. Fedak, C. J. Woodard, J. S. Palmer, D. Hauser, and F. Troxler. Beta-adrenergic receptor: Stereospecific interaction of iodinated beta-blocking agent with high affinity site. *Science* 186:1223-1224.

1976

With E. M. Brown, D. Hauser, and F. Troxler. Beta-adrenergic receptor interactions: Characterization of iodohydroxybenzylpindolol as a specific ligand. *J. Biol. Chem.* 251:1232-1238.

1977

With A. M. Spiegel, H. E. Harrison, S. J. Marx, and E. M. Brown. Neonatal primary hyperparathyroidism with autosomal dominant inheritance. *J. Pediatr.* 90:269-272.

With S. J. Marx, A. M. Spiegel, and E. M. Brown. Family studies in patients with primary parathyroid hyperplasia. *Am. J. Med.* 62:698-706.

1980

With A. M. Spiegel, S. T. Eastman, M. F. Attie, R. W. Downs Jr., M. A. Levine, S. J. Marx, J. L. Stock, A. Saxe, and M. F. Brennan. Intraoperative measurements of urinary cyclic AMP to guide surgery for primary hyperparathyroidism. *N. Engl. J. Med.* 303:1457-1460.

With S. J. Marx and A. M. Spiegel. Incidence of primary hyperparathyroidism. *N. Engl. J. Med.* 302:1313.

1982

With S. J. Marx, A. M. Spiegel, M. A. Levine, R. E. Rizzoli, R. D. Lasker, A. C. Santora, and R. W. Downs Jr. Familial hypocalciuric hypercalcemia: The relation to primary parathyroid hyperplasia. *N. Engl. J. Med.* 307:416-426.

With A. M. Spiegel, M. A. Levine, and S. J. Marx. Pseudo-hypoparathyroidism: The molecular basis for hormone resistance—a retrospective. *N. Engl. J. Med.* 307:679-681.

1983

With R. E. Rizzoli, M. Somerman, and T. M. Murray. Binding of radioiodinated parathyroid hormone to cloned bone cells. *Endocrinology* 113:1832-1838.

1986

With M. L. Brandi and H. G. Coon. Bovine parathyroid cells: Cultures maintained for more than 140 population doublings in serum-free medium. *Proc. Natl. Acad. Sci. U. S. A.* 83:1709-1713.

With M. L. Brandi, L. A. Fitzpatrick, R. Quarto, A. M. Spiegel, M. M. Bliziotes, J. A. Norton, J. L. Doppman, and S. J. Marx. Parathyroid mitogenic activity in plasma from patients with familial multiple endocrine neoplasia type I. *N. Engl. J. Med.* 314:1287-1293.

1989

With S. J. Bale, A. E. Bale, K. Stewart, L. Dachowski, O. W. McBride, T. Glaser, J. E. Green III, J. J. Mulvihill, M. L. Brandi, K. Sakaguchi, and S. J. Marx. Linkage analysis of multiple endocrine neoplasia type 1 with INT2 and other markers on chromosome 11. *Genomics* 4:320-322.

With E. Friedman, K. Sakaguchi, A. E. Bale, A. Falchetti, E. Streeten, M. B. Zimering, L. S. Weinstein, W. O. McBride, Y. Naamura, M. L. Brandi, J. A. Norton, A. M. Spiegel, and S. J. Marx. Clonality of parathyroid tumors in familial multiple endocrine neoplasia type I. *N. Engl. J. Med.* 321:213-218.

Photograph by Mount Wilson and Los Campanas Observatories

Horace W. Babcock.

HORACE WELCOME BABCOCK

September 13, 1912–August 29, 2003

BY GEORGE W. PRESTON

HORACE BABCOCK'S CAREER at the Mount Wilson and Palomar (later, Hale) Observatories spanned more than three decades. During the first 18 years, from 1946 to 1964, he pioneered the measurement of magnetic fields in stars more massive than the sun, produced a famously successful model of the 22-year cycle of solar activity, and invented important instruments and techniques that are employed throughout the world to this day. Upon assuming the directorship of the observatories, he devoted his last 14 years to creating one of the world's premier astronomical observatories at Las Campanas in the foothills of the Chilean Andes.

CHILDHOOD AND EDUCATION

Horace Babcock was born in Pasadena, California, the only child of Harold and Mary Babcock. Harold met Horace's mother, Mary Henderson, in Berkeley during his student days at the College of Electrical Engineering, University of California. After brief appointments as a laboratory assistant at the National Bureau of Standards in 1906 and as a physics teacher at the University of California, Berkeley, in 1907, Horace's father was invited by George Ellery Hale in 1908 to join the staff of the Mount Wilson Observatory (MWO), where he remained for the rest of his career. (Harold

Babcock was elected to the National Academy of Sciences in 1933.)

In 1912, when Horace was born, the Mount Wilson Observatory was in its heyday of expansion. The newly completed 60-inch telescope, then the largest in the world, was to be eclipsed within the decade by the 100-inch Hooker telescope under construction nearby.

Thus, Horace Babcock, son of a Mount Wilson astronomer, grew up in the environment of a great observatory in the making. In his oral interview for the American Institute of Physics (AIP) Horace recalls that many of his childhood recollections relate to Mount Wilson, seeing the astronomers, being aware of construction on the mountain, in particular "the noisy riveting of the 100-inch dome. . . So it was only natural that I would have an early strong interest in astronomy." Horace, attracted to science, went to the Pasadena public grade and high schools. For understandable reasons he was also interested in engineering, so much so that he majored in structural engineering at Caltech, and it was only after he graduated in 1934 that he decided to go into astronomy.

Throughout his life Horace was fascinated by fine mechanisms and by electrical and optical instruments. His father cultivated these interests by involving Horace in his own work from childhood. But, Horace remarks in his AIP oral interview, his father was careful never to try to make major decisions for him. Rather he tried to show Horace where opportunities and interests might lie. For example, he introduced Horace to photography and helped Horace build a 6-inch telescope. In 1928 Horace, then 16 years old, spent six weeks as a volunteer in the MWO optical shop, where he learned how to make lenses, mirrors, and prisms. During the summers of 1930, 1932, and 1935 he worked as a volunteer observer with the Snow solar telescope and

150-foot solar tower on Mount Wilson, where he produced spectrograms of the solar chromosphere, especially in the infrared. He published five short papers about these activities in the *Publications of the Astronomical Society of the Pacific* (one with his father). This was a period of learning outside the conventional paths of public education. He was learning practical spectroscopy, measuring wavelengths, and acquiring familiarity with the reference materials of observational astrophysics. In the course of these adventures he inevitably became acquainted with many astronomers at Monastery mealtimes. "The Monastery" was the official name of the (at that time all-male) sleeping and eating facility for observers on the observatory grounds. In the spring of 1930 Horace accompanied an MWO party that included his father, Seth Nicholson, and Ted Dunham to observe a total solar eclipse in Nevada.

In 1930 Horace also began his undergraduate studies at Caltech, a personal goal since childhood. He was well aware that the design and assembly of a 200-inch telescope were under way there. He majored in engineering (as his father did in Berkeley), but also studied physics (electricity and light, which he liked best). There was no undergraduate astronomy course at Caltech at that time. Horace wanted to study astronomy, so he wrote a petition asking that such a course be taught and posted it on a campus bulletin board. Many students signed it, and the next year such a course was offered by physicist John Anderson, head of the 200-inch telescope project under Hale. Horace was pleased to think that his petition had played a role in this development.

By 1934, when he graduated from Caltech, Horace knew that he wanted to be an astronomer. He realized that he would need a Ph.D., unlike his father who had gone to work at MWO with only a B.S., and Walter Adams, the director, who had only an M.S. Horace hoped that when the 200-inch telescope went into operation (then expected to happen around 1938) he might have a chance to participate in its use. Horace had visited Palomar Mountain before anything had been built there, and he was taken by the challenge of locating the 200-inch in that primitive environment.

The graduate school that most closely aligned with Horace's interests was the University of California, meaning course work in Berkeley and thesis work at the Lick Observatory on Mount Hamilton in California. "I didn't have much inclination to think of going to an Eastern university, which would not be strong on observing anyway. The University of California had the Lick Observatory and it was the place to go." Horace did not have a scholarship or assistantship during his three years on the Berkeley campus; his father paid his expenses. Horace's uncle, Ernest B. Babcock was a biology professor at Berkeley (also elected to the National Academy of Sciences, in 1946). It is possible that Horace lived with his family there.

Horace found the Berkeley Astronomy Department to be quite old-fashioned ("post-mature" to use the euphemism in his oral interview). It was dominated by several big names in "theoretical astronomy," a term which at that time in Berkeley meant "orbit theory." Horace had already been exposed to the new astrophysics that Hale had made the centerpiece of research at the Mount Wilson Observatory, but only Donald Shane, subsequently director of Lick Observatory, taught astrophysics at Berkeley in 1935. However, there were good physics courses. Horace enjoyed those offered by Robert Birge, Francis Jenkins, Harvey White, and particularly by J. R. Oppenheimer, whom Babcock regarded as remarkably articulate. Of course, there was the attraction of the Lick Observatory. While in Berkeley, Horace became acquainted with Nicholas Mayall of Lick Observatory, under whom he would later pursue his thesis observational work. Babcock regarded Mayall's extragalactic research with his nebular spectrograph at Lick's Crossley telescope as a "shining example of achievement."

Horace credits Mayall for proposing measurement of the rotation of the galaxy M31 (Andromeda Nebula) as a Ph.D. thesis topic. Mayall also provided a spectrograph at the 36-inch Crossley reflector capable of making the measurements, and he offered suggestions about places to make observations in the outskirts of M31, faint emission wisps. Horace took up these suggestions in his fourth year of graduate work, now supported by a fellowship, on Mount Hamilton. Most of his spectra were obtained with a long slit placed along the major axis of the galaxy. Velocities in the inner regions with sufficient surface brightness were derived from wavelength displacements of absorption lines produced by myriads of unresolved stars. Additionally, he observed five faint nebulosities identified by Mayall in the outer reaches of M31, where starlight is too weak to measure. We now recognize these wisps as H II regions similar to the Orion Nebula located nearby in our Milky Way Galaxy. They could provide velocities because they shine brightly at a relatively few discrete wavelengths due to the fluorescence of gas clouds illuminated by hot stars. The work was arduous. Exposure times were long, 10 to 20 hours (several nights spent obtaining each spectral photograph), but the results were spectacular. They are displayed in his thesis, published as Lick Observatory Bulletin, No. 498 in October 1939 and now reproduced in a more readily available journal *Publications of the Astronomical Society of the Pacific,* volume 116.

The rotation curve produced by this work (a plot of line-of-sight velocity derived from the optical Doppler effect versus angular position along the major axis of the galaxy) did not decline outside the galaxy's luminous bulk. Rather, it continued to rise to the outer angular limits of Horace's observations. This behavior was contrary to the expectations for Keplerian motion about a central gravitating body (in which velocity decreases inversely as the square root of distance from the center of the system). He analyzed the velocities, advised by Lick astronomer R. J. Trumpler, and found that they did not match the rotation curve calculated for the constant mass-to-light ratio, then the usual assumption made for starlight. Upon converting his radial velocities to angular velocities about the center of M31, Horace noted in his thesis that "the obvious interpretation of the nearly constant angular velocity from a radius of 20 minutes of arc outward is that a very great proportion of the mass of the nebula must lie in the outer (dim) regions." In retrospect we now know that Horace had come upon the crucial evidence for the existence of dark matter, but like Wegener's continental drift, it was a discovery before its time. No one could make any sense of it. He completed writing his thesis by June 1938 and got his Ph.D.

W. H. Wright, the Lick director, arranged for Horace to make oral presentations about his M31 rotation curve at the 1939 annual meeting of the American Philosophical Society (APS) in Philadelphia and at the dedication of the brand-new McDonald Observatory in Texas immediately afterwards. His paper fit right into the subject of the APS symposium, "Structure and Dynamics of Galaxies," and he discussed it afterward with Bertil Lindblad and Jan Oort, two of the world experts who gave review papers there. Babcock's was the first published rotation curve that extended significantly beyond the bright nuclear bulge of M31 into the spiral arm regions of the galaxy. Everyone agreed that his results were important, but no one had a good explanation for them. Thus, Horace's graduate education concluded on a satisfactory though puzzling note; at the least he had attracted some attention. Shortly thereafter Otto Struve offered him a position at Yerkes and McDonald Observatories. Horace believed that his presentation at McDonald got him the job.

GETTING STARTED

Before going to Yerkes, Horace enjoyed the summer of 1939 as a postdoc at Palomar working on a project partly financed by 200-inch funds. He used a small spectrograph put together around a fast Schmidt camera and a grating provided by his father, who was in charge of the MWO grating laboratory at that time. He and Josef Johnson, a graduate student working with Caltech astrophysicist Fritz Zwicky, took spectra of the night sky, which they continued through the year. Their system was responsive well into the UV, and it showed many night-sky bands in that region, which we now know are mostly emitted by various excited levels of terrestrial, atmospheric molecular oxygen (O_2). They also tried to trace the night-sky brightness variations through the night and through the year, but it was too complicated to unravel. One of their conclusions: "For the photography of faint nebulae it would seem advantageous to filter out the ultraviolet light." Perhaps because of their UV observations, the Palomar high command decided not to try to include that spectral region in the Palomar Sky Survey to be conducted by the 48-inch Schmidt telescope. Consequently, the Schmidt corrector plate was made of plate glass (to block the UV) rather than from UV transmitting glass. This design feature would increase the limiting magnitude of the blue exposures, an important improvement. Babcock, living at Palomar that summer of 1939, was one of the first astronomers to make an extended stay in the Lodge, as it was then called. He liked it and hoped to get a staff job there, but the 200-inch was being put on hold because of World War II, and was far from complete.

MCDONALD AND YERKES OBSERVATORIES

Otto Struve, director of Yerkes and McDonald Observatories by agreement between the University of Chicago and the University of Texas, was eager to hire young astronomers trained at other top observatories to work at McDonald. Wright's recommendations played a significant role in Struve's choices of Horace and his fellow Berkeley graduate student Daniel Popper, according to Osterbrock (at the 2004 Babcock Memorial Symposium). The positions were attractive, because the new 82-inch reflector was then the second largest telescope in the world. Horace would have preferred a job at Mount Wilson Observatory, but his father and Director Walter S. Adams, a close friend of the family, told him the experience would be useful to him in the long run.

Horace, by virtue of his thesis experience, was very interested in nebular spectroscopy, but even the fastest spectrograph of the 82-inch, used at the Cassegrain, was far too slow. Instead he had to work on low-resolution spectroscopy of relatively nearby bright stars, collaborating with other astronomers on their programs. One was with Popper on nova-like variables; another, with Philip Keenan, was devoted to spectra of stars near the north galactic pole.

Struve asked Horace to design a fast nebular spectrograph to be used at the prime focus, a concept Frank Ross, then a senior Yerkes staff member, had suggested to him. Horace had little experience in designing instruments, but his engineering training at Caltech and his discussions with his father had prepared him admirably for the task. He designed a grating spectrograph (unusual because good gratings were rare); his father, now supervisor of the Mount Wilson grating laboratory, was able to provide one for him.

It was pierced, so it could be used with a parabolic mirror collimator on axis, the favored MWO design at the time.

There was no instrument maker at McDonald, so Horace had to make the drawings and send them back to Williams Bay, where Yerkes machinist Charles Ridell constructed the mechanical parts. George Van Biesbroeck, the astronomer in charge of the shop, modified the plans, probably to simplify the work, before handing them over to Ridell, and neither Van Biesbroeck nor Struve felt called upon to notify Horace of the changes. He learned of them only when the parts arrived at the remote McDonald observing site, where the instrument could not be assembled and used effectively. Horace, who had been counting on using the spectrograph, wrote a hot letter to Struve asking why he had not been consulted, and Struve replied at once, chastising him severely for daring to question the judgment of his elders. In fact both of his elders were quite out of date about spectrographs, as demonstrated by the other instruments at McDonald, but as a result Horace did not manage to do nebular spectroscopy there. In spite of this early confrontation Horace expressed admiration for Struve's research and management style at the Yerkes-McDonald enterprise.

After World War II, Horace's fast B spectrograph was slightly modified by Thornton Page, who used it to measure the velocity differences in pairs of galaxies. The result Page found, that the indicated masses of the individual galaxies were larger than expected, was another manifestation of dark matter, not understood by Page or anyone else at that time. After Page left Chicago, the B spectrograph was used by Margaret and Geoffrey Burbidge to measure rotation curves of numerous other nearly edge-on galaxies. Thus, Horace's instrumental efforts at McDonald enabled important later extensions of his thesis work at Lick.

Soon after completion of the spectrograph in 1941 Struve asked him to take on the coronaviser, a device on loan from Bell Telephone Laboratories that had been invented by A. M. Skellet for observation of the solar corona outside of eclipse. Among improvements made by Babcock was use of an RCA 931, the first astronomical application of this precursor of the famed 1P21 photomultiplier.

Within the year, Horace was rotated from McDonald back to Yerkes for a long stay and a chance to write up his results. There he had many discussions with S. Chandrasekhar, then working in stellar dynamics, about the M31 rotation curve. Horace liked Chandra and enjoyed hearing his talks, but he wrote that the acclaimed theorist didn't understand much about observational astronomy.

While at Yerkes, Horace met, wooed, and married Margaret Anderson, an eighth-grade school teacher at Williams Bay High School. Later that year the two went back to McDonald, but the year was 1941 and America was close to entering World War II. Scientists were in demand for weapons projects, especially experimental physicists with skills in electronics. Albert Whitford had been recruited to the MIT Radiation Lab to work on radar in 1940; he had recruited Gerald Kron to come there, too, and now Kron recruited Horace, who arranged with Struve to take an indefinite leave of absence. He and his wife drove across the country (after a visit to his parents' home in Altadena), with perhaps a stop at the last big prewar American Astronomical Society meeting at Yerkes (in September 1941), and then on to Cambridge.

WORLD WAR II

Horace arrived in Cambridge knowing little more than that the Radiation Laboratory was engaged in electronics. Security was surprisingly tight and he was given to feel that he shouldn't be asking questions. He was attached to a group concerned largely with cathode-ray circuitry to present airborne radar information, all of it mostly new to Horace. He picked up most of his electronics there by osmosis. Horace did not begrudge the time, as most of the astronomers he knew were also engaged in military research. However, he yearned to return to California, and he seized an opportunity to join a new war laboratory at Caltech, where he worked at first on aircraft rocket launchers, on the development and subsequent testing of new types of rockets at Goldstone and China Lake, and on development of automatic sights for firing at surface targets.

Subsequently, in 1945, he and Carl Anderson were dispatched to Los Alamos to discuss development of a delivery system for the atomic bomb and radiotelemetry for testing bomb drops from B-29 airplanes, but it seems that Horace did not actually participate. He comments in his oral interview that he felt the United States should have made a demonstration of the bomb without using it on civilian targets. His thought at the time about drops in Japan: "Why should I get involved? I don't particularly like it if they are going to drop this thing on a city."

SCIENCE YEARS

"After the war was over, the big question was how do we get back into astronomy?" Horace, luckily, was well-positioned. In the Caltech Rocket Project he became reacquainted with Ira S. Bowen, the Caltech physics professor about to assume the directorship of the newly formed Mount Wilson and Palomar Observatories, whom he had met earlier at Lick. They had worked together organizing rocket tests and analyzing results. Bowen, recognizing Babcock's rare combination of astronomical knowledge and optical and electronic skills offered Horace a position at the observatories to become effective January 1, 1946, the first day of Bowen's directorship. Horace accepted and remained at the observatories for the rest of his professional career.

Bowen hired Horace with the understanding that Horace would divide his time equally between instrument development and personal research. Shortly before the outbreak of World War II, Hans Bethe discovered the sequence of nuclear reactions that powers the sun and stars. Mindful of Bethe's discovery Bowen gave high priority to observatories involvement in the derivation of atomic abundances in stars and galaxies as part of a broader inquiry into nuclear reactions and energy generation in stars. Accordingly, he wanted a new microphotometer that could extract abundance information from stellar spectrograms to be produced by the large coudé spectrograph of the 200-inch telescope. As a first assignment Bowen asked Horace to design and build such a microphotometer. His reaction: "OK, this was an assignment and I had to do it." He had no assistant. He did all the electronics himself: "design, purchase of every transformer, every vacuum tube socket." The mechanical parts of the machine were built in the observatory shops. He got it to work in 1948, but Horace regarded it as a failure because it was never used in a systematic way by his colleagues. He admitted that the machine was perhaps too elaborate and thus turned people off. He regarded the effort as educational, but "I have to remark that the time was wasted." Two admirable characteristics of Horace Babcock's professional style emerge from this episode: the avant-garde nature of his designs and his frank assessment of outcomes.

STELLAR MAGNETISM

While completing his microphotometer assignment, Horace also chose his first venture into scientific research at the Mount Wilson and Palomar Observatories. He comments in his oral interview that he sat down on his front porch one evening and said to himself, "I'd better come up with something here. What am I going to do in research?" He mused about what could be learned from the radiation that arrived from celestial sources. In 1946 astronomers were measuring frequency (or wavelength), intensity, and position of radiation from celestial sources, but no one was doing much with polarization. From this reflection his thoughts turned to Hale's discovery of magnetic fields in sunspots (atoms in magnetic fields produce polarized light) and he asked himself under what circumstances might one detect a magnetic field in a distant star. Reasoning by analogy to the sun's general magnetic field, "I got to thinking, suppose you had a star with a far stronger field than the sun . . . thousands of gauss, would there be any chance of detecting it?" By way of justification for such a notion he imagined that strong fields might be generated by some dynamo process in rapidly rotating stars: "I really did have the notion that rapid rotation would somehow result in strong magnetic fields." Accordingly, he estimated the stellar field strengths that might exist if magnetic field strength scaled with rotation. The sun with a general field of 50 gauss (the erroneously high but accepted value at that time) rotates at the speed of about 2 km/s at its equator. Most A-type stars (stars hotter and more luminous than the sun) rotate with speeds of 100 km/s or more at their equators, so fields ~1000 gauss might occur. Horace may or may not have been aware that this same speculation had been published 10 years earlier by the Dutch astronomer M. Minnaert in volume 60 of the British journal *Observatory*.

Could such fields be detected in the integrated light of the star? Horace (1947) made elaborate calculations of the Zeeman effect (the wavelength shift between polarized atomic line components) that would be produced in the integrated light from a star that possessed an embedded dipole field. Under the most favorable circumstances—when the dipole axis is parallel to the line of sight to the observer—a dipole field star with polar field strength of a few kilogauss would produce displacements of a few microns between the oppositely circularly polarized Zeeman components in the focal plane of the most powerful spectrographs available at the Mount Wilson 100-inch reflector. Detection of this miniscule effect was a long shot and Babcock knew it, but he was intrigued. He took the idea to Bowen, who said, "Why don't you give it a try?"

How would he make such measurements? In short order he assembled an analyzer of the type used by Pieter Zeeman himself, suitably modified for use at the large coudé spectrograph of the 100-inch reflector. It consisted of a quarter-wave plate cemented to the front face of a Nicol prism. The quarter-wave plate was oriented so as to direct incoming left and right circularly polarized light into the O (ordinary) and E (extraordinary) beams of the Nicol prism to which it was cemented. Thus, this analyzer separated oppositely circularly polarized components of starlight at the entrance aperture of the spectrograph, producing dual spectra of the star with opposite circular polarizations side by side on a photographic plate in the focal plane of the spectrograph camera. The two stellar spectra were flanked by reference spectra produced by an iron arc located near the spectrograph entrance slit. The iron arc spectra (containing myriads of Fe I emission lines of known wavelengths) established a coordinate system on the photographic plate within which it was possible to measure the small wavelength shifts between the two polarized spectra that signaled the presence of a magnetic field. To complete his analyzer Horace needed a quarter-wave plate, so he turned to his father for help. Harold had learned how to select, split, and test mica plates for similar analyzers used to measure the magnetic fields in sunspots at the Mount Wilson solar towers.

Next, which stars to observe? The spectral lines produced by stars rotating at speeds of 100 to 200 km/s are shallow and hundreds of times wider than the Zeeman shifts Horace expected from his scaling argument. However, Horace was aware that a small fraction (~10 percent) of the A-type stars have sharp lines and, as well, abnormally strong lines of certain chemical elements. He assumed that these were actually rapidly rotating stars with rotation axes pointed toward the earth. He supposed that the special aspect of this group was responsible for their sharp lines and spectral peculiarities. Alignment of the magnetic and momentum axes of such stars would make them naturals for his first observational experiments.

Horace took his analyzer to the Mount Wilson 100-inch telescope and during his first experimental run discovered a believable magnetic field of ~1 kilogauss in the sharp-lined peculiar A-type star 78 Virginis. All of his expectations were fulfilled with unanticipated speed. To recapitulate: Horace was hired in January 1946, ruminated on his front porch in February, made his analyzer in March, observed 78 Virginis at Mount Wilson in April, May, and June (see Babcock, 1947, Table 1), measured his spectra in July and August, and submitted his seminal paper to the *Astrophysical Journal* in September 1946. Thus, in the first nine months of his appointment to the staff of the observatory he had discovered a new astrophysical phenomenon. In doing so he created a new astrophysical discipline, one that took on unanticipated dimensions with the passage of time, as we shall see below.

Following this discovery Horace could get as much telescope time as he wished. He initiated a major search for stellar magnetic fields, principally among ~100 sharp-lined A-stars, by making a series of Zeeman observations for each.

The search was conducted primarily with the coudé spectrographs of the Mount Wilson 100-inch and Palomar 200-inch telescopes over the course of a decade. The characteristic duration of an observation was one hour. His research assistant, Sylvia Burd, measured his spectrograms and calculated magnetic fields from them by use of his prescriptions. Accordingly, she deserves some of the credit for the steady stream of discoveries that followed from their work. Although the field polarity 78 Vir remained positive with the passage of time and didn't change much, Babcock began to find many stars with variable magnetic fields that reversed polarity, HD 125248 being the first (1951). He concluded his survey with publication of a monumental *Catalogue of Magnetic Stars* (1958), which contains more than 1200 measurements of magnetic fields collected for more than a decade, together with radial velocities, notes, and star-by-star commentaries, all of which have proven invaluable to subsequent investigators. His stellar work after 1958 was concentrated on a few stars of particular interest. Periodic magnetic variability has proven to be invariably the rule among the A-type stars, whenever field strengths significantly exceed the errors of measurement, and the periods are invariably the periods of spectrum variation, a phenomenon known since the beginning of the 20th century. In the course of his investigations Horace also discovered the crossover effect, the appearance of peculiar line profiles that occurs in some stars at phases when the magnetic field reverses polarity.

Horace (1949) was well aware that the Oblique Rotator (a model in which magnetic variation is an aspect effect produced by a static magnetic field inclined to the rotation axis of a star) under plausible circumstances could produce the crossover effect, the field variations, and the changes in spectral line strengths. In fact, he may have invented the model, though history is a bit obscure on this point.

It is certainly the model of choice among today's experts, who have created an impressive array of experimental and computational techniques to study stellar magnetic variability. Furthermore, the now greatly augmented statistics of rotation for stars more massive than the sun demonstrate beyond reasonable doubt that the slowly rotating peculiar A-type stars are a species *sui generis*, rather than the "pole on" fraction of a rapidly rotating population. However, so far as I can ascertain Horace never accepted the Oblique Rotator interpretation of his data, arguing as late as 1958 that strong, coherent magnetic fields are a property of rapidly rotating stars, detectable only in that small proportion of such stars that happen to be observed pole-on. I attribute his preference for an explanation couched in hydromagnetic oscillations to his intimate familiarity with the complex details of solar magnetism (more of this below) and to the line of thought that led to his first successful detection in 1946.

History will record that Horace's discovery and empirical exploration of magnetic phenomena in hot main-sequence stars was his first great contribution to astrophysics. His pioneering efforts have inspired a host of subsequent searches for magnetic fields elsewhere in the Hertzsprung-Russell diagram: in cool solar-type stars, degenerate stars, interacting binaries, and pre-main sequence stars. And the oblique rotator, which explained his stellar observations, has become the paradigm for modeling of pulsars and magnetars.

SOLAR PHYSICS

Three scientists working independently at the Mount Wilson Observatory in the 1950s and 1960s created the underpinnings of what has become known as the solar-stellar connection, namely, the application of what has been learned from detailed investigation of activity on the solar disk to phenomena seen in the integrated light of remote, unresolved stars. In 1961 Horace incorporated the rich lore of solar activity, much of it accumulated at the Mount Wilson solar towers, into an extraordinary phenomenological model of the 22-year solar cycle. A year later, in 1962, Caltech physicist Robert Leighton and his colleagues, Robert Noyes and George Simon, working at the 60-foot Mount Wilson solar telescope reported discovery of the five-minute solar oscillations that have led to modern-day solar and stellar seismology. In a subsequent analytic treatment of Babcock's model in 1969 Leighton quantified Babcock's results, producing what is now known as the Babcock-Leighton dynamo that powers the solar cycle; nowadays, models based on this dynamo are called Babcock-Leighton models. Approaching the solar cycle independently on the basis of his own pioneering work on stellar chromospheres, Olin Wilson undertook a successful decade-long search at the Mount Wilson 100-inch telescope in the 1970s for stellar analogs of the solar cycle. Such cycles abound among solar-type stars. Thus, Horace's efforts were part of a major thrust in solar physics at Mount Wilson that began with George Ellery Hale's discovery of magnetic fields in sunspots at the 60-foot solar tower a half century earlier.

Horace's contributions to these seminal developments were several-fold: (1) working with his father, then retired, he developed a magnetograph at Hale's Solar Laboratory in Pasadena in 1952. This was a marvelous device that could scan the solar disk on a timescale of ~1 hour to produce a map of the surface magnetic field of the sun; (2) with this device he and his father detected the weak (~1 gauss) general high-latitude poloidal field of the sun in 1955; and (3) Harold Babcock used Horace's magnetograph to discover a reversal of this poloidal field during 1957-1958, which Horace incorporated into his model of the 22-year solar cycle.

Perhaps the most important aspect of Babcock's modeling was the ingenious way he used the complex of prior observational data to guide construction of his model, so that it would be at once reasonably compatible with the physics of magnetized plasma and, as well, with what was known about the solar surface at that time: the behavior of the high-latitude poloidal field (his discovery) including its polarity reversal (his father's discovery); the generation of sunspots, particularly bipolar magnetic regions; the polarities; polarity reversals every 11 years; and orientations of these regions, their numbers, integrated magnetic flux, and latitude drift during the cycle, inter alia. Horace's model was a semi-empirical tour de force.

In 1957 Robert Howard, using an improved version of the magnetograph at the 150-foot solar tower on Mount Wilson, began a series of daily observations of the sun, which continues to this day, now under the direction of Roger Ulrich at the University of California, Los Angeles. Because of their value for predicting flares and magnetic storms, these observations are reported in *Solar Geophysical Data* published by the National Oceanic and Atmospheric Administration. Today advanced technologies for the measurement of solar magnetic fields have proliferated at Big Bear Observatory in California; at the National Solar Observatory; at a complex of European facilities at Observatorio del Teide in the Canary Islands; and at solar observatories in Crimea, Japan, Russia, Ukraine, and elsewhere. Horace Babcock really started something.

INVENTION AND INSTRUMENTS

In the midst of his scientific research Horace found time to design, build, improve, and propose a remarkable array of scientific instruments for diverse purposes. Earliest examples, his spectrographs at Palomar and McDonald and the coronaviser, were undertaken as assignments. However, the stellar Zeeman analyzer and the solar magnetograph were initiatives undertaken to further his own research interests. He evidently regarded the magnetograph as a work in progress, for he continued to improve the version he installed at the 150-foot solar tower off and on until 1961, nine years after his first effort in Pasadena.

Inspection of his bibliography reveals a total of 20 papers devoted to various electro-optical mechanical devices. The timing of some of these papers relative to other papers on totally unrelated topics is remarkable. Thus, his seminal 1947 paper about the detection of a stellar magnetic field was followed within a year by a paper on the design and construction of an autoguider to increase the efficiency of observing at the coudé focus of a telescope, and shortly after that a theoretical inquiry into magnetic intensification of spectral lines. His very important 1951 paper on the periodic reversing magnetic field and associated crossover effect of HD 125248 is accompanied by a report in the same year on the performance of the Mount Wilson ruling engine, which Horace supervised, and the quality of diffraction gratings produced by it.

In 1953, while preparing a paper about the magnetograph and its use, he also published a seminal paper entitled *The Possibility of Compensating Astronomical Seeing*, a description of procedures by which it might be possible to produce diffraction-limited images of celestial sources at ground-based telescopes. By publication of this one paper Horace Babcock created adaptive optics, a new discipline that enables high-resolution imaging science at several major observatories and, more importantly, is playing a central role in the design of all very large optical telescopes of the future. It is likely that this contribution will prove as important for astronomy as any other of his works. In 1963 papers on a periodic magnetic variable star (53 Camelopardalis) and a theoretical inquiry into the possibility of element segregation in a magnetized stellar atmosphere accompanied the description of his astronomical seeing monitors (ASMs) to be used for astronomical site testing. These coeval forays into diverse topics testify to the breadth of Horace's intellectual curiosity and inventive skills.

Horace's supervision of the grating laboratory deserves a paragraph of its own. The impetus for the grating laboratory, like many other things at the Carnegie Observatories, came from George Ellery Hale, whose goal was to get the best possible high-resolution spectra of the stars and sun. Typically, Hale imported physicist John Anderson from Johns Hopkins University in 1912 to make very large gratings for the Mount Wilson telescopes. In 1929 after Anderson had taken charge of the 200-inch project, Harold Babcock succeeded him in the MWO grating laboratory, replacing Anderson's original ruling engine with a more compact machine. Over the years the ruling engine was torn down, modified, and improved continuously, and after about 1935 or 1940 most of the new Mount Wilson spectrographs used gratings ruled with it. The 200-inch coudé spectrograph needed a mosaic of four essentially identical gratings to fill its 12-inch beam, a demanding requirement, and Horace took on the task of making them when his father retired. Horace designed and installed the first interferometric control for the ruling process. By the 1960s the demand for gratings was so large that companies, especially Bausch and Lomb, got into the act seriously, and the grating lab was closed, but in its day it produced perhaps 100 gratings for the Mount Wilson and Palomar Observatories and other observatories around the world. After he retired, Babcock published an engaging account of the history, practices, and accomplishments of the grating laboratory (1986).

THE BIRTH OF LAS CAMPANAS OBSERVATORY

The creation of Las Campanas Observatory was the culmination of Horace Babcock's career. The sequence of events that led to completion of the observatory is complex, now blurred by lack of documentation in some instances and by conflicting memories in others. However, many relevant facts are beyond dispute. First, after 1950 Horace Babcock's role in the affairs of the observatories was on the rise. Director Ira Bowen drew him into observatory administration for reasons about which I can only speculate: his scientific reputation, his skill in instrumentation, and his interest in improvement of Carnegie astronomy. Horace became a member of the Observatory Committee in 1953, was appointed assistant director in 1956, and associate director in 1963, the last of these appointments occurring just one year before Bowen's mandatory retirement at age 65. Horace Babcock became director of the Mount Wilson and Palomar Observatories on July 1, 1964. This path from science into administration strongly parallels that of a predecessor, Walter Adams.

In his oral interview Horace cites the opportunity to develop an important Southern Hemisphere observatory as a major inducement to accept the appointment. On reading his first annual report it is evident that he embraced the opportunity: Horace devotes the entire "Introduction" (*Carnegie Year Book*, 1963) to an outline, published in *Carnegie Year Book* 1 in 1902, of requirements prepared by a committee of astronomers for choosing the site of a Carnegie southern observatory and he goes on to summarize a 165-page proposal for this observatory set forth in *Carnegie Year Book* 2 in 1903. Additionally, a major section of his report was devoted to a discussion of ASMs and the initiation of site testing in New Zealand, Australia, and Chile. In these writings Horace announced that he had a historical institutional mandate to pursue a southern observatory.

The salient events that led to the establishment of this observatory at Cerro Las Campanas are set forth below in the (edited) words of Arthur Vaughan, former observatory staff member who played several essential roles in the story. Arthur assisted Ira Bowen in optical designs for the special wide-field characteristics of the telescopes. He served as astronomer in charge of testing and acceptance of the du Pont telescope mirrors and for two years beginning in 1976 as assistant director for Las Campanas. Vaughan delivered his remarks at the Babcock Memorial Symposium held on the Caltech campus on May 21, 2004.

Vaughan recalls the first meeting of Horace's directorship, held in the reading room of the library at Santa Barbara Street. Horace, standing in the middle of the room, spoke with surprising restraint, outlining his vision for the future under his directorship: the construction of a big new observatory in the Southern Hemisphere, with possibly a Southern Hemisphere 200-inch telescope as well as a new 60-inch telescope that would be located at Palomar. He couched his arguments in terms of institutional goals built upon the opportunities afforded by the southern sky, in particular, the center of the Milky Way Galaxy and the Magellanic Clouds, our nearest extragalactic neighbors. Asked by Spencer Weart how he felt about the prospect of such undertakings, Horace replied,

> Well, it made me apprehensive, of course. I was torn between two points of view. In some respects, I looked with great disinterest at the idea of being director. I knew that it would mean giving up all my own science, and getting into this kind of activity that Bowen had been doing, which really in itself didn't have any appeal. . . Let's just say that by 1963, when I was offered the directorship here, it was clear that at the very least, we were going in for an

important site survey in the Southern Hemisphere, with the idea that we might be building a major telescope. This presented me with a tremendously attractive opportunity . . . the prospect of building a new observatory, with a major new telescope, was such an attractive and challenging idea that I couldn't turn it down . . . And, wisely or not, that's what I got into.

So where did this idea of a southern observatory come from? As noted above, Horace attributed the origins of the project to a 1903 proposal of George Ellery Hale. Then, 58 years later, in 1961, the Carnegie trustees initiated a review of the institution's activities and programs, inviting suggestions from directors and staff members as to possible new directions. Olin Wilson wrote a letter in March 1963 to Carnegie physicist Merle A. Tuve, later communicated to President Haskins and the trustees, arguing that the institution should build a major observatory in the Southern Hemisphere, and stressing the antiquated condition of the institution's Mount Wilson Observatory. Maybe that letter was influential. For whatever reasons, in May 1963 the trustees approved funds for undertaking a Southern Hemisphere site survey.

Horace credited the trustees with initiating the plan, but Allan Sandage remembers a different scenario. Allan recalls being dragged by Horace into Bowen's office as early as 1961. On these occasions Horace would plead the case for constructing a Southern Hemisphere observatory. Allan's part would be to argue the scientific merits of the case. Early on, Bowen declined to support such an idea, saying that he already had enough to do in running Palomar. Sometime around 1962, as Allan recalls, Bowen changed his mind, and authorized Horace to initiate the development of instrumentation that Horace felt would be needed in conducting a site survey.

Considering that Horace had already begun tests of his seeing monitors in 1962, it is difficult to avoid the conclusion that, as Sandage recalls, Babcock had a much larger and earlier influence on the inception of the Carnegie southern observatory project than he ever claimed, or is generally credited for. In any case, the idea suited Horace's fancy and, as it turned out, his fearsome tenacity was a key factor in bringing the project to a successful completion.

Four ASMs were built, and by the end of December 1963 Horace had taken two of them to Chile to establish surveys on various peaks. A third ASM went to New Zealand and a fourth went to Australia.

A lot happened in the 14 months that followed Horace's appointment as director designate, even before he actually assumed the directorship. There was no time to waste, because two other organizations were also moving to establish major observatories in Chile: ESO (the European Southern Observatory, a consortium that included Holland, Belgium, Germany, Italy, and France) and the U.S. academic consortium known as AURA (Associated Universities for Research in Astronomy). By 1963 AURA had purchased a 180-square-mile tract of land about 50 miles to the east of the city of La Serena and was engaged in developing the Cerro Tololo Inter-American Observatory (CTIO).

The AURA land includes several major peaks, the largest being Cerro Tololo, Cerro Morado, Cerro Pachon, and Cerro Cinchado. AURA's planning initially called for development of only Cerro Tololo, leaving the remaining peaks—especially Morado—open for possible future use by other organizations. Carnegie became one of those organizations considering the use of Morado for the proposed 200-inch telescope. The Europeans had purchased a similarly large tract of land at Cerro La Silla, about 100 miles north of Cerro Tololo, some 20 miles south of the location Horace later chose for Carnegie's observatory (Las Campanas). Horace was in contact with all of these organizations, especially AURA. He loved the outdoors and in the course of time made a close study of the entire Norte Chico region of Chile, where these sites are located.

In February 1964 Horace and Edward Ackerman (Carnegie's executive officer and one of Horace's closest allies in the founding of Las Campanas) trekked on horseback to the summit of Cerro Pachon near Cerro Tololo. They left behind a small team to make continuing seeing observations on Tololo and Pachon. Then in April 1964 Horace sailed to New Zealand and Australia to initiate site surveys there as well. Horace and his colleagues soon concluded that the best sites of all were those to be found in Chile.

By August 1963 an agreement had been signed between the Carnegie Institution and the University of Chile for conducting a site survey in Chile. By November 1963 a proposal had been written for the construction of a Carnegie 200-inch telescope for the Southern Hemisphere. This was soon accompanied by plans worked out by Horace and his engineer, Bruce Rule, for the development of Cerro Morado as a Carnegie observatory site, with detailed plans for roads, housing, water systems, electric power, and so on.

The quest for funding to build a Carnegie 200-inch telescope in Chile is a story unto itself. The quest occupied Horace and his closest associates, especially Ackerman, for at least three years beginning in 1963. An agreement between Carnegie and AURA for development of a Carnegie observatory on Cerro Morado was contingent upon Carnegie receiving a firm commitment for such funding within 24 months of the date of the agreement (that is, by March 9, 1968). Otherwise the agreement would lapse. Horace and his staff and the Carnegie Institution's officers had high hopes that the Ford Foundation would provide the necessary funds to build Carnegie's Southern Hemisphere 200-inch telescope, but by the end of 1966, it had become clear that Ford Foundation funding for a Carnegie southern 200-inch telescope would not be forthcoming soon, if ever. Horace was not deterred.

In 1966 while all this was playing out, Bowen received the prestigious George Darwin Award of the Royal Astronomical Society, and in October delivered his George Darwin lecture titled *Future Tools of the Astronomer*, in which he opined that survey telescopes with large fields of view would be important for the future of observational astronomy. At Horace's urging Bowen began to think about a survey telescope for the Carnegie Southern Observatory. His ideas later evolved into the 40-inch and 100-inch telescopes that were actually built.

A pivotal meeting of the Carnegie staff was held at Carnegie's Santa Barbara Street offices on June 7, 1966. The attendees were Robert Kraft, Olin Wilson, Armin Deutsch, Allan Sandage, Henrietta Swope, and Arthur Vaughan, with Horace chairing the meeting. Horace explained to the staff that Carnegie trustees had earmarked $2 million for a joint Carnegie-Caltech astronomy building on the Caltech campus, but that if a decision could be reached to proceed with a southern observatory, the institution might have a serious problem in providing its share of the funding. Horace polled the staff and reported to Ackerman: "The Observatory staff is firmly of the opinion that, if necessary, construction of a new headquarters building in Pasadena should be postponed in order to assure funding of the CARSO (Carnegie Southern Observatory) project." Furthermore, "If construction of a 200-inch proves impossible in the near future, we should … make an early beginning with the largest and best instrument that can be built with available funds… Bowen's design for a wide-field 85-inch telescope should be explored." This expression of priorities by the Carnegie staff dealt a mortal blow to plans for a new astronomy building at Caltech, and was received with dismay by campus astronomers.

The negotiations with officials of AURA for a Carnegie observatory site at Cerro Morado dragged on for some two years after funding for a Carnegie 200-inch had failed to materialize. AURA's position was that the Morado site would remain under AURA ownership but leased to Carnegie, and the lease would be limited to an area of some 92 acres, far too small to accommodate the array of large telescopes Horace envisioned. Horace washed his hands of that option.

Meanwhile the site survey work continued. Babcock and John Irwin, who ran the site testing program in Chile, summarized their conclusions in an unpublished memorandum:

> In 1968 it became clear that Las Campanas came closer than any other site in meeting the prescribed CARSO criteria: 29 degree S latitude; 7500-8300 feet elevation, with ample space for many telescopes; only 40 miles from the coast and well-separated from the cordillera; good topography, with no mountains to windward; no prospect of future light pollution; easy road construction; ready availability for purchase; and, as confirmed by test wells, adequate water sources.

In September 1968 Horace wrote to Ackerman that in view of the lack of substantial progress in his attempts to deal with AURA, he [Babcock] proposed informally that Carnegie should promptly review the possibility of locating on Las Campanas instead of Morado. This proposal was approved, and Morado was abandoned.

On November 19, 1968, Horace met with Eduardo Frei, the President of Chile, in Santiago and received approval to purchase Las Campanas. President Frei said that he was strongly interested in the project and that it had his cordial support. . . He inquired whether Horace was having any particular difficulties in our negotiations and, following some discussion, Frei telephoned the minister of land requesting that he give Horace all possible assistance. At meeting's end President Frei assured Horace, "The land is yours." It is a big piece of land (50,000 acres, 84 square miles at purchased price for about 30 cents an acre. Not all of that area is suitable for supporting telescopes, but choice sites for additional telescopes lie along a long ridge extending from Cerro Las Campanas northward past Cerro Manqui, site of the Magellan Telescopes, to Cerro Manquis, where the du Pont Telescope is located. The surrounding land area affords a generous buffer against future sources of interference.

Shirley Cohen's interview of Caltech's James Westphal (at website http://oralhistories.library.caltech.edu/107/01/OHO_Westphal_J.pdf, particularly pp. 71-74) contains a fascinating alternative account of some events that preceded purchase of the Las Campanas property. Horace had enlisted the assistance of Westphal, whom I can best describe as Caltech's 20th-century Renaissance man because of his broad laboratory and field experience, and his facility in matters of engineering, electronics, and astronomy.

With the purchase of the Las Campanas property, Horace had the land but not the funds to develop it or to build telescopes. Among Horace's papers Vaughan found a handwritten note dated April 13, 1970: "Dr. Haskins telephoned me today to say that . . . the (du Pont) family is seriously considering closing out one of its foundations. . . Dr. Greenewalt would like to see the assets go to the Southern Observatory." Crawford Greenewalt was at that time president of E. I. du Pont de Nemours and Co., and his wife, Margarita, was the daughter of the late company president and chairman Irénée du Pont. The upshot of all this was that Horace Babcock and Allan Sandage met with Mrs. Greenewalt at Carnegie headquarters in Washington to answer questions she posed. The conversation must have been productive, because the Greenewalts donated $1.5 million toward the construction of "a 60-inch or larger telescope, the balance of its cost to be provided by the Carnegie Institution." In December 1970 the trustees authorized the construction at Las Campanas of a 100-inch telescope, to be named after Mrs. Greenewalt's father. After seven years of traveling, testing, and talking, Horace aided by only a few close associates had finally set Las Campanas Observatory on its course.

Soon thereafter, Horace created the Las Campanas Observatory Committee, which included Bruce Rule as chief engineer, Ed Dennison in charge of electronics, J. B. Oke (Caltech) responsible for auxiliary instruments, Art Vaughan responsible for optics, and Bruce Adkison responsible for administration in Chile, with Ira Bowen serving as consultant. Horace chaired the committee. The first meeting was held on January 20, 1971. Horace's notes documenting the 36 or so Las Campanas Observatory Committee meetings held over the next five years (through January 1976) provide a detailed record of the course of the project.

Site development at Las Campanas proceeded under increasingly difficult conditions. Chile in the 1970s was wracked by severe inflation, political tensions, strikes, and other disruptions, including the assassination of Salvador Allende in Santiago in 1973. Schedule delays and cost overruns brought Horace into conflict with increasingly grumpy officials of the Carnegie Institution of Washington.

Through all of these vicissitudes Horace endeavored to keep the scientists on his staff informed about the status of the Las Campanas Project, while protecting their freedom to remain focused on research. For the most part the scientists paid little attention to the project, until shortly before the du Pont telescope was to be dedicated in late 1976, when the impact of having to staff and operate a new observatory could not be ignored.

The Swope 1.0-meter and du Pont 2.5-meter telescopes built in the 1970s were only the first steps in the development of the major observatory Horace Babcock had envisioned when he became director in 1964. Thanks to the continuing efforts of succeeding generations of Carnegie astronomers following in Horace's footsteps, Las Campanas is now the site of the two superbly engineered 6.5-meter Magellan telescopes, operated by a consortium consisting of Carnegie Institution of Washington, University of Arizona, Harvard University, Massachusetts Institute of Technology, and University of Michigan. And with installation of the Polish 1.2-meter OGLE (Optical Gravitational Lensing Experiment) telescope and the University of Birmingham (U.K.) automated solar oscillation telescope, the Las Campanas operation has taken on an international flavor.

Horace enjoyed the opportunities that came his way for interacting with folks at the working level in the Las Campanas Project. He spoke their language. They appreciated his encouragement. The Las Campanas Observatory that grew out of Horace's vision represents a supreme asset in the hands of the astronomers who use it today. Its value lies not only in the quality of its dark skies and exquisite seeing but also in its infrastructure, including roads, water resources, and geographical expanse suitable for accommodating the largest telescopes currently foreseen. This asset is Horace's legacy, for which he deserves lasting recognition and thanks.

Horace's efforts at Las Campanas perturbed relations among astronomers in Pasadena in several ways. First, the decision to create Las Campanas Observatory was viewed with dismay by some Caltech astronomers, most notably by Jesse Greenstein, who worried that the new facility would put Carnegie and Caltech into a competition for foundation money in which both institutions would lose. Jesse would have preferred to see the Carnegie Institution invest its astronomical resources at Palomar Observatory, so that it could better compete with well-funded national (AURA) and international (ESO) facilities. Furthermore, the relatively modest aperture of the 100-inch du Pont telescope did little to redress the imbalance between Carnegie and Caltech facilities. Beyond that, I believe there was a general perception among Caltech astronomers in the 1970s that Babcock was primarily a Carnegie director, who devoted far too much of his effort to Las Campanas, and virtually none to fund raising and scientific administration for the improvement of Palomar. Such feelings began to erode the collegial foundations of the joint operation of Hale Observatories. Worries about this erosion, in turn, created concern among some Carnegie astronomers, who feared that collapse of the Hale Observatories would endanger their access to the 200-inch telescope. Such issues may have bothered Horace as well, but they did not weaken his resolve to complete Las Campanas Observatory. Thirty years later these concerns are largely forgotten, indeed unknown to the present generation of astronomers, but they seemed very important in 1975, and they should be acknowledged in considering the impact of Horace Babcock's drive to create Las Campanas Observatory.

CODA

Horace Babcock seldom looked back. He labored long and hard to establish superb empirical foundations of subjects that he had mastered—stellar and solar magnetism—but he never again published a refereed paper on these topics after accepting the observatories directorship in 1964. He worked tirelessly to initiate and oversee completion of Las Campanas Observatory: the infrastructure (access roads, water supply, electrical system, lodge) and the telescopes (1.0-meter Swope and 2.5-meter du Pont), but he never set foot on the mountain after his retirement in 1978. He devoted his postretirement years almost exclusively to topics in experimental instrumentation: optical gyroscopes, adaptive optics, a pneumatic telescope. Such devices were perhaps his first love, and he returned to them.

Horace won worldwide acclaim for his contributions to astronomy. Following his election to the National Academy of Sciences in 1954, he was the recipient of the Henry Draper Medal of the Academy in 1957, the Eddington Medal of the Royal Astronomical Society in 1958, the Catherine Wolfe Bruce Medal of the Astronomical Society of the Pacific in 1969, the Gold Medal of the Royal Astronomical Society in 1970, and the George Ellery Hale Medal of the Solar Physics Division of the American Astronomical Society in 1992. In 2004 Symposium No. 224 of the International Astronomical Union, titled “The A Star Puzzle,” convened in Poprad, Slovakia. A session held on the first evening of the symposium was devoted to memorial presentations about Vera Khoklova, a Russian astrophysicist who died in 2003, and about Horace Babcock.

Horace was a reserved man who seemed to measure his words on most occasions. He was ill at ease in public situations. He was steadfast, even obdurate, in the execution of his plans for Las Campanas. On the lighter side, Horace enjoyed the sea, and from time to time he relaxed on a 26-foot sailboat that he kept in a slip at Redondo Beach, California. On more than one occasion he invited Pasadena astronomers to accompany him on weekend excursions to the Channel Islands. As one might expect, his boat was equipped with an autopilot of his own design. And, of course, the autopilot took its directions from the earth’s magnetic field.

Following retirement Horace continued to work quietly in his office at Santa Barbara Street until 1998, when he moved to a retirement community in Santa Barbara to be near a son. He died 15 days short of his 91st birthday in 2003 and was buried in the family plot at Mountain View Cemetery in Pasadena, following a simple graveside gathering of family and friends, at which his children in turn reminisced about their father. Horace is survived by his children: Ann L. and Bruce H. by his first marriage, and Kenneth L. by a second marriage, to Elizabeth Aubrey (divorced).

IN PREPARING THIS MEMOIR I borrowed from presentations of Donald Osterbrock and Arthur Vaughan delivered at the Babcock Memorial Symposium held at Caltech on May 21, 2004. I also referred extensively to the American Institute of Physics oral interview of Horace conducted by Spencer Weart on July 25, 1977. Some of my remarks are based on personal recollections.

SELECTED BIBLIOGRAPHY

1939

The rotation of the Andromeda Nebula (Ph. D. thesis). Lick Obs. Bull. No. 498.

1947

Zeeman effect in stellar spectra. *Astrophys. J.* 105:105-119.

1948

A photoelectric guider for astronomical telescopes. *Astrophys. J.* 107:73-77.

1949

Magnetic intensification of absorption lines. *Astrophys. J.* 110:126-142.

Stellar magnetic fields and rotation. *Observatory* 69:191-192.

1951

The magnetically variable star HD 125248. *Astrophys. J.* 114:1-35.

1952

With H. D. Babcock. Mapping the magnetic fields of the sun. *Publ. Astron. Soc. Pac.* 64:282-287.

1953

The possibility of compensating astronomical seeing. *Publ. Astron. Soc. Pac.* 65:229-236.

1955

With H. D. Babcock. The sun's magnetic field, 1952-1954. *Astrophys. J.* 121:349-366.

1958

A catalog of magnetic stars. *Astrophys. J.* 3(suppl.):141-210.

Magnetic fields of the A-type stars. *Astrophys. J.* 128:228-258.

1961

The topology of the sun's magnetic field and the 22-year cycle. *Astrophys. J.* 133:572-586.

1963

Instrumental recording of astronomical seeing. *Publl. Astron. Soc. Pac.* 75:1-8.

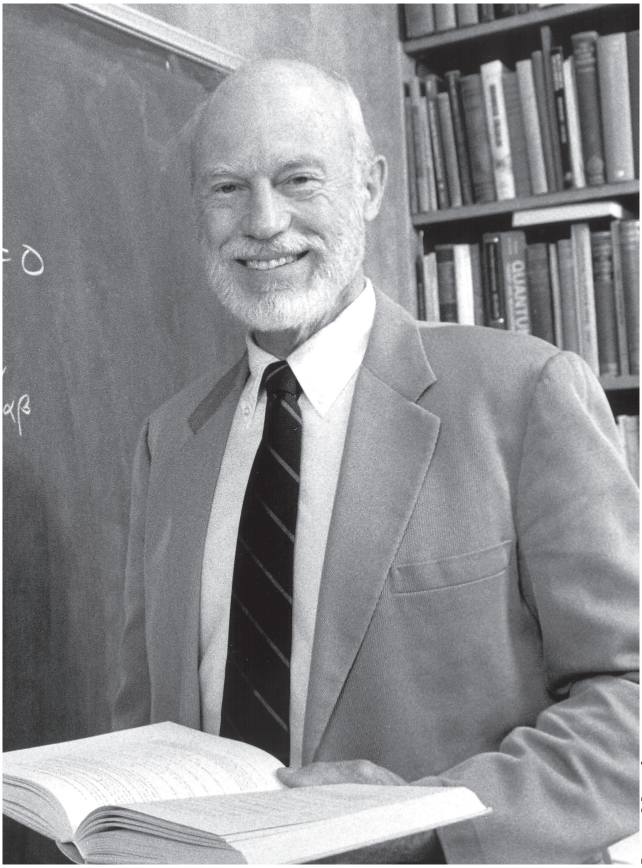
The sun's magnetic field. *Annu. Rev. Astron. Astrophys.* 1:41-58.

1977

First tests of the Iréneé du Pont telescope. *Sky Telescope* 54:90.

1986

Diffraction gratings at the Mount Wilson Observatory. *Vistas Astron.* 29:153-174.



Larry Murphy

BRYCE SELIGMAN DEWITT

January 8, 1923–September 23, 2004

BY STEVEN WEINBERG

BRYCE SELIGMAN DEWITT, PROFESSOR EMERITUS IN THE physics department of the University of Texas at Austin, died on September 23, 2004. His career was marked by major contributions to classical and quantum field theories, in particular, to the theory of gravitation.

DeWitt was born Carl Bryce Seligman on January 8, 1923, in Dinuba, California, the eldest of four boys. His paternal grandfather, Emil Seligman, left Germany around 1875 at the age of 17 and emigrated to California, where he and his brother established a general store in Traver. Emil married Anna Frey, a young woman who had emigrated from Switzerland at about the same time. They had 11 children, whom Anna raised in the Methodist church.

In 1921 DeWitt's father, who had become a country doctor, married the local high school teacher of Latin and mathematics. Her ancestors were French Huguenots and Scottish Presbyterians. DeWitt was raised in the Presbyterian Church, and the only Jewish elements in his early life were the matzos that his grandfather bought around the time of Passover. DeWitt described his early exposure to religion as a boy in California in a moving memoir, "God's Rays," published posthumously in *Physics Today*. His grandmother told him that Armageddon would come in the summer of

1997 and hounded his grandfather to his deathbed, trying to make him give up his belief in Darwinian evolution. Looking back in his memoir, DeWitt came to the conclusion that it was love that gave Christianity its overwhelming impact, but that love "needs no religious framework whatever to exert its power."

DeWitt's mother chafed at her rural surroundings and determined that her sons would live elsewhere. At the age of 12 DeWitt entered Middlesex School in Concord, Massachusetts. The headmaster at Middlesex had initiated a national scholarship program similar to the one at Harvard, and DeWitt had taken (at his mother's insistence) the competitive examination in which he earned his admission.

He graduated from Middlesex at the age of 16, and was admitted to Harvard and Caltech. He chose Harvard because he had become passionate about rowing while at Middlesex, and Harvard had "crew." He eventually stroked the Harvard Varsity. As a physics major he was deferred from military service but always felt guilty about it. Upon graduation in 1943 he went to work on the Calutron at Berkeley, the accelerator used in the Manhattan Project for the final separation of uranium-235 from uranium-238. (This had been recommended to him by Robert Oppenheimer when, because DeWitt wanted to get back to California, he had turned down Oppenheimer's invitation to join a secret research project in an undisclosed location.) He spent seven months at the Berkeley branch of the Manhattan Project and then asked to be released. He reasoned that any bright youngster could do what he was doing (hand soldering, reading meters, general gofer work), and he didn't see that his physics degree was relevant. In January 1944 he enlisted in the navy and became a naval aviator, but World War II ended before he saw combat.

DeWitt came back to Harvard in January 1946. In 1947 he began his thesis work under the nominal supervision of Julian Schwinger. The topic he chose, the quantization of the gravitational field, became his life's work. In 1949 he began his first postdoctoral year at the Institute for Advanced Study. When Wolfgang Pauli, in November of that year, learned what he was working on, he remained silent for several seconds, alternately nodding and shaking his head (a well-known Pauli trademark), and then said, "That is a very important problem. But it will take somebody really smart!"

In 1950 two major but totally unrelated developments occurred in his life. First, he became engaged to be married to Cecile Morette, a young French physicist who was in her second postdoctoral year at the institute. Second, at the urging of their father, he and his three brothers began the legal procedures for changing their surname to a name from their mother's side of the family. The younger boys were, or had been, at school in the eastern United States, and all had encountered repeated misunderstandings and false assumptions based solely on their surname, something that had seldom occurred in California.

From June to December 1950 DeWitt was with Pauli at the ETH in Zürich, and afterward he went to Bombay to spend a postdoctoral year at the Tata Institute of Fundamental Research. This sojourn did not make good professional sense, but it suited his roving spirit. Unfortunately it ended in an abrupt and serious illness, which forced his return to Europe. In May 1951 he and Cecile were married in Paris, and in July they were in Les Houches, where the famous l'École d'Été de Physique Théorique was starting its first year. This school had been created by Cecile as a penance for marrying a foreigner, but she also saw it as something potentially valuable in its own right. It was certainly valuable to DeWitt, who during the summers he was there, was exposed to a very broad range of topics in theoretical physics. That the school was also valuable to others is attested by the fact that at its jubilee in 2001 it numbered among its past students and lecturers 26 who later became Nobel laureates and two who became Fields medalists.

In September 1951 DeWitt, this time accompanied by Cecile, returned to the Tata Institute, determined to complete his postdoctoral year. Their eldest daughter was born in Bombay in April 1952. Three other daughters were born during the following decade. In the summer of 1952 Cecile was back at Les Houches while DeWitt was looking for a job in the United States. His years abroad had kept him out of the market for academic appointments, so he accepted a job at the nuclear weapons laboratory at Livermore, where he remained for three and a half years. During his stay at Livermore, in addition to writing a treatise on "The Operator Formalism in Quantum Perturbation Theory," he became the lab's expert on (2+1)-dimensional hydrodynamical computations (impelled by NATO's desire to possess nuclear artillery shells). This expertise was applied by him years later in computations of the behavior of colliding black holes, and by his students in a variety of astrophysical problems.

Through the efforts of John Wheeler, who had become aware of his work on quantum gravity, DeWitt was offered and accepted the directorship of the Institute of Field Physics at the University of North Carolina in Chapel Hill. His initial title at UNC was visiting research professor, which enabled him to teach, or not, as he chose, and to have students. With his very first student, and with the aid of the book of Jacques Hadamard on the Cauchy problem, he discovered the basic properties of Green's functions in curved spacetime. He was also led to the beginnings of a manifestly covariant quantum theory of gravity in which, unlike the usual approach to quantum mechanics, the Hamiltonian has no role to play.

In quantum mechanics the commutator AB-BA of any two quantities A and B is inferred from a quantity known as the Poisson bracket, which is calculated on the basis of classical mechanics. DeWitt came upon the 1952 paper of Rudolf Peierls, which gave a global definition of the Poisson bracket in terms of these Green's functions. Peierls's definition yields a completely unambiguous Poisson bracket for any pair of quantities, whose definition does not depend on the choice of coordinate system. The problem now addressed by DeWitt was to extend these classical results to the quantum theory with all its infinities.

In January 1957 Cecile, who had also been given the title of visiting research professor, organized the first of the general relativity and gravitation (GRG) conferences: "On the Role of Gravitation in Physics." The participants included Christian Møller, Leon Rosenfeld, Andre Lichnerowicz, Hermann Bondi, Thomas Gold, Dennis Sciama, Peter Bergman, John Wheeler, and Richard Feynman. Samuel Goudsmit had recently threatened to ban all papers on gravitation from *Physical Review* and *Physical Review Letters* because he and most American physicists felt that gravity research was a waste of time. The conference aimed to point out the shallowness of this view. In those early years, arguments were often put forward that gravity should not be quantized. Feynman vigorously disagreed and became interested in the problem while visiting Chapel Hill. Four years later, at the GRG conference in Warsaw, Feynman gave the first correct statement of how to quantize gravity (and also the non-Abelian gauge field) in the one-loop order of perturbation theory. He was the inventor of what are known as "ghosts" in non-Abelian gauge theories. These theories, invented in 1954 by Chen-Ning Yang and Robert Mills, became the subject of much of DeWitt's future work, and later turned out to furnish the basis of successful theories of all of the observed interactions of elementary particles except gravitation.

DeWitt, who had followed Feynman's work closely, extended it to two-loop order in 1964. In the meantime he had pushed forward on several other fronts. On three occasions he had presented courses of lectures at Les Houches. In 1963 he gave his most famous course, "The Dynamical Theory of Groups and Fields," which was published as a book the following year. In it he introduced a condensed notation applicable to all field theories, extended Schwinger's heat kernel methods to curved spacetime and other nonconstant backgrounds, and gave the first (and now standard) nonperturbative definition of the effective action as a Legendre transform of the logarithm of the vacuum persistence amplitude.

By the end of 1965 he had found the rules for quantizing the gravitational and non-Abelian gauge fields to all orders. But this work did not get published until late 1967 for two reasons. First, his Air Force grant was terminated and he could not pay the page charges that *Physical Review* had begun levying. Second, there seemed to be no rush. The standard model of electroweak interactions had not yet been worked out and the fundamental importance of the non-Abelian gauge field was not fully understood. Dimensional regularization, which would make renormalization easy, had not yet been invented. And he was momentarily sidetracked by John Wheeler's eagerness to develop a canonical approach to quantum gravity based on Dirac's theory of constraints. The application of Dirac's methods to gravity had some interesting features of its own. DeWitt was led to what subsequently became known as the Wheeler-DeWitt equation, which has since been applied many times to problems in quantum cosmology.

DeWitt's paper on the non-Abelian Feynman rules finally appeared two weeks before a paper by Fad'deev and Popov deriving the same rules. These rules were seized upon by 't Hooft and Veltmann who, apparently unaware of DeWitt's contributions, proceeded to call Feynman's ghosts "Fad'deev-Popov ghosts," a name that has stuck.

In the summer of 1968 DeWitt was visited by Max Jammer, who was thinking of writing a book on the interpretation of quantum mechanics and its history. DeWitt was astonished to learn that Jammer had never heard of Hugh Everett III, who had published a paper on this topic in the same issue of *Reviews of Modern Physics* in which contributions from the 1957 Chapel Hill conference had appeared. In fact Everett's paper, which proposed that one should regard the formalism of quantum mechanics as providing a representation of reality in exactly the same sense as the formalism of classical mechanics was once thought to do, had been totally ignored by the physics community during the intervening years. DeWitt resolved to correct this situation and in 1970 wrote a popular article in *Physics Today* expounding Everett's views. These views, although almost totally rejected at first, have little by little gained increasing numbers of adherents. The assumption that the formalism of quantum mechanics provides a direct representation of reality implies the existence of what from the point of view of classical physics would appear as many "realities." Everett's interpretation consequently became known as the "many-worlds" interpretation. DeWitt, who found Everett's ideas liberating in the sense that they lead one to ask questions that might not occur to one otherwise, became regarded as one of the foremost champions of the many-worlds interpretation, although it was always peripheral to his main interests.

By 1970 the DeWitts had begun to think of leaving Chapel Hill. Several years earlier Bryce's title had been changed to professor while Cecile had been demoted to lecturer. In addition, upon the death of Agnew Bahnson Jr., the Winston-Salem industrialist who had founded and provided financial security for the Institute of Field Physics and upon his widow's transfer of its backup funds to the university, the status of the institute underwent an abrupt change. No longer was it possible to offer postdoctoral positions with the assurance that funds would be available even if grant money failed to materialize. The postdocs of earlier years had included Felix Pirani, Ryoyu Utiyama, Peter Higgs, and Heinz Pagels. This stream of talented people had now come to an end.

In the fall of 1971 DeWitt accepted a visiting professorship at Stanford. The physics department was looking for a replacement for Leonard Schiff, who had died the year before. Stanford indeed looked promising, not least because the mathematics department expressed an interest in hiring Cecile. The members of the physics department were sufficiently pleased by Bryce's visit that they made preparations to offer him a professorship. This, however, was vetoed by Felix Bloch, who upon learning that Bryce had changed his surname 20 years earlier, refused to allow the offer to proceed.

An alternative then appeared at the University of Texas at Austin. A few years earlier Alfred Schild had secured the university's agreement to establish a well-funded Center for Relativity. Schild, as its director, brought to Austin such people as Roy Kerr, Robert Geroch, and Roger Penrose. In a short time these gifted young people were snapped up by other more prestigious institutions. There was always a vacancy at the Center for Relativity, and Schild was determined to get the not-so-young DeWitts. He arranged that they would both be offered full professorships, Cecile half-time at first in the astronomy department and then later full-time in the physics department.

Mixing astronomy and relativity, the DeWitts became co-leaders of a National Science Foundation-funded eclipse expedition to Mauritania in 1973. The aim of the expedition was to repeat, with modern technology, the light-deflection observations of bygone years. This effort would not have been possible without warm cooperation between the astronomy department and the Center for Relativity.

The DeWitts were instrumental in attracting to Austin John Wheeler, who was facing compulsory retirement at Princeton. Texas gave him a center of his own to which he invited people such as David Deutsch and Philip Candelas, with whom DeWitt had become acquainted during a Guggenheim year as visiting fellow at All Souls College, Oxford, in 1975-1976.

DeWitt's early years at Texas were devoted to the colliding black hole problem and to the problems of quantum field theory in curved spacetime, including the problem of the conformal or Weyl anomaly and the description of Hawking radiation. He also continued to develop his Hamiltonian-free approach to quantum field theory. By 1983 when he again lectured at Les Houches, he was able to set the theory of conservation laws, tree theorems, and dimensional and zeta-function regularizations completely within this framework.

In the 1980s he wrote his book *Supermanifolds.* Supermanifolds are spaces that have coordinates that anticommute (in the sense that xy=-yx), as well as having the ordinary sort of commuting coordinates (for which xy=yx). The book brought together in a systematic way a number of related but never before united topics, such as supertraces, superdeterminants, Berezin integration, super Lie groups, and path-integral derivation of index theorems. A useful topology that he introduced for integration on supermanifolds is now known by mathematicians as the DeWitt topology. A second edition of *Supermanifolds* appeared in 1991.

In 1992 DeWitt and his associates completed a lattice quantum field theory study of the O(1,2) nonlinear sigma model in four dimensions. This model, which bears some similarities to quantum gravity, proved to be trivial in the continuum limit.

DeWitt's last book, *The Global Approach to Quantum Field Theory* (1042 pages), was published in 2003, when he was 80 years old. It effectively sets forth his special viewpoint on theoretical physics and includes the following unique contents:

- A derivation of the Feynman functional integral from the Schwinger variational principle and a derivation of the latter from the Peierls bracket;
- Proofs of the classical and quantum tree theorems;
- A careful statement of the many-worlds interpretation of quantum mechanics in the context of both measurement theory and the localization-decoherence of macroscopic systems, which leads to the emer gence of the classical world;
- A display of the many roles of the measure functional in the Feynman integral, from its relation to the Van Vleck-Morette determinant in semiclassical approximations to its justification of the Wick rotation procedure in renormalization theory;
- Repeated use of the heat kernel in a wide variety of contexts, including a zeta-function computation of the chiral anomaly in curved spacetime;
- An exhaustive analysis of linear systems, both bosonic and fermionic, and their behavior as described through Bogoliubov coefficients;
- A novel approach to ghosts in non-Abelian gauge theories: use of the Vilkovisky connection to *eliminate* the ghosts in the closed-time-path formalism that is used to calculate "in-in" expectation values; and
- A proof of the integrability of the Batalin-Vilkovisky "master" equation.

DeWitt's obituary in *Physics Today* notes that:

> as a scientist, Bryce was bold and extraordinarily clear thinking. He eschewed bandwagons and the common trend of trying to maximize one's publication list. Most of his papers are long masterpieces of thought and exposition. Indeed, Bryce had a rare, perfect combination of physical and mathematical intuition and raw intellectual power that was very rarely surpassed.

To this I would add that he was a fount of wisdom about theoretical physics for his colleagues at the University of Texas. His death has left a gap in our working lives that time does not seem to cure.

For his many contributions to physics DeWitt received the Dirac Medal of the Abdus Salam International Centre for Theoretical Physics (Trieste), the Pomeranchuk Prize of the Institute of Theoretical and Experimental Physics (Moscow), and the Marcel Grossmann Prize (with Cecile). Shortly before his death he was named the recipient of the American Physical Society's Einstein Prize for 2005. He was elected to membership in the National Academy of Sciences in 1990; he was also a member of the American Academy of Arts and Sciences. DeWitt was an indefatigable trekker and mountain climber, traveled widely, and lectured in many parts of the world. He is survived by his wife, Cecile, and four daughters.

THIS MEMOIR INCORPORATES materials provided to me by Bryce DeWitt before his death.

SELECTED BIBLIOGRAPHY

1955

The operator formalism in quantum perturbation theory. University of California Radiation Laboratory Pub. No. 2884. Berkeley: University of California Radiation Laboratory.

1957

Dynamical theory in curved spaces. I. A review of the classical and quantum action principles. *Rev. Mod. Phys.* 29:377-397.

1960

Invariant commutators for the quantized gravitational field. *Phys. Rev. Lett.* 4:317-320.

1964

With C. M. DeWitt. *Relativity, Groups and Topology*. 1963 Les Houches Lectures. New York: Gordon and Breach.

Theory of radiative corrections for non-Abelian gauge fields. *Phys. Rev. Lett.*12:742-746.

1966

Superconductors and gravitational drag. *Phys. Rev. Lett.*16:1092-1093.

1967

Quantum theory of gravity. I. The canonical theory. *Phys. Rev.* 160:1113-1148.

Quantum theory of gravity. II. The manifestly covariant theory. *Phys. Rev.* 162:1195-1239.

Quantum theory of gravity. III. Applications of the covariant theory. *Phys. Rev.* 162:1239-1256.

1968

The Everett-Wheeler interpretation of quantum mechanics. In *Battelle Rencontres: 1967 Lectures in Mathematics and Physics*, eds. C. M. DeWitt and J. A. Wheeler, pp. 318-332. New York: W. A. Benjamin.

1970

Quantum mechanics and reality. *Phys. Today* 23(9):30-35.

1971

The many-universes interpretation of quantum mechanics. In *Proceedings of the International School of Physics "Enrico Fermi" Course IL: Foundations of Quantum Mechanics*, ed. B. d'Espagnat, pp. 211-262. New York: Academic Press.

1973

With F. Estabrook, H. Wahlquist, S. Christensen, L. Smarr, and E. Tsiang. Maximally slicing a black hole. *Phys. Rev. D* 7:2814-2817.

1975

Quantum field theory in curved spacetime. *Phys. Rep.* 19c:295-357.

1976

With Texas Mauritanian Eclipse Team. Gravitational deflection of light: Solar eclipse of 30 June 1973. I. Description of procedures and final results. *Astron. J.* 81:452-454.

With L. Smarr, A. Čadež, and K. Eppley. Collision of two black holes: Theoretical framework. *Phys. Rev. D.* 14:2443.

1981

Approximate effective action for quantum gravity. *Phys. Rev. Lett.* 47:1647-1650.

1984

The spacetime approach to quantum field theory. In *Relativity, Groups and Topology* II, eds. B. S. DeWitt and R. Stora, pp. 381-738. Amsterdam: North Holland.

Supermanifolds. Cambridge: Cambridge University Press.

1988

The uses and implications of curved-spacetime propagators: A personal view. Dirac Medal Lecture, pp. 11-40. Trieste: International Center for Theoretical Physics.

1989

Nonlinear sigma models in 4 dimensions as toy models for quantum gravity. In *Geometrical and Algebraic Aspects of Nonlinear Field Theory*, eds. S. De Filippo, M. Marinaro, G. Marmo, and G. Vilasi, pp. 97-112. Amsterdam: North Holland.

1990

With C. M. DeWitt. The pin groups in physics. *Phys. Rev. D* 41:1901-1907.

1992

With J. de Lyra, S. K. Foong, T. Gallivan, R. Harrington, A. Kapulkin, E. Myers, and J. Polchinski. The quantized O(1,2)/O(2) × Z2 sigma model has no continuum limit in four dimensions. 1. Theoretical framework. *Phys. Rev. D* 46:2527-2537.

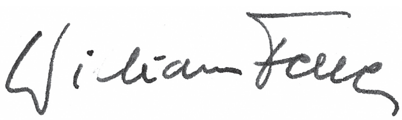
2003

The Global Approach to Quantum Field Theory. Vols. 1 and 2. Oxford: Oxford University Press.

2005

God's rays. *Phys. Today* 58(1):32-34.

Photograph by Orren Jack Turner, Courtesy Princeton University Library.



WILLIAM FELLER

July 7, 1906–January 14, 1970

BY MURRAY ROSENBLATT

William Feller was one of the major figures in the development of interest and research in probability theory in the United States as well as internationally. He was born in Zagreb, Yugoslavia, on July 7, 1906, the son of Eugen Viktor Feller, a prosperous owner of a chemical factory, and Ida Perc. Feller was the youngest of eight brothers, one of twelve siblings. He was a student at the University of Zagreb (1923-1925) and received the equivalent of a master of science degree there. Feller then entered the University of Göttingen in 1925 and completed his doctorate with a thesis titled "Uber algebraisch rektifizierbare transzendente Kurven." His thesis advisor was Richard Courant. He left Göttingen in 1928 and took up a position as *privat dozent* at the University of Kiel in 1928. Feller left in 1933 after refusing to sign a Nazi oath. He spent a year in Copenhagen and then five years (1934-1939) in contact with Harald Cramér and Marcel Riesz in Sweden. On July 27, 1938, he married Clara Nielsen, a student of his in Kiel.

At the beginning of the 20th century the most incisive research in probability theory had been carried out in France and Russia. There was still a lack of effective basis for a mathematical theory of probability. There was a notion of a collective introduced by von Mises, defined as a sequence of observations with certain desirable asymptotic properties. An effective formalization of probability theory was given in a 1933 monograph of Kolmogorov that was based on measure theory.[1] An attempt to get a rigorous format for the von Mises collective led in the 1960s to an approach using algorithmic information theory by Kolmogorov and others. Feller's research made use of the measure theoretic foundations of probability theory as did most mathematical work.

Feller's first published paper in probability theory (1936) obtained necessary and sufficient conditions for the central limit theorem of probability theory. This was a culmination of earlier work of DeMoivre, Laplace, and Liapounov. The concern is with the asymptotic behavior of the partial sums $S_n=X_1+\ldots+X_n$ of a sequence of independent random variables $X_1, X_2, \ldots, X_n, \ldots$. Assuming that means $m_k=EX_k$ and variances $B_n^2=E(S_n^2)-(ES_n)^2$ exist, Feller showed that the normalized and centered sums $\sum_{j=1}^{n}(X_j-m_j)/B_n$(with the summands $(X_j-m_j)/B_n$ uniformly asymptotically negligible) in distribution converge to the normal law

$$\Phi(x)=\frac{1}{\sqrt{2\pi}}\int_{-\infty}^{x} e^{-\frac{u^2}{2}}du$$

as n→∞ if and only if the Lindeberg condition

$$\frac{1}{B_n^2}\sum_{k=1}^{n}\int_{|x|>\varepsilon B_n} x^2 dF_k(x+m_k)\to 0$$

(F_k is the distribution function of X_k) as $n\to\infty$ is satisfied for each ε>0.

The actual fluctuating behavior of the sequence S_n was first properly formulated by Borel in 1909. After initial advances by Hausdorff, Hardy and Littlewood, and Khinchin, Kolmogorov obtained a law of the iterated logarithm, which states that under some strong boundedness conditions on the X_k that with probability one

$$\limsup_{n\to\infty} \frac{S_n - E(S_n)}{\sqrt{2B_n^2 \log\log B_n^2}} = 1$$

In a number of papers Feller through much research during his life extended and improved the law of the iterated logarithm.

A reviewing journal *Zeutralblatt für Mathematik* had been set up in 1931. The editor was Otto Neugebauer whose interests were in the history of mathematics and astronomy. Neugebauer resigned after the Nazi government's racist restrictions on reviewers were implemented. The American Mathematical Society with outside monetary support established a mathematics reviewing journal, *Mathematical Reviews*, with Neugebauer, Tamarkin, and Feller as effective editorial staff. The mathematical community is indebted to Feller for his help in setting up what became the primary mathematical reviewing journal in the world. Willy and Clara Feller moved to the United States in 1939 and Feller became an associate professor at Brown University.

In 1931 a paper of Kolmogorov on analytic methods in probability theory discussed the differential equations satisfied by the transition probabilities of continuous time parameter Markov processes (random processes with the property that past and future of the process are conditionally independent given precise knowledge of the present).[2] The conditional probability that a system at time t at location x will at time $\tau > t$ be less than or equal to y is given by a function $F(t,x,\tau,y)$. It was known that in the case of a diffusion the function F would satisfy a Fokker-Planck or diffusion equation

$$\frac{\partial F}{\partial t} + a(t,x)\frac{\partial^2 F}{\partial x^2} + b(t,x)\frac{\partial F}{\partial t} = 0$$

in the backward variables t,x, where $a(t,x)$ and $b(t,x)$ represent the local fluctuation and local drift respectively at x at time t. Feller, under appropriate growth and smoothness conditions on $a(t,x)$ and $b(t,x)$ constructed a conditional distribution function F as a solution of the diffusion equation and demonstrated its uniqueness in (1937,1). Under additional conditions on the coefficients $a(t,x)$, $b(t,x)$ the conditional distribution function $F(t,x;\tau,y)$ is shown to have a density $f(t,x;\tau,y)=\frac{\partial}{\partial y}F(t,x;\tau,y)$ in y and the density satisfies the formal adjoint differential equation in τ,y.

$$-\frac{\partial f}{\partial \tau}-\frac{\partial^2}{\partial y^2}[a(\tau,y)f]+\frac{\partial}{\partial y}[b(\tau,y)f]=0$$

The case of purely discontinuous or jump Markov processes in continuous time was also examined and under appropriate conditions an integrodifferential equation is shown to be satisfied by the constructed transition probability function of the Markov process. The more complicated situation in which one has continuous excursions as well as jumps was also considered. These results were a strong extension of those of Kolmogorov.

The paper (1939) is an example of Feller's continued interest in applications, here in a biological context. He looks at the Lotka-Volterra equations, which are a deterministic predator-prey model as well as a deterministic model of population growth (and decrease). In setting up corresponding Markovian models it is noted that the mean response in the stochastic models may not agree precisely with the original deterministic models. A diffusion equation is shown to arise naturally if population is considered a continuous variable but an equation that is singular in having the coefficient $a(x)$ zero at a boundary point.

An amusing and insightful paper (1940,2) of Feller's related to some presumed statistical evidence for the existence of extrasensory perception (ESP). Experiments were carried out involving sequences of fair coin tosses. Track was kept of the successful versus unsuccessful guesses of the coin tossing up to time n, S_n. It was noted that there were often large excursions of S_n above zero as well as large excursions below zero. A large excursion above zero was interpreted as presence of ESP and a large excursion below zero as its departure. Feller incisively notes that the standard model of fair coin tossing accounts for such excursions without the extra introduction of special effects like ESP.

Feller moved to Cornell University in 1945 as professor of mathematics and remained there until 1950, when he left to go to Princeton University. While at Cornell he wrote the paper (1948) in which he derived the Kolmogorov-Smirnov limit theorems by methods of a simpler character than those used to derive these results originally. The limit theorems provide procedures for effective one and two sample statistical tests. An elegant paper (1949,1) written jointly with Paul Erdös and Harry Pollard used elementary methods to establish limit behavior of transition probabilities for countable state discrete time Markov chains under appropriate conditions (a result obtained earlier by Kolmogorov using arguments that were more elaborate). Feller's paper (1949,2) on recurrent events also appeared and extended the basic idea of an argument that often was used in the analysis of Markov chains and that was perhaps suggested by early work of Wolfgang Doeblin.

The year 1950 is marked by the publication of the first volume of Feller's *Introduction to Probability Theory and Its Applications*. The book gives an extended discussion of the nature of probability theory. To avoid measure theory the development is limited to sample spaces that are finite or countable. Results on fluctuation in coin tossing and random walks are dealt with. There is the usual discussion of conditional probability and stochastic independence. The binomial and Poisson distributions are introduced together with the classical deMoivre normal approximation to the binomial distribution. Definitions of random variables and expectation are then given. A law of large numbers and a law of iterated logarithm (for partial sums of 0-1 random variables) are given as limit laws in the context of finite sample size with increasing sample size so as to avoid a formal discussion using a background of measure theory. Branching processes and compound distributions are introduced using generating functions as a convenient tool. Recurrent events (as introduced by Feller) and renewal theory then follow and are considered in the context of Geiger counters or the servicing of machines. Stationary transition function countable state Markov chains are introduced as examples of dependent sequences. The Chapman-Kolmogorov equation is noted as a consequence of the Markov property (but not equivalent to it). The ergodic properties of Markov processes are then developed. An algebraic treatment is given for finite state Markov chains. Finally birth and death processes are introduced and considered as examples of countable state continuous time parameter processes. The book is remarkable with its extensive collection of interesting problems and its discussion of applications.

Feller worked for eight years on the preparation of this volume. The volume was completed in the last year of Feller's tenure at Cornell University. The book is dedicated to Neugebauer. Gian-Carlo Rota remarked that the book was "one of the great events in mathematics of this century. Together with Weber's *Algebra* and Artin's *Geometric Algebra* this is the finest textbook in mathematics in this century. It is a delight to read and it will be immensely useful to scientists in all fields."[3]

The academic year 1949-1950 at Cornell University was a truly remarkable one in probability theory. The permanent and visiting faculty members were W. Feller, M. Kac, J. L. Doob, G. Elfving, G. Hunt, and K. L. Chung, and it was a most stimulating time for students.

I recall some impressions from my own days as a graduate student in the late 1940s at Cornell, where I took most of my courses in stochastics with Feller as a lecturer. Though I completed my doctorate with Mark Kac as adviser, I had an overwhelming impression of Will Feller as a man of supreme enthusiasm and occasional exaggeration that at times required some modification. An amusing example is given by a lecture where he introduced the three series theorem and turned to the student audience inquiring, "Isn't it obvious?" Luckily we persuaded him to give a detailed presentation, and a series of two or more lectures on the theorem followed. He did give great insight in his lectures.

In 1950 he took a position at Princeton University as Eugene Higgins Professor of Mathematics. He held this position until his death on January 14, 1970, at 63 in the Memorial Hospital of New York.

The paper (1952,1) is an indication of Feller's renewed interest in diffusion processes and their application in genetics. Stochastic processes as models in genetics and the theory of evolution are developed. Current methods at that time were due to R. A. Fisher and Sewall Wright for the most part. It is indicated how in a model of S. Wright the gene frequency $u(t,x)$ that satisfies a diffusion equation

$$u_t = \{\beta x(1-x)\,u\}_{xx} - \{[\gamma_2 - (\gamma_1+\gamma_2)\,x]\,u\}_x$$

is obtained by an appropriate limiting process from a bivariate discrete model. The equation has singular boundary points at 0 and 1. Here β, γ_1 and γ_2 are constants with the γ's denoting mutation rates.

In the 1950s Feller carried out his well-known research on one dimensional diffusion processes with stationary transition function $F(t,x;\tau,y)=P(\tau-t;x,y)$. He made use of appropriate modifications of the Hille-Yosida theory of semigroups. The stationary transition function $P(t)= P(t;x,y)$ generates a semigroup

$$P(t+s;x,y)= \int P(t;x,dz)P(s;z,y),\ \ t,s\geq 0$$

because of the Chapman-Kolmogorov equation. The transition function can be considered as an operator on appropriate spaces. Feller found it convenient to make the assumption that the transition function $P(t)$ acting on the space of continuous functions f

$$\{P(t)f\}(x)= \int P(t;x,dy)f(y)$$

takes continuous functions f into continuous functions $P(t)f$. Such transition functions are now usually called Feller transition functions. The derived operator L given by

$$\lim_{t\downarrow 0}(P(t)f-f)/t=Lf$$

is defined for a subclass of functions f and is in the case of the one dimensional diffusion the second order operator in x of the Fokker-Planck equation. A corresponding Markov diffusion process was determined by boundary conditions that might differ from those conventionally dealt with in the standard theory of differential equations. The boundary conditions could be regarded as a restraint on the class of functions on which L operated that in turn made the restrained L the infinitesimal generator of a properly defined transition function semigroup. He generalized the type of differential operator of diffusion theory. Every such operator (barring certain degenerate cases) could be written in the form $(d/d\mu)(d/d\sigma)$, with σ a scale function and μ an increasing function (or speed measure). In a natural way the general linear diffusion was related to a Wiener process (Norbert Wiener's model of Brownian motion) that was locally rescaled in space and speed. Aspects of this program were laid out in the papers (1954; 1959,1). Dynkin carried out research on a number of related problems.

Feller also carried out analyses of countable state continuous time Markov chains with stationary transition probabilities. Here the transition probabilities are given by a matrix-valued function $P(t)=(p_{ij}(t))$ (i,j states of the process) with the semigroup property $P(t)P(s)=P(t+s),\ t,s \geq 0$. If $P(t)$ is differentiable at zero with finite derivatives $q_{ij}=p'_{ij}(0)$, the equalities $\sum_j q_{ij}=0$ hold. The backward differential equation $QP(t)=P'(t)$ and the forward differential equation $P(t)Q=P'(t)$ are often referred to as the Kolmogorov differential equations ($Q=(q_{ij})$). In (1957,2) Feller described a general method of constructing transition probability functions $P(t)$ that satisfy the conditions on the q_{ij}'s. It is also shown how to construct transition functions $P(t)$ that satisfy both systems of differential equations. Research on topics of this type was also carried out by J. L. Doob, K. L. Chung, H. Reiter, and D. Kendall.

The second volume of Feller's *Introduction to Probability Theory and Its Applications* appeared in 1966. It was written so as to be independent of the first volume. Further, it was aimed to be of interest to a large audience ranging from a novice to an expert in the area. The book certainly succeeds, but it understandably could not be as popular as the first volume. The first few chapters deal with special distributions like the exponential, the uniform, and the normal. Chapter 4 introduces probability spaces and probability measures. Laws of large numbers, the Hausdorff moment problem, and the inversion formula for Laplace transforms follow in Chapter 7. The central limit theorem and ergodic theorems for Markov chains are obtained in Chapter 8. Infinitely divisible distributions follow in the next chapter. A host of additional topics follow in the remaining chapters of the book: Markov processes and semigroups, renewal theory, random walks on the real line, characteristic functions, expansions related to the central limit theory, the Berry-Esséen theorem on the error term in the central limit theorem, large deviations, and aspects of harmonic analysis. Many of the topics are dealt with in an elegant and succinct manner.

Feller was always interested in the problems of genetics. Toward the end of his life, as a permanent visiting professor at Rockefeller University, he had a close collaboration there with Professor Dobzhansky and colleagues. The paper (1966,2) is a result of this interaction and corrects an error in the theory of evolution due to assumption of constant population size in the case of a two-allele population.

The papers (1968, 1970) show Feller's continued research on problems related to the law of the iterated logarithm continued throughout his life.

Feller's investigations were greatly appreciated. He was elected to the National Academy of Sciences in 1960 and was a member of the American Philosophical Society, the American Academy of Arts and Sciences, and a foreign member of the Danish and Yugoslav academies of science. He was president of the Institute of Mathematical Statistics. His widow accepted the National Medal of Science on his behalf in 1970.

J. L. Doob, who was as influential as Feller in nurturing and developing the growth and interest in probability theory, remarked,

[A]part from his mathematics those who knew him personally will remember Feller most for his gusto, the pleasure with which he met life, the excitement with which he drew on his endless fund of anecdotes about life and its absurdities, particularly the absurdities involving mathematics and mathematicians. To listen to him deliver a mathematics lecture was a unique experience. . . In losing him, the world of mathematics has lost one of its strongest personalities as well as one of its strongest researchers.

A colleague of many years at Cornell University and Rockefeller University, also a remarkable researcher in probability theory, Mark Kac, said of Feller:

Feller was a man of enormous vitality. . . The intensity of his reactions was reflected in what his friends called the "Feller factor," an imprecisely defined number by which one had to scale down some of his pronouncements to get near the truth. . . But he was not stubborn and underneath the bluster, kind and generous. . . Much as he loved mathematics, his view of it was anything but parochial. . . I recall a conversation in which a colleague asked, . . . "What can the generals do that we mathematicians couldn't do better?" "Sleep during battle," said Feller and that was that. . . When he learned that his illness was terminal his courage and considerateness came poignantly to the fore. Having accepted the verdict himself he tried to make it easy for all of us to accept it too. He behaved so naturally and he took such interest in things around him that he made us almost forget from time to time that he was mortally ill. . . One of the most original, accomplished and colorful mathematicians of our time.

Henry McKean, a student of and co-researcher with Feller noted:

[H]is enthusiasm, his high standards, his indefatigable desire to make you understand "what's really going on." That was also his watchword when he lectured. He would get quite excited, his audience in his hand and come (almost) to the point. Then the hour would be over, and he would promise to tell us what's really going on next time. Only next time the subject would be not quite the same, and so a whole train of things was left hanging, somewhat in the manner of Tristram Shandy. But it didn't matter. We loved it and couldn't wait for the next (aborted) revelation. . . Back to Will himself. He was short, compact, with a mop of wooly gray hair, irrepressible. In conversation quick, always ready with an opinion (or two) addicted to exaggeration. If you knew the code, you applied the "Feller factor" (discount by 90%). . . I think of him often, hearing his voice, remembering him so full of fun.

THIS MEMORIAL IS BASED IN part on an obituary in The Annals of Mathematical Statistics 1970, vol. 41 and on accounts written by J. L. Doob and M. Kac in the *Proceedings of the 6th Berkeley Symposium on Mathematical Statistics and Probability*. Helpful written remarks of H. P. McKean have also been used.

NOTES

1. A. Kolmogorov. *Grundbegriffe der Wahrscheinlichkeitsrechnung.* Berlin: Springer, 1933.
2. A. Kolmogorov. über die analytischen Methoden in der Wahrscheinlichkeitsrechnung. *Math. Ann.* 104(1931):415-458.
3. Back Cover Blurb. from W. Feller. *An Introduction to Probability Theory and Its Applications,* vol. 1, 3rd edition. New York: Wiley, 1968

SELECTED BIBLIOGRAPHY

1928

Über algebraisch rektifizierbare transzendente Kurven. *Math. Z.* 27:481-495.

1936

Über den zentralen Grenzwertsatz der Wahrscheinlichkeitsrechnung. *Math. Z.* 40:521-559.

1937

[1] Zur Theorie der stochastischen Prozesse (Existenz und Eindeutigkeitssätze). *Math. Ann.* 113:113-160.

[2] Über den zentralen Grenzwertsatz der Wahrscheinlichkeitsrechnung. II. *Math Z.* 42:301-312.

1939

Die Grundlagen der Volterraschen Theorie des Kampfes ums Dasein in wahrscheinlichkeitstheoretischer Behandlung. *Acta Biotheor.* A 5:11-40.

1940

[1] On the integro-differential equations of purely discontinuous Markov processes. *Trans. Am. Math. Soc.* 48:488-515.

[2] Statistical aspects of ESP. *J. Parapsychol.* 4(2):271-298.

1941

On the integral equation of renewal theory. *Ann. Math. Stat.* 12:243-267.

1943

The general form of the so-called law of the iterated logarithm. *Trans. Am. Math. Soc.* 54:373-402.

1945

The fundamental limit theorems in probability. *Bull. Am. Math. Soc.* 51:800-832.

1946

The law of the iterated logarithm for identically distributed random variables. *Ann. Math.* 47:631-638.

1948

On the Kolmogorov-Smirnov limit theorems for empirical distributions. *Ann. Math. Stat.* 19:177-189.

1949

[1] With P. Erdös and H. Pollard. A property of power series with positive coefficients. *Bull. Am. Math. Soc.* 55:201-204.

[2] Fluctuation theory of recurrent events. *Trans. Am. Math. Soc.* 67:98-119.

1950

An Introduction to Probability Theory and Its Applications, vol. 1. New York: Wiley.

1952

[1] Diffusion processes in genetics. *Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability*, ed. J. Neyman. pp. 227-246: Berkeley and Los Angeles: University of California Press.

[2] Some recent trends in the mathematical theory of diffusion. *Proceedings of the International Congress of Mathematicians (2)*, pp. 322-339. Providence: American Mathematical Society.

1954

The general diffusion operator and positivity preserving semigroups in one dimension. *Ann. Math.* 60:417-436.

1957

[1] Generalized second order differential operators and their lateral conditions. *Illinois J. Math.* 1:459-504.

[2] On boundaries and lateral conditions for the Kolmogorov differential equations. *Ann. Math.* 65:527-570.

1959

[1] Differential operators with the positive maximum property. *Illinois J. Math.* 3:182-186.

[2] The birth and death process as diffusion process. *J. Math. Pure. Appl.* 38:301-345.

1966

[1] *An Introduction to Probability Theory and Its Applications*, vol. 2. New York: Wiley.

[2] On the influence of natural selection on population size. *Proc. Natl. Acad. Sci. U. S. A.* 55:733-738.

1968

An extension of the law of the iterated logarithm to variables without variance. *J. Math. Mech.* 18:343-356.

1970

On the oscillations of the sums of independent random variables. *Ann. Math.* 91:402-418.

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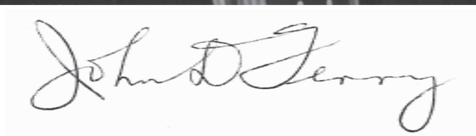


JOHN DOUGLASS FERRY

May 4, 1912–October 18, 2002

BY ROBERT F. LANDEL, MICHAEL W. MOSESSON,
AND JOHN L. SCHRAG

PROFESSOR JOHN D. FERRY was a towering figure in polymer science—in the development of our understanding of viscoelasticity and its origins in polymer structure and associated local and long-range molecular motions. His enormous contributions to the field were such that his career was both a centerpiece and a mirror of these developments. His work was always marked by a persistent, orderly, and detailed investigation of unsolved areas: actively identifying them, developing experimental tools to investigate them, interpreting the results on phenomenological and molecular scales as appropriate, and, from this, identifying the most important areas to be pursued next.

John left polymer science with an enormous legacy of understanding of both the linear viscoelastic properties of polymeric systems and the origins of these properties in their conformations and motional dynamics. This was not just an experimental legacy developed by him and his colleagues during a nearly 60-year career but, most importantly, a conceptual legacy woven from threads drawn from across the polymer field and elegantly summarized in the three editions of his book *Viscoelastic Properties of Polymers.*[2] Its translation into Japanese, Russian, and Polish is a tribute to its authority.

John's Ph.D. research (under George Parks, completed in 1935) at Stanford University began at a time when the concept of polymers as giant molecules was just being accepted. For his Ph.D., Ferry sought to determine whether polyisobutylene, then a laboratory curiosity, had a glass temperature.[1] His research also included an investigation of the temperature dependence of the viscosity of poly-isobutylene.[1] It was here that he first encountered the phenomenon of viscoelasticity that was to become the central tenet of his research. On trying to turn a rod immersed in a sample contained in a tube to measure its viscosity, he was astonished to find that when the rod was released it would spin backwards. It was also at this time that he began to develop his science philosophy, which was as follows:

1. When one has related scientific phenomena that depend on many variables, much depends on how one formulates the dependence. If one can arrange the variables suitably then some important generalizations may appear that will provide considerable insight. Thus,a researcher should set up a way of looking at the group of phenomena—a conceptual scheme— which would lead to alternating theoretical and experimental work. In addition, in the development of a science it is important to have a conceptual scheme and notation accepted by workers in the field, allowing everyone to communicate readily and to approach problems from a common general point of view.

2. When planning experiments to answer a particular question one should make experiments not just accurate enough to answer that question, but much more accurate, if possible. Then one may not only answer the original question but also discover something entirely new.

These concepts were employed repeatedly in Ferry's lab and led to many new discoveries.

Between obtaining his B.A. and Ph.D. degrees, Ferry went to the National Institute for Medical Research in London to learn how to make ultrafiltration membranes that could separate proteins according to size. This was the start of his parallel second career: the study of proteins, an activity as unknown to the polymer community as his polymer work was unknown to the protein community. (Many years later he confessed that he was really leaping at the chance to go to Europe. When his project ended, he "spent three marvelously enjoyable and instructive months rambling about there.")

John's first employment after earning his Ph.D. was as a private research assistant to David Spence at the Hopkins Marine Station of Stanford University. Spence was the first recipient of the Charles Goodyear Medal, an award Ferry would recieve in 1981. With Spence, he worked on the chemistry of cross-linking reactions in the curing of natural rubber.

Ferry went to Harvard University in 1937 with joint appointments as an instructor and tutor in biochemical sciences, and as a researcher in the Department of Physical Chemistry, headed by E. J. Cohn, in the Harvard Medical School. In the latter role he worked under J. L. Oncley on the dielectric properties of proteins. Here he became tremendously impressed by the elegance and power of frequency-dependent measurements, which simultaneously gave information about the energy storage (elastic) and energy loss (viscous) components of a material's response. Ferry determined to employ similar measurements in the study of polymers.

In 1938 he was appointed a junior fellow of the Society of Fellows at Harvard. This enabled him also to pursue (still half-time) studies of his own choice, which by then had settled down to the viscoelastic properties of polymers as measured by dynamic mechanical methods. He decided that all measurements should be in shear, never tension, because of the anticipated great difficulty in accurately determining the separate contributions of shear and bulk response. It was here, in 1941, that he invented his unique shear wave propagation apparatus, with which he took advantage of the flow birefringence of polymer solutions and gels to visualize propagating waves.[3] He found the theory for shear wave propagation in a viscoelastic medium in a geology journal and got help with the optical side from Professor Mueller at MIT who was one of the world's experts on the physics of anisotropic media. This work on viscoelasticity was interrupted by World War II (though two papers appeared in 1942), but the wave machine became a mainstay in his early years at Wisconsin.

During World War II, Ferry held joint appointments at Woods Hole Oceanographic Institute and at Harvard Medical School. At Woods Hole he worked on antifouling paints for navy applications. Here he met his future wife, Barbara Norton Mott, who had just completed her degree in chemistry at Radcliffe. (Their two children, Phyllis and John M., were born in Madison, Wisconsin.) At Harvard he was attached to another E. J. Cohn project, finding uses for various plasma proteins. Large quantities of these blood components were being obtained as by-products from the large-scale fractionation of human blood to obtain blood plasma and red blood cells for clinical use by the U.S. armed forces. The unit to which John was attached was asked to find applications for fibrinogen. John, along with Peter Morrison, produced two particularly useful materials: a fibrin foam that found extensive use in the stoppage of bleeding during surgery and tooth extractions, and a highly elastic and tough fibrin film. This fibrin film became the first safe and effective surgical replacement for the dural membrane, thus making brain surgery feasible for the first time; it was used extensively to treat head wounds in the later stages of the war. A biodegradable tubular form of fibrinogen, suitable as a blood vessel replacement or scaffold was also produced, but too late to be used during the war. (The requisite large-scale pooling of blood before fractionation made contamination of the fibrinogen products a major complication. This resulted in abandonment of these materials for medical purposes after the war. In the 1990s, means of avoiding contamination were developed, which has led to a resurgence of interest in, and clinical use of, such materials.) John and Peter also produced interesting fibrinogen-based plastics, eventually resulting in two patents. Three additional patents were issued to John and other collaborators. All patents were held up until after the war, and the last was applied for and granted even later. Thus began an aspect of his work (30 percent of his papers) that is little known to the polymer community: fibrinogen and its conversion to fibrin by polymerization.[4]

His group at Wisconsin first proposed, in 1947, the detailed mechanism for this polymerization as a stepwise lateral dimerization of the rod-like fibrin monomer units, with partial lengthwise overlapping, to give double stranded fibrils with a structure resembling a two-layer brick wall.[5] This idea became the theoretical basis for almost all subsequent studies on fibrin polymerization and was amply corroborated by numerous multidisciplinary studies that appeared over the ensuing years. It has now become an axiom.

In 1971, after a gap of 14 years, his interest in fibrinogen-fibrin conversion resumed, and in concert with students and postdoctoral colleagues he continued to make many important contributions, always in the context of contemporary ideas. These studies included investigations of the viscoelastic properties of fibrin clots that had been covalently linked with factor XIIIa compared with those that had not been ligated, often supported by flow birefringence, light and small-angle X-ray scattering, and electron microscopic analyses.

Even after his retirement in 1982 Ferry participated in society and group meetings on fibrin polymerization in Madison, Milwaukee (International Fibrinogen Workshop, 1988), and elsewhere (New York Academy of Sciences, 1982). In his six years of publishing after retirement, 23 of 40 papers by him and his colleagues were on fibrinogen. One important result of these studies came from his insightful connection of the previously determined mechanical properties of cross-linked fibrin with the probable positioning of ligated gamma chains in the assembled polymer. His reasoning was clearly set forth in a 1996 letter to one of us (M.W.M.), which provided a functional connection between two heretofore-unrelated fibrin properties. This notable achievement was recently chronicled in Biophysical Chemistry (112 [2004] :215).

Ferry continued his protein studies when he went to Wisconsin in 1946, though he was now focused on dynamic mechanical and viscosity behaviors. He continued to research proteins in separate bursts throughout his career. The materials he studied included myosin, paramyosin, and meromyosin; DNA, RNA, TMV, and sodium thymonucleate; gelatin and collagen; and poly-γ-benzyl-L-glutamate. After 1971 Ferry took advantage of new instumentation developed in his laboratories to explore molecular stiffness variations in these materials.

When he arrived at Wisconsin and returned to his polymer work, it was a time of sorting out and trying to understand the vague outlines of viscoelastic response, especially its temperature dependence. Three research pioneers had contributed substantially to the concept of time-temperature superposition in polymers. Leaderman discussed its potential application to mechanical, dielectric, and magnetic relaxation experiments in 1943; Andrews and Tobolsky used empirical shifts along the log time axis to construct the first "master curves" for isothermal stress relaxation data in 1945.

In 1950 in a simple but massively powerful stroke, Ferry introduced the key and unifying principle of *reduced variables* in linear viscoelasticity and rheology. This gave the physical basis and a general mathematical form for experimental observations on time-temperature superposition. With reduced variables, any response, e.g. modulus or compliance, of bulk polymers (and in some cases, solutions) was cleanly separated into two functions: one for the response vs. reduced time (or frequency) alone and a second for the dependence of the time scale shift factor, a_T, on temperature. (He introduced the symbol a_T.) Thus for pure polymers one now had a way of examining the time or frequency dependence alone, for all types of small strain deformation—transient, dynamic and steady flow, and similarly for the temperature dependence of the time scale alone.[2] An earlier extension of this principle to the behavior of solutions gave a third, similar, (though approximate) general function: a_c. The study of concentration dependence was important in those days because at that time experimental techniques were so limited in their temperature and frequency ranges that definitive studies were carried out primarily on concentrated solutions. Changing the concentration in such solutions dramatically changes the time scales of chain motions, which is why this was useful even if only approximately correct.[2] (It isn't even approximately correct for dilute solutions.)

Ferry immediately employed the superposition principle as a practical tool by showing how the shift factor, a_T, for solutions could be readily obtained from simple steady flow viscosity measurements. Later he extended the concept to the effect of pressure on time scales, leading to another function, a_P[2].

Equally important, failure of time-temperature superposition in any one of the different responses points to new molecular mechanisms contributing to the response. For example, poly n-butyl methacrylate at high temperature and low frequencies had a different a_T for J" than for J', G', and

G”, due to an additional dispersion centered at still lower frequencies that only J” was sensitive to.

In the years immediately following this development, Ferry carried out extensive studies of the viscoelastic properties of well-characterized polymers to examine the influence of molecular structure. Here the choice of polymers was critical, partly because of limitations in instrumental working frequency and temperature ranges. Ferry’s mass of accurate and detailed data over closely spaced temperatures provided the base for the development of the widely used Williams-Landel-Ferry equation (WLF). This equation and its molecular underpinnings were major paradigm shifts for rheology, establishing the concepts that T_g and fractional free volume are defining molecular parameters. Thus the temperature dependence of log a_T for amorphous homopolymers and nonblock copolymers, of widely different chemical structures, is remarkably similar but displaced along the temperature scale according to their glass transition temperatures, T_g. The a_T behavior can be described by a widely applicable empirical equation in $T - T_s$, where the reference temperature, T_s, lies about 50 degrees above T_g.[2] (It was later found by others that for noncrystallizing, nondegrading polymers, this expression could be used to estimate very long time properties, including rupture, well beyond the experimentally accessed range.)

The response of individual polymers is described by a more detailed, molecularly based analysis based on fractional free volume, its temperature dependence, and T_g. Thus, the fractional free volume occupies a central position in trying to understand the molecular origins of the temperature dependence of viscoelastic response. Furthermore, T_g is a defining temperature for viscoelastic response even though it is operationally difficult to measure.

In addition, the theory behind the WLF equation showed that there is a need to understand the magnitude and time dependence of volume changes associated with any mechanical property measurement. This item stems especially from Kovac's work, initiated in Ferry's lab, on the simultaneous measurement of the time dependence of volume and properties, after a temperature jump.[2] The fractional free volume concept has been the underlying driver for studies of physical ageing and of the effect of high pressure on T_g and mechanical properties.

The WLF equation has had a huge and widely diverse practical impact. E. g., for polymeric solids: acoustic and vibration damping, tear, tire friction, ultimate (break) properties, diffusion (e.g. slow pheremone release) and physical ageing after molding; for solutions and melts: fabrication and molding. It has also found wide use in fabrication, molding and use properties for foodstuff and cosmetic materials.

Rouse's molecular theory, which described the viscoelastic properties of long-chain molecules in terms of chain conformational dynamics, appeared just prior to the Williams-Landel-Ferry equation. Rouse had been intrigued by Ferry's experimental results and wanted to provide a physics explanation. Ferry immediately recognized that Rouse's theory provided the molecular level framework he had been searching for. This led to many seminal contributions, including the following:

- Giving a molecular basis for both reduced variables and the explicit form observed for the relaxation spectrum's different time dependencies in the dispersion, plateau, and termination (or onset of steady flow-like behavior) zones.
- Applying the extended Rouse theory to his unique, extensive study of the glass-to-rubber transition zone employing five methacrylate esters so that the influence of chemical structure and side-chain length could be explored. This is still the most complete study ever made of the effect of a regular

change in side-chain substituents on the overall response of the same backbone chain. This work showed that although the directly measured responses or properties were quite different, the relaxation spectra were essentially identical in shape although displaced in magnitude and location on the time scale. The displacements were accounted for by differences in the polymer's effective bond length (determined independently by light scattering from dilute solutions), the Rouse monomeric friction coefficient, the molecular weight between entanglements and the fractional free volume.[2]

• Definitive experiments that demonstrated the direct relationship between small molecule diffusion in a rubber matrix and the Rouse monomeric friction coefficient for the matrix chains. This had practical implications for such widely diverse areas as air retention in tires, controlled drug and insect pheromone release, adhesive bonding of polymers, and healing of fractures in polymers.[2]

• Extensive studies of the entanglement plateau region for uncrosslinked polymers. This work showed the importance of the molecular weight between entanglements, M_{en}, as the dominant parameter controlling observed viscoelastic properties in this region of response, although the effect of entanglement coupling of chains enters as well. The extent of entanglement was varied by selecting different chemical structures (polyisobutylene, styrene-butadiene, natural rubber, and polydimethylsiloxane) or by modifying the type and extent of branching. Later, even more extensive studies of the plateau region enabled him to demonstrate that additional mechanisms can appear or disappear as temperature is changed. G', G", J', J", or tan δ then will not superpose. The additional vertical and/or horizontal shifts required for superposition indicate the molecular origin of this behavior.

Subsequently, Ferry made detailed investigations of the effect of trapped entanglements in cross-linked systems. The trapped entanglement treatment developed by Langley and Ferry is probably the best, most comprehensive description of such systems to date.[2] Ferry then used novel, ingenious, and revealing two-network experiments: a) with chemically cross-linked systems containing *free, unattached* chains, and b) with *two networks* formed by employing a new technique of radiation cross-linking specimens prestretched at reduced temperature, and then warmed so that the initial entanglement network was in a state of extension and the cross-linked network in a state of compression. These studies enabled him to explain the uniaxial extension behavior of systems that included both permanent and transient cross-links. Investigation of trapped entanglements also led him, for the first time, to study response outside the linear range. Here he showed, for example, that the Mooney-Rivlin C_2 parameter could be attributed entirely to the entanglement network. This gave a molecular explanation for the well-known fact that C_2 increases, decreases, or remains constant as the degree of cross-linking is increased.

Two-network studies also enabled molecular interpretation of the time dependence of the effects of trapped entanglements in terms of the tube model as separate contributions of segmental motion within the tube diameter, whole chain diffusion along the tube, and tube rearrangement itself.[2]

In work carried out after publication of the third edition of *Viscoelastic Properties of Polymers*, Ferry and Fitzgerald initiated a series of studies on carbon-black-filled rubbers showing that in the linear viscoelastic range, black particles formed their own associative network. Its contribution to the behavior of rubbers was then examined by Ferry in novel experiments (e.g., measuring the small strain dynamic response before, during, and after the application of a 40 percent strain for a period of time). The results showed that the black particles form an independent, separate network of their own in which a temperature jump causes a vertical shift in dynamic properties, without the timescale shift seen in the unfilled polymers, by an amount that depends on the type of black. In isothermal straining, the density of the black network junctions was decreased at step strains but then recovered rather than decreasing over time if the step was not too large.[6]

By 1967 Ferry's group had obtained extensive solution viscoelasticity data for synthetic polymers in viscous solvents, but instrumental limitations were such that high precision measurements could not be obtained on dilute to very dilute solutions, especially at higher frequencies. This severely limited molecular-level interpretations of the data. In addition, since solution viscosities had to be greater than 4 poise for instrumental reasons, this precluded studies of aqueous based biopolymer solutions.

From 1967 to 1970 his group carried out major revisions of existing apparatus, developed unique computerized high-precision data acquisition systems, and built new instruments (especially multiple-lumped resonators) that enabled very-high-precision viscoelasticity measurements for solution viscosities down to 0.02 poise at shearing frequencies up to approximately 10 kHz.[2,7,8,10] This enabled Ferry's group to obtain, on an extensive scale, high-precision data for very dilute solutions of a wide variety of synthetic and biopolymers. These measurements were sufficiently precise and covered sufficiently broad frequency ranges to provide reliable extrapolations to obtain the infinite dilution properties required for quantitative tests of the elegant (isolated molecule) statistical mechanical theories of Kirkwood, Rouse, Zimm, Tschoegl, Peterlin, and others. These results were the first to unambiguously establish that the Zimm theory (bead-spring model)—when modified to include a finite number of polymer segments, intermediate hydrodynamic interaction, and exact eigenvalues—provides astonishingly precise predictions for the global motion component of the measured viscoelastic properties for linear and regularly branched polymers (less than seven arms), and provides semiquantitative-to-quantitative predictions for comb structures and randomly branched chains as well.[8-11]

This new characterization potential led to extensive studies exploring the influence of molecular weight, molecular weight distribution, chain flexibility, side-group size, long-chain branching, solvent quality, and charge screening (polyelectrolytes) on the observed viscoelastic properties. Polymer coil expansion obtained by either employing good solvents or inserting charges along the chain also substantially alters the observed frequency dependence. The high effective frequency range now accessible gave high-frequency viscoelastic properties showing, unexpectedly, that η' did not approach the solvent viscosity limit as was expected.[11,12] In many solvents this high-frequency limit of η', denoted as η'_∞, was later found by Stokich and others to reflect a substantially modified, strongly temperature-dependent solvating environment contribution (modified by the presence of polymer) as well as other contributions. Ferry's group also was the first to note that fairly rigid biopolymers exhibit quite different behavior, reflecting a combination of overall rotatory and flexural motions rather than the "entropy spring" behavior of flexible (Gaussian) chains, and developed an empirical "hybrid relaxation time spectrum" to describe the observed properties.[13] Shortly thereafter a Japanese theoretician confirmed the validity of this spectrum.

One of the principal reasons that John Ferry made such wide-ranging, powerful, and unique contributions to our understanding of the role of molecular motions in rheology was that with each move to a different area, he and his collaborators developed excellent new, high-precision instrumentation that could probe the requisite temporal and viscosity regimes.[2,3,7,8,12,14-18] Such instrumentation was not (and to a large degree still is not) available commercially. Generally, experimental studies of chain dynamics via viscoelasticity have substantially preceded theoretical understanding, due largely to the unique and enabling instrumentation developed by Ferry and his collaborators and the resulting investigations.

His influence and impact on people who interacted with him can be gleaned from illustrative comments (personal communications to one of us [R.F.L.])made by peers and associates. K. Ninomiya, Japan Synthetic Rubber Co., once said, "His lab had the most research-conducive atmosphere I ever encountered anywhere." Similarly, DeGennes has said (before reptation), "I was trying to find out more about polymers after hearing a Sadron lecture. I looked at lots of books to no avail. But then someone told me about Ferry's book… And there it was!"

On the personal side,[19] John is almost certainly the only American polymer scientist to have been born in Dawson in the Yukon Territory of Canada. He spent his first two years in that immediate area since his father was a civil and mining engineer specializing in dredge mining of placer deposits. Having seen the Yukon ice breakup, he was officially a sourdough. (Perhaps his early years in that cold environment are the reason why he tended to be most interested in polymer properties well above T_g!) Most of John's childhood was spent in small mining communities in Idaho and Oregon. At age eight he had a boy's size "rocker" for processing gold-bearing gravel. (Until very late in life he remained an expert with the gold pan, employing all the correct swirling and sloshing motions while maintaining the proper tilt.) Later he helped his father survey and assay placer gold. His father often told him, and this much later influenced his science philosophy, "John, you must always be sure to extract the very last nugget from your claim."

Ferry attended a one-room school in the now ghost town of Murray, Idaho. Murray was similar to Dawson in that the family was snowed in from November until May; a trip to the doctor was an all-day affair by horse-drawn sleigh over two mountain passes. At school he completed the eight grades in four years of what he described as "somewhat uneven training." A voracious reader, while still in the first grade he was successively moved to higher-level reading classes until placed in the seventh-grade level. The next year his father held him out of school because he was so much younger than his classmates. As an adult he could not remember being taught to read at home but did remember reading extensively at age five. He noted that his mother had graduated from college, in classics, and taught Latin and German in high school, and that the family had an extensive library. During high school, John taught himself enough Latin and German to later go into advanced courses in these subjects. This fascination with language persisted throughout his life as his most extensive avocation.

John entered Stanford University at age 16, after again being held back by his father for a year, and received his A.B. degree three years later in 1932 (undergrad research with Parks). He was the first Stanford undergraduate (out of about 32,000) to achieve a straight A average. In those days Stanford's Department of Chemistry each year selected and prominently displayed on a silver cup the name of the outstanding freshman chemistry student. In 1929 John's name was posted; in 1930 it was David Packard, later of the Hewlett-Packard Co.

John loved music and began to sing in the choir as a senior in high school, continued at Stanford (both choir and Glee Club) and in London (High Gate Choral Society and Westminster Choral Society with concerts in Central Hall, Westminster, across the square from Westminster Abbey). Returning to Palo Alto, he sang in the Palo Alto Philharmonic Chorus, including the Carmel Summer Bach Festival during the year he worked in Monterey. At Harvard he joined the Bach Cantata Club. In Madison he sang in the choir of the Episcopal Church for many years. While in London he also learned to play the balalaika, as he occasionally demonstrated to his research group (while he sang in Russian) at social gatherings at the Ferry home.

Former students and associates have many fond memories of times spent at the Ferry home with John and his charming and vivacious wife, Barbara, probably best known for her elegant sculptures. The playing of games, including treasure hunts and charades, was a particuar attraction. The games featured highly imaginative clues that tested, sometimes mischievously, one's breadth of knowledge.

John was equally well known and appreciated for attributes other than his scientific abilities and contributions. He always had a genuine and abiding interest in and concern for all his students and collaborators. Examples of his empathy for his students' needs are shown in the following.

• When he came to the University of Wisconsin in 1946, he had students who were married veterans and were housed at a former ordinance plant 30 miles away. Their only means of transportation was by bus, with the last return run at about 5 p.m., This schedule precluded the expected lab work in the evening and night. Workarounds covering direct contact time, experimental procedures, group meetings, etc., were worked out on an individual basis.

• Howe's Point: Howe was a student who, it was mutually concluded, was not cut out for science. He did make one viscosity measurement, however, and the published paper has an asterisked point in a figure, with the comment in the legend: "This point is due to Mr. Howe." Ferry's very serious comment to his questioning coauthors was: "Well, this will be Mr. Howe's only contribution to science and it should be duly noted."

• He arranged for postdocs in his lab for both Malcolm Williams and one of us (R.F.L.) so that our wives could finish their degrees in nursing and biochemistry, respectively. He also agreed to my coming in to the lab from 3:30 to midnight so that I could care for our baby while my wife did her Ph.D. lab research work from early morning until mid afternoon. He helped Ignacio Tinoco find a postdoc in biophysics and get out early to support his wife and baby.

• In a similar vein, he did not tell one of us (J.L.S.) until *after* I had succeeded in obtaining infinite-dilution viscoelastic properties, that all of the instrumental experts he had consulted concluded that these properties definitely would *not* be obtainable.

In early 1946 John joined the faculty of the Department of Chemistry of the University of Wisconsin as an assistant professor; by 1947 he had been promoted to full professor. He served as department chair from 1959 to 1967. In 1973 he was appointed Farrington Daniels Research Professor. He was a founding member of the Rheology Research Center at Wisconsin, serving on its Executive Committee until 1984. He officially retired in 1982 but continued research until 1988, after which he continued writing scientific and historical papers until 1998. He supervised more than 50 graduate students, and more than 30 postdoctoral and foreign associates from 17 countries worked in his laboratories at Wisconsin.

Throughout his career he received many national and international awards, including membership in the National Academy of Sciences (elected, 1959), the National Academy of Engineering, and the American Academy of Arts and Sciences. Ferry was a fellow of the American Physical Society. He was honored with the Eli Lilly Award in Biological Chemistry of the American Chemical Society, the Bingham Medal of the Society of Rheology, the Kendall Award in Colloid Chemistry of the American Chemical Society, the High Polymer Physics Prize of the American Physical Society, the Colwyn Medal of the Institution of the Rubber Industry (London), the Witco Award in Polymer Chemistry of the American Chemical Society, the Technical Award of the International Institute of Synthetic Rubber Producers and the Charles Goodyear Medal of the Rubber Division of the American Chemical Society. His name is in the Rubber Hall of Fame at Akron, Ohio. Other honors conferred on him include honorary president of the 5th International Congress on Rheology (Kyoto, Japan, 1968); honorary member of the Groupe Francaise de Rheology (1972); keynote speaker and guest of honor at the New York Academy of Sciences Symposium on the Molecular Biology of Fibrinogen (1982); cochair, Gordon Research Conference on Polymer Physics (1982) and honorary member of the Japan Society of Rheology (1983).

He aided the scientific community in various capacities, as for example, the chair of the Committee on Macromolecular Chemistry of the National Research Council, president of the Society of Rheology, joint editor of the distinguished series *Advances in Polymer Science,* and as an editorial board member for five journals.

John Ferry was an extraordinary scientist who was a patient and dedicated teacher and mentor. He was admired for his encyclopedic knowledge, his ethics and absolute integrity, his ability to bring out the best in other individuals, and his linguistic abilities. In any given generation there is an occasional person who through his intellect, imagination, and ability to communicate, makes an indelible and important contribution to knowledge in his field. John Ferry was one of those people. Those of us who knew him are privileged to have interacted with him, and all of us will remember him.

NOTES

1. J. D. Ferry. Physical Chemical Studies on Polyisobutylene. Ph.D. dissertation. Stanford University, 1935.

2. J. D. Ferry. *Viscoelastic Properties of Polymers.* 1st ed. New York: John Wiley, 1961, 2nd ed., 1970, 3rd ed., 1980.

3. J. D. Ferry. Studies of the mechanical properties of substances of high molecular weight. I. A photoelastic method for study of transverse vibrations in gels. *Rev. Sci. Instrum.* 12(1941):79-82.

4. Ferry's early fibrinogen work (1942-1957) led to 41 publications. His later work (1971-1988) produced an additional 36 papers.

5. J. D. Ferry and P. R. Morrison. Preparation and properties of serum and plasma proteins. VIII. Conversion of human fibrinogen to fibrin under various conditions. *J. Am. Chem. Soc.* 69(1947):388-400. See also J. D. Ferry. The mechanism of polymerization of fibrinogen. *Proc. Natl. Acad. Sci. U. S. A.* 38(1952):566-569.

6. K. Arai and J. D. Ferry. Differential dynamic shear moduli of various carbon black-filled rubbers subjected to large step shear strain. *Rubber Chem. Technol.* 59(1986):605-614.

7. J. L. Schrag and R. M. Johnson. Application of the Birnboim multiple lumped resonator principle to viscoelastic measurements of dilute macromolecular solutions.*Rev. Sci. Instrum.* 42(1971):224-232.

8. D. J. Massa and J. L. Schrag. Computerized measurement of viscoelastic properties of macromolecular solutions: frequency dependence over an extended range of solvent viscosity. *J. Polym. Sci.* A-2 10(1972):71-87.

9. R. M. Johnson, J. L. Schrag, and J. D. Ferry. Infinite-dilution viscoelastic properties of polystyrene in Θ−solvents and good solvents. *Polym. J.* 1(1970):742-749.

10. J. L. Schrag and J. D. Ferry. Mechanical techniques for studying viscoelastic relaxation processes in polymer solutions. *Faraday Symp. Chem. Soc.* 6(1972):182-193.

11. D. J. Massa, J. L. Schrag, and J. D. Ferry. Dynamic viscoelastic properties of polystyrene in high-viscosity solvents; extrapolation to infinite dilution and high-frequency behavior. *Macromolecules* 4(1971):210-214.

12. K. Osaki and J. L. Schrag. Viscoelastic properties of polymer solutions in high-viscosity solvents and limiting high-frequency behavior. I. Polystyrene and poly-α−methyl styrene. *Polym. J.* 2(1971):541-549.

13. R. W. Rosser, J. L. Schrag, J. D. Ferry and M. Greaser. Viscoelastic properties of very dilute paramyosin solutions. *Macromolecules* 10(1977):978-980.

14. T. L. Smith, J. D. Ferry and F. W. Schremp. Measurements of the mechanical properties of polymer solutions by electromagnetic transducers. *J. Appl. Phys.* 20(1949):144-153.

15. R. S. Marvin, E. R. Fitzgerald and J. D. Ferry. Measurements of mechanical properties of polyisobutylene at audiofrequencies by a twin transducer. *J. Appl. Phys.* 21(1950):197-203.

16. E. R. Fitzgerald and J. D. Ferry. Method for determining the dynamic mechanical behavior of gels and solids at audifrequencies: comparison of mechanical and electrical properties. *J. Coll. Sci.* 8(1953):1-34.

17. D. J. Plazek, M. N. Vranken, and J. W. Berge. A torsion pendulum for dynamic and creep measurements on soft viscoelastic materials. *Trans. Soc. Rheol.* 2(1958):39-51.

18. M. H. Birnboim and J. D. Ferry. Method for measuring dynamic mechanical properties of viscoelastic liquids and gels: the gelation of polyvinyl chloride. *J. Appl. Phys.* 32(1961):2305-2313.

19. Personal interview. University of Wisconsin Oral History Project, 1985.

SELECTED BIBLIOGRAPHY

1936

With G. S. Parks. Studies on glass. XIII. Glass formation by a hydrocarbon polymer. *J. Chem. Phys.* 4:70-75.

1944

With P. R. Morrison. Chemical, clinical, and immunological studies on the products of human plasma fractionation. XVI. Fibrin clots, fibrin films, and fibrinogen plastics. *J. Clin. Invest.* 23:566-572.

1950

Mechanical properties of substances of high molecular weight. VI. Dispersion in concentrated polymer solutions and its dependence on temperature and concentration. *J. Am. Chem. Soc.* 72:3746-3752.

1951

With S. Shulman. The conversion of fibrinogen to fibrin. III. Sedimentation and viscosity studies on clotting systems inhibited by hexamethylene glycol. *J. Phys. Coll. Chem.* 55:135-144.

1952

The mechanism of polymerization of fibrinogen. *Proc. Natl. Acad. Sci. U. S. A.* 38:566-569.

1953

With S. Shulman and I. Tinoco Jr. The conversion of fibrinogen to fibrin. XII. The influence of pH, ionic strength, and hexamethylene glycol concentration on the polymerization of fibrinogen. *Arch. Biochem. Biophys.* 42:245-256.

With E. R. Fitzgerald and L. D. Grandine Jr. Dynamic mechanical properties of polyisobutylene. *J. Appl. Phys.* 24:650-655.

1955

With R. F. Landel and M. L. Williams. Extension of the Rouse theory of viscoelastic properties to undiluted linear polymers. *J. Appl. Phys.* 26:359-362.

With M. L. Williams and R. F. Landel. The temperature dependence of relaxation mechanisms in amorphous polymers and other glass-forming liquids. *J. Am. Chem. Soc.* 77:3701-3707.

1956

With R. F. Landel. Molecular friction coefficients in polymers and their temperature dependence. *Kolloid-Z.* 148:1-6.

1961

Viscoelastic Properties of Polymers. 1st ed. New York: John Wiley, 2nd ed., 1970, 3rd ed., 1980.

1963

With A. J. Kovacs and R. A. Stratton. Dynamic mechanical properties of polyvinyl acetate in shear in the glass transition temperature range. *J. Phys. Chem.* 67:152-161.

With K. Ninomiya. Phenomenological relations for the viscoelastic properties of polymer blends of different molecular weight species. *J. Coll. Sci.* 18:421-432.

1968

With S. P. Chen. The diffusion of radioactively tagged n-hexadecane and n-dodecane through rubbery polymers—effects of temperature, cross-linking, and chemical structure. *Macromolecules* 1:270-278.

With N. Langley. Dynamic mechanical properties of cross-linked rubbers. VI. Poly (dimethyl siloxane) networks. *Macromolecules* 1:353-358.

1970

With R. M. Johnson and J. L. Schrag. Infinite-dilution viscoelastic properties of polystyrene in Θ-solvents and good solvents. *Polym. J.* 1:742-749.

1971

With D. J. Massa and J. L. Schrag. Dynamic viscoelastic properties of polystyrene in high-viscosity solvents: Extrapolation to infinite dilution and high-frequency behavior. *Macromolecules* 4:210-214.

1973

With T. C. Warren and J. L. Schrag. Infinite-dilution viscoelastic properties of poly-γ-benzyl-L-glutamate in helicogenic solvents. *Biopolymers* 12:1905-1915.

1974

With O. Kramer, R. L. Carpenter, and V. Ty. Entanglement networks of 1,2-polybutadiene cross-linked in states of strain. I. Cross-linking at 0° C. *Macromolecules* 7:79-84.

With O. Kramer, R. Greco, and R. A. Neira. Rubber networks containing unattached macromolecules. I. Linear viscoelastic properties of the system butyl rubber-polyisobutylene. *J. Polym. Sci., Polym. Phys. Ed.* 12:2361-2374.

1975

With N. Nemoto, J. L. Schrag, and R. W. Fulton. Infinite-dilution viscoelastic properties of tobacco mosaic virus. *Biopolymers* 14:409-417.

1977

With R. W. Rosser, J. L. Schrag, and M. Greaser. Viscoelastic properties of very dilute paramyosin solutions. *Macromolecules* 10:978-980.

1978

With H-C. Kan. Interpretation of deviations from neo-Hookean elasticity by a two-network model with cross-links and trapped entanglements. *Rubber Chem. Tech.* 51:731-737.

1979

With C. R. Taylor. Nonlinear stress relaxation of polyisobutylene in simple extension and recovery after partial relaxation. *J. Rheol.* 23:533-542.

1982

With F. J. Roska, J. S. Lin, and J. W. Anderegg. Studies of fibrin film. II. Small-angle X-ray scattering. *Biopolymers* 21:1833-1845.

1983

With S. Granick. Entangled chain structure trapped in a styrene-butadiene random copolymer by cross-linking in simple extension. *Macromolecules* 16:39-45.

1986

With K. Arai. Differential dynamic shear moduli of carbon black-filled styrene-butadiene rubber subjected to large shear strain histories. *Rubber Chem. Tech.* 59:241-254.

With G. Schindlauer and M. Bale. Interaction of fibrinogen-binding tetrapeptides with fibrin oligomers and fine fibrin clots. *Biopolymers* 25:1315-1336.

GEORGE MCCLELLAND FOSTER JR.

October 9, 1913–May 18, 2006

BY ROBERT V. KEMPER

GEORGE MCCLELLAND FOSTER JR. was one of the most influential leaders of American anthropology in the 20th century. Going far beyond his graduate training at the University of California in the 1930s, he became widely known for his pioneering contributions to medical anthropology and applied anthropology, his brilliant comparative analyses of peasant communities (especially his works on the "Image of Limited Good" and the "Dyadic Contract"), and his commitment to long-term research in the community of Tzintzuntzan, Michoacán, Mexico.

In reflecting on his life, Foster declared, "I think chance has been the *leitmotif* of my whole life" (2000, p. 40). He repeatedly turned "chance" into serendipity, which led in turn to innovative explanations about such widespread features of the human condition as the envy of others; the linkages between individuals, groups, and communities; the impact of technology on society and culture; and the tendency to resist change. The quantity, quality, and long-term value of his scholarly work led to his election to the National Academy of Sciences in 1976 and the awarding of numerous other honors, both before and after his retirement in 1979 as professor emeritus at the University of California, Berkeley.

Foster's leadership extended beyond his scholarly work. A natural leader, he served as director of the Institute of Social Anthropology, Smithsonian Institution; director of the Anthropology Museum at the University of California; chair of the Department of Anthropology at Berkeley; principal investigator of three consecutive five-year training grants from the National Institute of General Medical Sciences that combined to support more than 100 Berkeley graduate students; and president of the American Anthropological Association during the time of a great professional crisis associated with the Vietnam War.

Foster's legacy is not a single theory or a narrowly constructed model. Far from it. Well anchored at his research base in Tzintzuntzan, he voyaged throughout the world to fulfill professional consultations and to enjoy travel adventures with his extended family. All of these experiences provided significant data for answering the many questions that inspired his anthropological work for more than 70 years.

FOSTER'S FAMILY AND HIS EARLY YEARS

Foster was born on October 9, 1913, in Sioux Falls, South Dakota, where his father ran the newest of the family's meatpacking plants. In 1878 his grandfather, Thomas Dove Foster (born in Bradford, England, in 1847), had traveled to Ottumwa, Iowa, where he built the first packing house for the Morrell Company. Foster's father (George McClelland Foster, born in 1887) trained in engineering for two years at the University of Pennsylvania and spent a year working at General Electric in Schenectady, New York, before returning to work for the family meatpacking business. Everyone took it for granted that Foster Jr. would follow this same career path.

In 1922 Foster's family returned to Ottumwa, strategically located 280 miles west of Chicago along what then was called the Burlington Railway. He was the oldest child, followed by two brothers, Bob (Robert Morrell Foster, born 1916) and Gene (Eugene Moore Foster, born 1921), and sister Janet (Mrs. Thorndike Saville Jr., born 1927). In Ottumwa Foster was raised in a "pretty well-to-do" and staunchly conservative Republican household, attended Presbyterian church services (which gave him severe headaches because the sermons were so boring), and grew up assuming that he would attend college. He was sufficiently good at school that he rarely had homework. He joined the Boy Scouts at age 12, and completed the requirements to become an Eagle Scout at age 17. He had his first foreign travel experience in Puerto Rico in 1927, when at age 13, he and his younger brother, Bob, traveled by train from Iowa to New York, where they stayed at the Vanderbilt Hotel. Then, accompanied by a cousin and an adult chaperone, they traveled for four days by the *San Lorenzo* of the New York and Porto Rico Steamship Company to reach San Juan, where they visited relatives who ran a sugar plantation.

His early family travels—to Massachusetts, to Minnesota, to Mackinac Island in Michigan, to Estes Park in Colorado—gave Foster a lifelong desire to travel the world. These trips also inspired his interest in transportation itself—in knowing all about trains, boats, and planes (cf. Foster, 1985).[1] Early on, he began to collect train timetables and shipping schedules, and later would walk through airports gathering up schedules at every airline ticket counter. He saved these items in shoeboxes as others saved baseball cards. Decades later his train timetables were donated to the DeGolyer Library at Southern Methodist University, his airline schedules to

Northwestern University, and his collection of materials about ships and boats to the Maritime Museum of San Diego. His love of travel and transportation was typical of his life; what began as a hobby turned into expert knowledge and then into stewardship and philanthropy.

THE COLLEGE YEARS: FROM HARVARD TO HERSKOVITS

Expected to follow his father's career in engineering, Foster entered Harvard in 1931. Looking back on that year, he felt that he suffered a serious case of culture shock and depression in trying to make the leap from a small town in Iowa to a major university. After a year he transferred to Northwestern. He was much nearer to home, and even was given a car—his mother's old Essex—as a birthday present in the fall of his sophomore year so that he could travel home every six weeks or so. While his spirits were buoyed, his grades in engineering courses continued to sink. Abandoning engineering, Foster sought refuge in history, but did not find it a good fit. Looking back on his failure in engineering, he felt that he couldn't live up to his father's example: "I had to get out into a completely different field where I didn't have anyone I had to equal or come close to" (2000, p. 32).

In the spring of his junior year Foster took a friend's advice and registered for introductory anthropology. Taught by Melville Herskovits, then the only anthropologist on the Northwestern University faculty, that class introduced Foster to cultures and peoples far beyond his familiar world. He loved it. Not only was Herskovits a top-notch teacher, the class was made more attractive by the presence of a sophomore named Mary LeCron (known as Mickie).[2] Foster recalls that he was smitten from that moment, while she didn't even know that he was there. In this way, anthropology not only became his life's work but also introduced him to the love of his life, Mickie. Foster followed up his initial encounter with anthropology by taking a long trip to China and Japan in the summer of 1933. A highlight of the trip was his ascent and descent of Mount Fujiyama.

In the fall of 1933 Herskovits convinced both Foster and Mickie to take honors degrees, including comprehensive written and oral examinations. Lacking graduate students, Herskovits mentored the two with the goal of preparing them for graduate work. When well-known anthropologists (such as Bronislaw Malinowski) came to the Chicago area, he arranged gatherings in his home, to which Foster and Mickie were invited. In this way Foster was brought into contact with professional anthropologists beginning in his undergraduate days. Years later the Fosters' elegant, architect-designed home in the Berkeley hills, with its panoramic view of San Francisco, the Golden Gate Bridge, and Mount Tamalpais, became a focal point for gatherings of anthropology faculty and students alike.

GRADUATE STUDIES AT BERKELEY: 1935-1941

In the spring of 1935 Foster wrote to Alfred L. Kroeber, one of the leading anthropologists in the country, to inquire about pursuing graduate study in anthropology at the University of California. In his role as acting chair of the Department of Anthropology, Robert H. Lowie wrote back to Foster and urged him not to consider coming out to Berkeley, saying, in effect, that there were no jobs, and it was a just dead-end field (2000, p. 48). Foster persisted, so Lowie grudgingly accepted Foster into the program, along with a handful of other applicants (the best known of whom was Walter Goldschmidt, later professor of anthropology at UCLA).

Foster arrived in Berkeley in mid-August 1935 to begin his graduate studies. He found himself literally at the frontier of anthropology, isolated by days of train travel from the major centers of anthropological studies located at universities, museums, and government institutions east of the Mississippi. At that time the Department of Anthropology at the University of California consisted of Alfred L. Kroeber (then age 60) and Robert H. Lowie (age 53)—who were jointly responsible for offering graduate seminars—and instructors Ronald Olson and Edward Gifford.

In his extended recollections of his graduate studies at Berkeley, Foster observed:

> As a group we were tremendously supportive of each other. It occurred to no one to conceal ideas or data, and it astonished me when I returned to Berkeley many years later to find that anthropology was regarded by many graduate students as a limited good, so that one had to be cautious in discussing data and ideas with fellow students and faculty lest they be "stolen" (1976, pp. 15-16).

During the 1935-1936 academic year Mickie remained at Northwestern to finish her senior year. Meanwhile, Foster dated other women and she other men. He was briefly engaged in the spring of 1936 to another woman, but broke it off. With the distance Foster and Mickie were drifting apart, so much so that she went east to Columbia University for graduate study in anthropology. During the five-week Christmas break of 1936, he took a solo trip to Mexico. Traveling by train, without any knowledge of Spanish or even a dictionary in hand, Foster went to Mexico City, Oaxaca, Veracruz, and Tabasco, returning through Guadalajara and up the west coast. Back to Berkeley, he told Kroeber that he was set on specializing in Mexico. Just as significant, after returning from Mexico he sent to Mickie some presents acquired during his trip. This initiative proved successful, and they began writing back and forth.

During the summer of 1937, Foster was given $200 by Kroeber and sent to Round Valley in northern California to study the Yuki culture. Foster recounts that when Kroeber told him that he was to go to the Yuki,

> I had more than a few doubts as to how to go about it. "Professor Kroeber," I asked, "can't you give me some advice about fieldwork?" His eyes twinkled, he paused a moment, and then said, "I suggest you get a stenographer's notebook and a pencil." Then he marched on down the hall (1976, p. 17).

Looking back on that initial foray into the field, Foster saw it as a test that he survived. He was discouraged at first, and told Kroeber that he couldn't do it. Kroeber told him to go back and finish the summer—and Foster did. Soon after, he wrote *A Summary of Yuki Culture* that eventually appeared in the University of California *Anthropological Records* series (1944).

In September 1937 Mickie came to Berkeley, where she had relatives, and renewed her relationship with Foster. Although she arrived too late to register for fall semester courses, she remembers that Foster arranged for her to audit some anthropology courses (M. Foster, 2001, p. 116).[3] They were married on January 6, 1938, in Washington, D.C., where her Democrat father was employed in the Department of Agriculture. His parents came from Iowa to attend the private marriage ceremony held at her home.

The next day, the newlyweds took the Cunard liner *Scythia* to Liverpool, traveled through London, where they attended one of Malinowski's seminars at the London School of Economics, and then onward to Vienna, where they remained until May 24 (and thus were present during the *Anschluss*). During the summer Mickie's father sent over a new car by ship so that they could drive around Europe. They traveled through Scandinavia and then attended the International

Congress of Anthropological and Ethnological Sciences in Copenhagen, where they visited with Herskovits, Malinowski, and other famous anthropologists, before continuing on to Paris. They remained there, studying French (which Mickie already knew well from her high school year abroad in Grenoble), until November 11, when they embarked on the SS *Veendam* to return to New York.

While he was learning French and German in Europe, Foster became intrigued with cultural differences. Reflecting on that experience, he commented that his ideas on envy began when he realized that

> the word for "tip" in German is *Trinkgeld* and the word for "tip" in French is *pourboire* and then a "tip" in English clearly comes from the word "tipple." I didn't do anything with those ideas for another thirty years, but they were basic in the work that I did on the anatomy of envy (2000, p. 70).

Foster and Mickie returned to Washington, D.C., in November 1938 and then traveled to California, where their son Jeremy was born in March 1939. Mickie tells the story that while she and the baby were still at Peralta Hospital in Oakland, George came to see her. When she asked him if he had gone to see the baby through the nursery window, he replied, "No." When she asked, "Why not?" he said,

> Because the other babies are so puny and small and red, and don't look attractive, and if I ask for my baby—he's so big and so beautiful, I think they'll feel badly and be envious, so that I don't want to put them though that (M. Foster 2001, p. 121).

In this intimate family experience one can see elements of what later became Foster's famous theories about envy and the "Image of Limited Good."

Once again installed in Berkeley, Foster renewed his studies with the aim of preparing for the comprehensive written and oral examinations in the fall of 1939. The "writtens" were especially grueling—30 hours of essays spread over five days. After passing that portion of the examination the orals were more or less a formality. Foster's committee included Kroeber and Lowie from within the department and three outsiders: geographer Carl Sauer, historian Herbert Bolton, and economist Frank Knight.

Having passed his exams, Foster still faced the hurdle of mastering Spanish. He determined that while Mickie and baby Jeremy stayed with her parents in Washington, D.C., he would drive to Mexico. He left on the third of January 1940 and arrived in Mexico City on the 13th. There he made contact with friends whom he had met on his earlier trips to China and Mexico, and then connected with their friends in turn. Leaving one-year-old Jeremy with her parents, Mickie came down in March and stayed six weeks before returning to Washington, D.C.

Foster found that Mexican anthropologists proved to be invaluable friends and guides through the maze of Mexican government agencies, opening up doors that Foster did not even know existed. He especially came to depend on his connections with Irmgard Weitlaner and her engineer father, Roberto, who introduced Foster to the Sierra Popoluca of Veracruz in the spring of 1940.

Foster met Isabel Kelly, a Berkeley Ph.D. who had done the first systematic archaeological research in western Mexico but had moved to Mexico City by 1940. He also met Donald and Dorothy Cordry, Miguel Covarrubias, Wigberto Jiménez Moreno, and Frances Toor. This early experience in building social networks with local anthropologists provided Foster with an important lesson that he passed on to his students for decades to come.

In April 1940 Foster drove back to Ottumwa in just four days, took a train to Washington, D.C., returned by train to Ottumwa, and then drove out to Berkeley, arriving in mid-June. He spent several months studying Spanish and revising his study of Yuki culture for publication. In November he again drove alone to Mexico. In January 1941, having arranged for Jeremy to stay with Foster's mother in Ottumwa, Mickie joined him in Mexico. Now they were ready to carry out fieldwork among the Sierra Popoluca. With Mickie's help, Foster spent three months gathering information on economics and linguistics in the town of Soteapan. Returning to Berkeley (via Ottumwa, where they picked up little Jeremy), he quickly wrote up a slim dissertation (published in 1942 as *A Primitive Mexican Economy* in the monograph series of the American Ethnological Society).

Following the Berkeley system established by Kroeber and Lowie, the dissertation was not intended to be a magnum opus but simply a progress report on the student's development, the last in a long series of exercises. As a result Foster later told his own students to write short dissertations—with 200 pages usually being more than adequate to the need. Having learned the skill of concise writing from Kroeber, Foster passed it along to his own students. In fact, many Berkeley students asked him to serve on their dissertation committees not so much for his ethnographic knowledge or theoretical insights, but because he was willing to spend time working with them on their writing. The experience of Eugene Hammel is typical of what so many students encountered when they handed in a dissertation draft to Foster. He recalls that he gave a copy to Foster on a Friday and got it back, thoroughly marked up, on Monday:

> He called me in, handed it back, and told me to start over, giving me a list of suggestions. I spent a solid week rewriting and gave the revisions to Foster on a Friday. On Monday he called me in . . . , and this process was repeated several times. . . this anecdote [illustrates] Foster's complete dedication to his task . . . and the promptness of his response (Hammel, 2000, p. v).

FINDING WORK AS AN ANTHROPOLOGIST

In 1941, as Lowie had warned him, jobs were scarce in American anthropology, and Foster had no prospects. Then, in September Kroeber asked him if he were willing to take a one-year job teaching sociology and anthropology at Syracuse University. Foster agreed, and almost immediately took a United DC-3 sleeper plane to Chicago and then an American DC-3 on to Syracuse (with stops in Detroit, Buffalo, and Rochester). Mickie and their son Jeremy followed later, after she arranged to rent out the small house they had bought on LeRoy Avenue (which they owned until 1946). Although Foster never had taken any courses in sociology, he found himself teaching three sections of introductory sociology: Monday, Wednesday, and Friday at 8:00 a.m., 9:00 a.m., and 11:00 a.m. plus an anthropology course on Tuesday, Thursday, and Saturday at 8:00 a.m.

During the spring term he received a letter from Ralph Beals (also a Berkeley Ph.D.) asking if Foster were interested in coming to UCLA to replace him for the 1942-1943 academic year, while Beals went to Washington, D.C., to work with Julian Steward on the project for a *Handbook of South American Indians.* Even though the growing Foster family (daughter Melissa was born while they were in New York) was enjoying the year in upstate New York, he was happy to return to the west coast. At the end of the summer in 1942 the Fosters returned to California, where they stayed in the Beals house in Santa Monica for the following academic year. At UCLA Foster taught a cut-down version of Kroeber's famous course on culture, plus courses on world ethnology, general anthropology, and social organization.

With the war in full gear Foster assumed that he would be drafted into military service when his year at UCLA came to an end. Instead, the Berkeley draft board classified him as

4-F because of allergies, and soon thereafter he was invited to join the staff of Nelson Rockefeller's new Institute of Inter-American Affairs in Washington, D.C. That experience set the course for the rest of his life. In his role as a social science analyst, Foster was taking his first steps along the path of applied anthropology. He realized that this was not a field that anthropologists generally followed:

> In fact, we were trained to despise applied anthropology. The war had the positive effect of making American anthropologists aware of the possibilities. The Society for Applied Anthropology, for example, was established in, I believe, 1942, and it has been a vigorous organization ever since. But I didn't join until about 1950 (2000, p. 120).

Even in those early days of his career, in the dark days of World War II, Foster made the most of the opportunities that came his way. In the 10 years between 1943 and 1952 he went from "despising applied anthropology" to working as an advocate for anthropological research on U.S. technical aid programs in Latin America. He went from being an ethnographer in the tradition of A. L. Kroeber and Robert H. Lowie to becoming an analyst and interpreter of culture, behavior, and bureaucratic premises in contemporary societies. He did not abandon his ethnographic roots but found new ways to blend theory and practice.

THE INSTITUTE OF SOCIAL ANTHROPOLOGY IN MEXICO: TRAINING STUDENTS AND DOING FIELDWORK

Foster's transformation did not occur intentionally, but through serendipity. After his short time at the Institute of Inter-American Affairs, he was the first anthropologist hired by Julian Steward (whom Foster knew because both had been at Berkeley in the 1930s) to go to Latin America as a representative of the new Institute of Social Anthropology, created in 1942 within the Smithsonian Institution. Sent to Mexico City to train students at the Escuela Nacional de

Antropología e Historia (ENAH), Foster also was expected to take a group of students to the field—specifically, to the Tarascan region in the State of Michoacán (the home of the former president of Mexico, Lázaro Cárdenas). After a bumpy start in the Tarascan village of Ihuatzio, Foster, his assistant Gabriel Ospina, and several students moved their project over the hill to the *mestizo* town of Tzintzuntzan ("the place of the hummingbirds"), 400 years earlier the capital of the Tarascan empire. At that time in 1945-1946 Foster had no idea that his long-term ethnographic work would enhance Tzintzuntzan's fame, or that Tzintzuntzan would provide him with the source of some of his best ideas—especially the "Image of Limited Good" and the "Dyadic Contract."

Reflecting on his now-classic monograph, *Empire's Children: The People of Tzintzuntzan* (1948), Foster recalled that "we were just interested in doing a basic community study. Word pictures of the way of life, the people, all aspects, as many aspects as we could deal with" (2000, p. 135). After leaving Mexico in 1946 he did not return to Tzintzuntzan until 1958, when his long-term study of the community began in earnest. Only in the 1960s and thereafter did Foster develop theoretical models to explain the impact of external forces on the community's culture.

WASHINGTON, D.C.: DIRECTING THE INSTITUTE OF SOCIAL ANTHROPOLOGY

In the summer of 1946 after convincing his colleague Isabel Kelly to replace him as head of the Institute for Social Anthropology (ISA) program in Mexico, Foster went to Washington, D.C., where he took over the ISA from Steward, who was leaving to become a professor at Columbia University. In this new role as a government administrator and bureaucrat Foster recognized the need to learn more about ISA's programs in Latin America. On February 1, 1947, he went on his first ISA trip to South America. In Ecuador he visited Aníbal Buitrón; in Peru, Alan Holmberg and George McBryde; and in Colombia, John Rowe and Gregorio Hernández de Alba. On a second trip, lasting from February 14 to April 11, 1948, Foster traveled to Colombia, where he again saw Rowe and Hernández de Alba; to Ecuador and Peru, where he saw Holmberg and Jorge Muelle; and to Bolivia and Brazil, where he visited Donald Pierson and Kalervo Oberg.

SPAIN: STUDYING THE ROOTS OF LATIN AMERICAN ACCULTURATION

In 1949-1950 Foster took a leave of absence from the ISA and, with a Guggenheim Fellowship, went to Spain to carry out a detailed study of the Spanish roots of Spanish American culture. He and Mickie (with a Plymouth sedan) went to Spain on March 4 on the Italian Line MV *Vulcania* and returned on the same vessel, arriving in New York City on May 14. After spending the summer vacationing with family, Foster, Mickie, and their children (with a Pontiac station wagon) sailed to Spain on September 6 on American Export Lines SS *Excambion*. One year later, on September 19, they arrived back in New York on the MV *Saturnia*, although they were delayed one day en route by a storm with 100-125 mph winds.

While in Spain, Foster benefited substantially from his work and friendship with the anthropologist Julio Caro Baroja. Together they drove 25,000 miles in Spain, from Jeréz de la Frontera in the southwest to Barcelona in the northeast. In addition, he journeyed to the Baleares, where he studied the *feixas* (irrigation channels) on Ibiza. In his travels Foster emphasized the regions of Extremadura and Andalusia, the places best known for sending conquistadors and emigrants to the New World. Through his ethnographic, ethnohistorical, and library research Foster found that the *time sequence* for the introduction of cultural traits to the Americas was more important than their places of origin.

This ethnographic and ethnohistorical survey of Spain served as the basis of his well-known book, *Culture and Conquest: America's Spanish Heritage* (initially rejected without explanation by the University of California Press, but finally published by the Wenner-Gren Foundation in 1960 and subsequently translated into Spanish in 1962 and again in 2003). In this masterpiece of cultural history and synthesis Foster presented his important concept of "conquest culture," which he defined as

> the totality of donor influences, whatever their origin, that are brought to bear on a recipient culture, the channel whereby the dominant ways, values, and attitudes are transmitted to the weaker. . . The formation of a conquest culture is characterized by a "stripping down" or "reduction" process in which large numbers of elements of the donor culture are eliminated and the complexity and variety of many configurations become simplified (1960, p. 12).

What Foster had learned from Herskovits about acculturation at Northwestern in the 1930s was proudly displayed in *Culture and Conquest*, arguably the last great ethnographic study based on the acculturation framework.

RECONFIGURING THE INSTITUTE OF SOCIAL ANTHROPOLOGY: TOWARD PUBLIC HEALTH

When Foster returned to Washington from Spain, he realized that the days of the ISA were numbered. He made his third (and final) ISA trip to Latin America between March 3 and March 28, 1951. He saw Richard Adams in Guatemala, Charles Erasmus and Luis Duque Gomez in Colombia, and Ozzie Simmons and Muelle in Peru. Upon returning to the U.S., Foster made the strategic decision to attempt to save the jobs of the anthropologists working in the ISA program by shifting their focus from research and training to the evaluation of U.S. technical aid programs in Latin America. Given his earlier experience with the Institute of Inter-American Affairs, Foster determined that of its three main divisions (agriculture, education, and health), only the area of health held much prospect for success.

So Foster went across the Mall to see the acting head of the Institute of Inter-American Affair's Health Division. After some discussion they agreed that the ISA anthropologists would work on the institute's health programs and the cultural problems being encountered in several countries. Foster sent instructions to Kelly in Mexico, Adams in Guatemala, Erasmus in Colombia, Simmons in Peru, and Oberg in Brazil. After a couple of months they sent their notes to Foster, who assembled a 104-page mimeographed report titled *A Cross-Cultural Anthropological Analysis of a Technical Aid Program* (1951). His work at integrating the disparate information provided by his colleagues demonstrated his rare skill at classification and explanation, hard-won in Kroeber's seminars at the University of California 15 years earlier.

According to Foster, "This paper was, you might say, a bombshell" (2000, p. 159). The promise of the anthropological approach was so compelling that Henry van Zile Hyde, head of the institute's Health Division, agreed to hire all of the ISA field staff for the coming year if they would focus their attention on the U.S.-sponsored public health programs in their countries.

In June 1952 a one-week conference was held in Washington to discuss the anthropologists' work. According to Foster,

> And on one day, I presented our findings. That was one of the great days of my life and a great day, I think, for public health, too. You've never seen such enthusiasm. We were able to explain a lot of things that the public health personnel had been knocking their heads about (2000, p. 160).

In reconfiguring the Institute of Social Anthropology, Foster transformed his own vision of the world. Always the ethnographer, he had learned how bureaucracies had their own cultures and what later he would call their own "implicit premises" (1969,1, pp. 90-113). His experiences in working on public health programs with the Institute of Inter-American Affairs in 1951-1952 might have pointed him toward a permanent position in government circles, but he realized that at age 39 he had to make a choice between a government career and the academic life. Since his wife's parents had retired from Washington to Berkeley in 1943, Foster, Mickie, and their two children often traveled there on family vacations. Thus, Foster had been able to stay in contact with the Berkeley Department of Anthropology, even though he was a continent away.

BERKELEY: LEADING, TEACHING, AND TRAINING, 1953-1979

Foster returned to Berkeley in 1953 as a visiting lecturer, hoping to land a permanent job initially designed to be split one-third and two-thirds between public health and anthropology, respectively, but the arrangement never came to fruition. Fortunately, it happened that Gifford retired soon after Foster's arrival, thus creating a need for a new director of the Museum of Anthropology. Foster was appointed into Gifford's position. In that role he soon found himself serving as liaison with the architects hired to build a new building, in which the museum would be housed along with the departments of anthropology and of art. After a three-year stint as acting director at the museum, Foster moved into the Department of Anthropology on a full-time tenured appointment in 1955. He served as chair of the department during 1958-1961 and then again from 1973 to 1974.

During his years at Berkeley, Foster taught many different courses, ranging from regional surveys of "Europe and the Mediterranean" and "Latin American Culture" to his famous "Anthropology and Modern Life" (later called "Applied Anthropology"). In the fall term of 1968 I served as his teaching assistant for the applied anthropology course. I remember that it had well over 100 students, many of them graduate students in public health, education, social welfare, and architecture. We all had to rise early to be on time for this 8:00 a.m. class, where with missionary zeal Foster blended case studies and theories to teach us about culture and the "implicit premises" of peoples, professions, and bureaucracies. With his bow tie in place and his lecture notes typed out on 5×8-inch sheets, he presented a serious and formidable figure in the classroom. Yet, he was passionate about enlightening students to an anthropological way of seeing and understanding the world. According to Foster, that was the most successful course that he ever gave:

> I wrote two books on the basis of my lectures, *Traditional Cultures and the Impact of Technological Change* (1962) and then, of course, I had to rewrite my lectures. That gave rise to *Applied Anthropology*, after which I had to rewrite my lectures once more. And that resulted in the revised edition of *Traditional Cultures* that appeared in 1972, under the mercifully shortened title of *Traditional Societies and Technological Change* (2000, p. 201).

Among his many successful and critically acclaimed monographs and textbooks, his *Traditional Cultures* book was the best-selling, with well over 100,000 copies sold in English. It also was translated into Spanish, Portuguese, Dutch, and Farsi.

Remembering his own struggles in the Berkeley graduate program two decades earlier, Foster set about the enormous task of eliminating what no longer worked and introducing new elements as needed. In this effort he used the skills developed earlier at the ISA. He talked with and listened to the faculty, the staff, and the students, not only about the intellectual rigor of the program but also the sense of community that it might create. Eugene Hammel gives Foster credit for being the "architect of the modern Ph.D. program at Berkeley," for being "instrumental in developing the plans for Kroeber Hall in the 1950s, especially concerning the inclusion of the museum," and for beginning the "democratization of the department" (2000, p. iv-vi).

Aware of the damage done by Kroeber's "sink or swim" approach to training graduate students for field research, Foster was convinced that the graduate program should include courses on research methods and should encourage students to get into supervised field situations prior to attempting their dissertation work. He was convinced that predissertation fieldwork was vital to designing the most effective dissertation research projects, a conviction that would result in two major endowments at the end of his life.

Foster was the first of the Berkeley faculty to take untested students into the heartland of his own research—Tzintzuntzan and the adjacent towns and villages—where they could learn the basics of fieldwork under his supervision. This was not an annual "field school" in the way that Evon Vogt maintained the Harvard Chiapas Project as a field experience for Harvard graduate and undergraduate students (Vogt, 2002). Foster took students to the field only from time to time, according to who was willing to commit to his three-part process.

In 1967, for example, four graduate students went to the Tzintzuntzan area. Stanley Brandes lived in the poor barrio of Yahuaro in Tzintzuntzan, while Ron Maduro went to Santa Fe de la Laguna, a pottery-making village across the lake. Melody Trott studied middle-class teenagers in the regional market town, Pátzcuaro, located about 15 km south of Tzintzuntzan, and I (and a Mexican anthropology student, Francisco Ríos, from the ENAH) worked on a restudy of Pátzcuaro's marketplace, which Foster had studied two decades earlier. Except for Ríos we all prepared for our fieldwork in a spring quarter seminar and then analyzed our data and presented our findings in a follow-up seminar in the fall quarter. Rare 40 years ago, Foster's extended approach to the anthropological fieldwork experience is widespread today.

CONSULTING WITH THE WORLD HEALTH ORGANIZATION AND OTHER AGENCIES

Beginning with his participation in 1951-1952 on a joint U.S. Public Heath Service/Institute of Inter-American Affairs evaluation team that assessed "The First Ten Years of Bilateral Health Programs in Latin America," and concluding with a 1983 trip to participate in a WHO/EURO workshop on the "Scientific Analysis of Health Care," Foster accepted 36 international consulting assignments during his career (for a complete listing see Kemper [2006, pp. 9-10]). His retirement in 1979 hardly slowed the pace of his international travels to work with the World Health Organization and other agencies. Although he served on some administrative commissions and did a few site visits at universities, all of his applied anthropology assignments were international, ranging from Latin America to Africa and from Asia to Europe. Although many of his consulting projects were focused on public health issues, they often were labeled more broadly as "community development."

Foster was an excellent consultant who listened carefully and took a positive approach to the people who worked in the sponsoring agencies. The stories of his consultancies have appeared in numerous anthropological and interdisciplinary journals. His international experiences also created for him a global network of contacts in diverse agencies, especially in the World Health Organization and other public health agencies. Early in his career Foster recognized the importance of understanding the cultures of these "innovating organizations" rather than focusing only on the cultures of the "target group." One of his important contributions to applied anthropology was his realization that it was the "interaction setting" between change agents and the recipient peoples that determined much of the success (or failure) of development projects. This perspective is cogently presented in his *Applied Anthropology* (1969,1), generally considered to be the first textbook in the field.

LONG-TERM FIELDWORK: THE TZINTZUNTZAN COMMUNITY STUDY

With a large grant from the National Science Foundation, in 1958 Foster returned to Tzintzuntzan, where he initiated an innovative long-term study of sociocultural change, economics, personality, and health. This research resulted in many important contributions to understanding peasant life, including his oft-cited (and sometimes controversial) works on pottery making (1965,2), the "Dyadic Contract" (1961, 1963), The "Image of Limited Good" (1965,1), the *compadrazgo* (1969,2), and "hot-cold" theories of illness (1994). Foster's goal was to develop models to explain how villagers' traditional worldviews (emphasizing balance, harmony, and reciprocity) were being transformed as the national and international political economic system increasingly influenced local culture (see Rollwagen, 1992).

Anyone who observed him in the course of fieldwork could tell that his principal love was the close observation and recording of social and cultural life (2002, 1979). The people of Tzintzuntzan, who collaborated in his studies for over half a century, formed the subject of his major ethnographic corpus. He was an inveterate note taker and, relying on a classificatory scheme developed by the Human Relations Area Files, he accumulated what must count as one of the most exhaustive and detailed bodies of ethnographic writing on the widest range of subject matter in the annals of cultural anthropology.[4]

Foster's curiosity was boundless, as were his range of intellectual interests and his data collection strategies. On the one hand, he was a proponent of the vacuum-cleaner ethnographic style, which he had learned through his Kroeberian graduate school training. He believed that detailed information on every possible topic of social and cultural life should be collected and recorded with assiduous care. A main product of this approach was *Empire's Children: The People of Tzintzuntzan* (1948, Spanish translation 2000), which stands as an invaluable source of knowledge about rural life in central Mexico in the mid–20th century.[5]

On the other hand, Foster was an eminently post-Boasian anthropologist, a problem-oriented researcher who constantly asked questions of his data and knew how to probe a topic until he understood thoroughly the way the local people thought about it. He was interested in assessing the range of opinions and knowledge that might be expressed on any given subject, and in determining the reasons for this variation. Because of his historical bent, his inherent interest in social dynamics, and the decades of research devoted to Tzintzuntzan, Foster made good use of his voluminous and meticulously crafted fieldwork files to determine intracultural variation in local patterns of social and cultural change. The principal product of this approach was *Tzintzuntzan: Mexican Peasants in a Changing World* (1967, Spanish translation 1972), a monograph that he believed would exert a greater impact and have more lasting value than the original Tzintzuntzan ethnography.[6]

In his last two decades of fieldwork in Tzintzuntzan Foster concentrated on the domains of health and illness. He gathered extensive ethnographic data on every illness with which the people were familiar. He used a technique of asking multiple informants about certain key issues every time he had a question. For example, he determined the "hot" and "cold" qualities of a long list of foods and related products by asking more than a dozen persons on several different visits. Eventually, Foster saw the patterns and the anomalies in the data, and came to the conclusion that rather than being immutable, the categories of hot and cold were adaptable to the empirical medical circumstances of individuals.

Reflecting on more than 50 years of research in Tzintzuntzan, Foster emphasized the importance of serendipity and "trigger mechanisms" (what others might call insight) as critical features of long-term research.[7] As in so much of his own life and career he felt that many of his best ideas came not through careful design but from good luck and persistence. In the end he argued,

> Theories come and go but good data are timeless, grist for the anthropologist's mill when least expected. And, clearly, one of the advantages of repeated visits to a research site is that, as our data accumulate and we have time to ask questions about their meanings and their anomalies, we can write with confidence on theoretical matters. (2002, p. 266).

In reflecting on how our theories and models are subject to unforeseen factors, including the passage of time, Foster reached the conclusion that had he initiated his fieldwork in Tzintzuntzan in, say, 1970, the "Image of Limited Good" model might never have occurred to him. Moreover, he felt that

> It would be entirely possible for young anthropologists to study Tzintzuntzan today, search for evidence of Limited Good, and, on the basis of their findings, argue that the Limited Good hypothesis is inappropriate. . . But such an argument, because of its lack of time depth, in no way destroys the model. It merely confirms what we already know: worldviews can and do change (2002, p. 267).

Foster's numerous visits—at least yearly and often more frequent—to Tzintzuntzan were made much more enjoyable and ethnographically fruitful by his good fortune to live with the family of Doña Micaela González from 1959 until the new millennium. In the first 10 years he and Mickie occupied a small room on the ground floor. Then, in 1968 he had the idea to build a second floor containing a spacious bedroom, a study, and a patio. Not only did this give him more privacy but he also gained wonderful views of the lake to the north, the church tower to the south, and the *yácatas* (pyramids) to the east. In recent years these views have been greatly diminished by the construction of second-floor rooms above nearly all of the nearby houses—through economic prosperity linked to emigrant remittances.

The Fosters' relationship with Micaela's family was based on mutual respect and reciprocity. Foster loved to celebrate birthdays and anniversaries among his "family" in Tzintzuntzan. In a letter sent to me on May 9, 1991, Foster wrote,

> Jeremy's 20-year old daughter, Emily, is planning to go to Tzintzuntzan in August, probably only a short visit. It pleases me that my children and grandchildren feel as much at home there as in other places they visit. I wonder how many other cases there are where grandchildren of the original investigator view the community in that light?

Foster brought Micaela (born May 8, 1906; died July 1, 2000),[8] her two unmarried daughters (Lola, born May 24, 1929, and Virginia, born April 8, 1934), and their coresident friend (María Flores, born August 6, 1937) to the United States on several occasions. These "ladies" (as they have come to be known after being so called by my son John when he was a small boy) have traveled to Berkeley and throughout California, as well as to Texas, New Mexico, Colorado, Utah, Arizona, and Washington, D.C. On these trips they saw spectacular tourist venues like the Grand Canyon, Las Vegas, and Disneyland. More important were two trips to Berkeley. I brought them from Mexico to Berkeley in mid-December 2001, to be with Foster and Mickie ("Mariquita") at a time when she was losing her battle with cancer. We all were present when she died on the evening of the 14th. In early January 2006 I again took the ladies to Berkeley, where we spent three days with Foster, just a few months before he passed away.

His affiliation with the people of Tzintzuntzan was not only anthropological but also philanthropic. Over the years what he referred to as "the Foster Foundation" provided individuals and families with tens of thousands of dollars for medicines and doctors' bills, for school tuition and books, and for other pressing needs. He also regularly contributed toward the costs of sponsoring the numerous local fiestas. He was proud of receiving diplomas from the municipal council in recognition of his long-term research to make Tzintzuntzan better known in the world. In May 2006 when Dolores and Virginia learned of his death, they placed his photograph on the household altar, next to those of their mother, Doña Micaela, and Mickie Foster.

MEDICAL ANTHROPOLOGY: TURNING PRACTICE INTO THEORY

Foster's interest in public health and community development programs arose in the early 1950s while he was working in Washington, D.C., with the Institute for Social Anthropology and the Institute for Inter-American Affairs. The move from government service back to the academy in 1953 allowed him to expand this emerging area of research. Beginning with Margaret Clark, his first doctoral student, Foster directed numerous dissertations related to health and illness around the world. Upon learning about federal interest in training medically oriented behavioral scientists, he promptly submitted an ambitious grant proposal to the National Institute for General Medical Sciences (NIGMS), up to that time not a standard source for anthropological funding. Over a period of 15 years, from 1965 to 1979, the grant brought in some $3 million (equivalent to more than $15 million in 2007 dollars) and supported about 100 students in the Berkeley doctoral program. Surely, this is the largest graduate student training grant in the history of American anthropology. Without that training grant the scholarly corps for doing medical anthropology would have taken much longer to develop.

Eager to institutionalize training in medical anthropology, Foster established the joint Berkeley-UCSF Ph.D. program in 1972 and directed it until his retirement. He also coauthored the first textbook in the field with Barbara Gallatin Anderson, another of his former students, who recently had accepted a position at Southern Methodist University to develop a specialization in medical anthropology. Global in scope, *Medical Anthropology* (1978) included discussions of the origins and scope of the field; dealt with ethnomedicine, ethnopsychiatry, curers, and non-Western medical systems; examined illness behavior, hospitals, doctors, and nursing in the Western world; and considered roles for medical anthropologists, lessons learned from the past, and contemporary trends and dilemmas. The textbook concluded with provocative chapters on nutrition and bioethics, both of which have become important domains for medical anthropological work in recent decades.

RETIREMENT: TRAVELS AND FAMILY

Foster decided to retire—two years in advance of the statutory requirement—in 1979 when the funding for the third five-year cycle for his NIGMS training grant came to an end. As professor emeritus he continued for several years to keep a small office in the department and to serve on some dissertation committees. Retirement allowed him more time for social interaction with colleagues on and off campus. His routine included an off-campus "Monday Lunch Bunch" (with "the Boys"), a Faculty Club gathering with other retired anthropology professors ("the Emeriti") on Wednesdays, and an interdisciplinary group ("Little Thinkers") at the Faculty Club on Friday.

Retirement also provided the Fosters with more time to spend at Snag, their family's weekend place in Calaveras County. Snag offered Foster, Mickie, and other family members and friends time and space to relax, walk along the country lanes, go fishing in the adjacent river, go swimming in their "lake," go bird watching, pick bushels of apples from their trees, or just read from among the stacks of mostly nonfiction books. An avid angler, Foster was especially proud of a large rainbow trout—mounted on the kitchen wall—that he caught in their stretch of the river.

Their retirement years permitted the Fosters to indulge their pleasure in traveling throughout the world, especially as participants on specialized cruises on small vessels and "adventure tourism" to unusual venues. Virtually every spring and fall Foster and Mickie (sometimes accompanied by their extended family members) departed for distant lands and waters. Antarctica, Polynesia, the Indian Ocean, Micronesia, Papua New Guinea, Nepal, the Amazon, and the Caribbean were just a few of the more than 100 nations and regions they visited during a lifetime of travel adventures and consulting assignments. Even after Mickie died in December 2001 and Foster's physical mobility became more challenged by Parkinson's disease, he and his son Jeremy took a train tour through Mexico's famous Copper Canyon and made a separate trip together to Tzintzuntzan in 2004. Foster also traveled in 2002 through the Panama Canal and later to China, both times accompanied by his son-in-law Wijbrandt van Schuur (of Nijmegen, Netherlands). His final trip—in 2005—took him (with Wijbrandt, Melissa, and their daughter Klaartje) to Alaska on the Celebrity Cruise Lines *Infinity*.

FOSTER'S LEGACY IN ANTHROPOLOGY AND PHILANTHROPY

On June 16, 1979, Foster was presented with a Festschrift volume entitled *From Tzintzuntzan to the "Image of Limited Good": Essays in Honor of George M. Foster* (Clark et al., 1979), which contained congratulatory letters from numerous colleagues and former students, a dozen articles written by former students, and a comprehensive bibliography of Foster's publications from 1939 to mid-1979.[9]

He was passionate about his chosen discipline, one that in many ways defined him. Near the end of his life he declared, "I didn't choose anthropology, anthropology chose me. Anthropology and I, we were made for one another" (personal communication). Anthropology for Foster was a calling, and his enthusiasm for his chosen field never abated. He continued to write long after retirement, even publishing an analysis of his beloved cruise experiences (1985). To the end of his days anthropological journals and monographs remained at his side.

Foster's accomplishments were recognized with many honors and awards. He was elected to the National Academy of Sciences in 1976 and the American Academy of Arts and Sciences in 1980, served as president of the American Anthropological Association (AAA) during the turbulent Vietnam war years of 1969-1970, and was recognized with the AAA's Distinguished Service Award in 1980. In 1982 he received the Bronislaw Malinowski Award from the Society for Applied Anthropology (see 1982; Weaver, 2002). On his retirement he received the Berkeley Citation, the campus's highest honor, and in 1997 the Berkeley Anthropology Library was renamed in honor of the Fosters. In 2005 the Society for Medical Anthropology awarded Foster its Career Achievement Award and in the same year created the George Foster Practicing Medical Anthropology Award.

In a tribute at the time of his retirement Foster's Berkeley colleagues Eugene Hammel and Laura Nader wrote of him:

> George Foster stands as a challenge to those anthropologists who believe that specialization is incompatible with breadth of view, that scientific and applied work cannot productively be part of one career, that historical and long time association with the same community and region tends to narrow comparative insight (Hammel and Nader, 1979, p. 159).

Foster's commitment to anthropology went far beyond research, teaching, publications, and service. Inheriting considerable family wealth, he quietly provided gifts and endowments totaling well over $1 million to sustain the anthropological institutions with which he was most closely identified: his beloved Anthropology Department at Berkeley, his alma mater Northwestern University, and Southern Methodist University, where two of his former students shaped the growth of its new anthropology department and continued to work with him on writing projects related to medical anthropology and Tzintzuntzan's community transformation.

But it was not just in major gifts and endowments that Foster's commitment was manifested. At the 1969 AAA annual meeting in New Orleans, a group of anthropologists interested in Mexico and Latin America gathered to discuss the formation of a professional organization (which eventually became the Society for Latin American Anthropology). The leaders of the group had arranged with the hotel to provide drinks and food to those who came to the reception following the meeting. Unfortunately, there was some confusion about whether the organizers or the AAA would pay the bill of several hundred dollars. Foster heard about the situation and anonymously took care of the bill.

Throughout his life, Foster attributed his success to chance, luck, and serendipity. In the end, American anthropology was lucky to have Foster.

CHRONOLOGY

1913	Born on October 9 in Sioux Falls, South Dakota
1935	B.S. degree in anthropology, Northwestern University, Evanston, Illinois
1935	Enrolled in doctoral program in anthropology at University of California, Berkeley
1936	First trip to Mexico
1937	Summer fieldwork among the Yuki of Round Valley, California
1938	January 6, married Mary LeCron (known as Mickie)
1940-1941	Fieldwork among the Sierra Popoluca in Soteapan, Veracruz, Mexico
1941	Ph.D. in anthropology, University of California, Berkeley
1941-1942	Instructor in sociology, Syracuse University
1942-1943	Lecturer in anthropology, UCLA
1943	Social science analyst, Institute of Inter-American Affairs, Washington, D.C.
1943-1952	Ethnologist, Institute of Social Anthropology, Smithsonian Institution; 1944-1946, Mexico City; 1946-1952, Institute director in Washington, D.C.
1945-1946	Initial fieldwork in Tzintzuntzan, Michoacán, Mexico
1949-1950	Fieldwork in Spain on Spanish background of contemporary Latin America
1951-1952	Consultant with Institute of Inter-American Affairs on applied anthropology in Latin America
1953-1979	University of California, Berkeley, director, Museum of Anthropology, 1953-1955; lecturer in public health, 1954-1965; professor of anthropology, 1955-1979; department chair, 1958-1961, 1972-1973; director, joint (with UCSF) Ph.D. program in medical anthropology, 1972-1979; professor emeritus, 1979-2006
1957-1959	Member, Executive Board, American Anthropological Association
1958-2004	Continuing longitudinal field research in Tzintzuntzan, Michoacán, Mexico

1982	Bronislaw Malinowski Award, Society for Applied Anthropology
1990	Honorary doctor of humane letters degree, Southern Methodist University, Dallas, Texas
1996	Berkeley Anthropology Emeriti Lecture (by Evon Z. Vogt) in honor of Foster
1997	Anthropology Library at Berkeley renamed in honor of George and Mary Foster
2000	First annual George and Mary Foster Distinguished Lecture in Cultural Anthropology, Southern Methodist University, Dallas, Texas
2004	The Deering Family Award, Northwestern University
2005	Career Achievement Award, Society for Medical Anthropology

PROFESSIONAL RECORD

1935	B.S. degree in anthropology, Northwestern University
1941	Ph.D. in anthropology, University of California, Berkeley
1941-1942	Instructor in sociology, Syracuse University
1942-1943	Lecturer in anthropology, UCLA
1943	Social science analyst, Institute of Inter-American Affairs, Washington, D.C.
1943-1952	Ethnologist, Institute of Social Anthropology, Smithsonian Institution; 1944-1946, Mexico City; 1946-1952, institute director in Washington, D.C.
1953-1979	University of California, Berkeley, director, Museum of Anthropology, 1953-1955; lecturer in public health, 1954-1965; professor of anthropology, 1955-1979; department chair, 1958-1961, 1972-1973; director, joint (with UCSF) Ph.D. program in medical anthropology, 1972-1979; professor emeritus, 1979-2006

MEMBERSHIPS

American Anthropological Association (fellow)
Cosmos Club, Washington, D.C.
Society for Applied Anthropology (fellow)
Society for Latin American Anthropology
Society for Medical Anthropology
Sociedad Mexicana de Antropología

NOTES

1. Foster's line-a-day diary listed details of his travels. His entries typically included the name of the vessel, train, or type of aircraft, times of departure and arrivals, cities and countries along the way, hotels and restaurants, and persons visited. The information provided here, derived from his 56-page single-spaced typed summary of the Fosters' trips and cruises from 1938 to 2000, is intended to capture some of his enthusiasm for travel.

2. To avoid confusion, I refer throughout to Prof. George McClelland Foster Jr. as "Foster" and to Prof. Mary LeCron Foster as "Mickie." This use of his patronymic and her nickname reflects the way many perceived them. Even in Tzintzuntzan he always was called "*el Doctor*" while she was called "Mariquita."

3. Mickie provided Foster with invaluable assistance—reading and suggesting corrections and improvements on all of his papers—for the next 22 years, until she returned to graduate studies in linguistics at Berkeley. She took advantage of his return to Tzintzuntzan to do field research for a dissertation on Tarascan grammar. Subsequently, she published important work on symbolism, language origins, and peace and conflict (cf. Brandes, 2003, M. Foster, 2001).

4. The Tzintzuntzan corpus of field notes, photographs, censuses, genealogies, etc. eventually will join the rest of Foster's professional materials in the archives of the Bancroft Library at the University of California, Berkeley. At the time of this writing, the Tzintzuntzan files are being digitized and organized for scholarly use at the Department of Anthropology, Southern Methodist University. Interested scholars should contact the author, who serves as literary co-executor (with Stanley Brandes of the University of California, Berkeley) of Foster's professional materials.

5. In 2000 a decade of efforts came to fruition when a Spanish translation of *Empire's Children* was published by El Colegio de Michoacán as *Los Hijos del Imperio: La Gente de Tzintzuntzan.* More than 800 copies of this handsome volume have been provided at no cost to households in the community and to numerous Tzintzuntzeño emigrant households in Mexico and in the United States.

6. The Tzintzuntzan monograph, translated into Spanish as *Tzintzuntzan: Los campesinos mexicanos en un mundo en cambio* was published by the prestigious Fondo de Cultura Económica and went through three reprintings. The American edition is still in print, having gone from the imprint of Little, Brown (1967) to Elsevier in 1979 and finally to Waveland Press in 1988.

7. Foster's account of the theoretical, methodological, logistical, and personal dimensions of his over 50 years of research in Tzintzuntzan is presented in the volume *Chronicling Cultures* (Kemper and Royce, 2002), and represents an extension from his account of 30 years of research published in the earlier volume on *Long-Term Field Research in Social Anthropology* (1979).

8. The dates for Micaela and members of her household come from the master file ("*fichero*" in Spanish) created by Foster in the 1960s after he became committed to a long-term study of Tzintzuntzan. Since then, data derived from a series of decennial household censuses, the parish and civil archives, and genealogical data on the major families have been combined on individual 5×8-inch sheets for each of more than 5,000 individuals. The master file is maintained and updated by the author, with assistance from a research team of knowledgeable community members.

9. Without a doubt the most important piece in the Festschrift is that of Eugene Hammel and Laura Nader, titled "Will the Real George Foster Please Stand Up? A Brief Intellectual History" (1979, pp. 159-166). This article is available at the Anthropology Emeritus Lecture Series website, specifically the Fifth Emeritus Lecture delivered by Evon Z. Vogt on October 21, 1996: http://sunsite.berkeley.edu/Anthro/foster/bio/fobib.html. This website also includes a link to the exhibit "Tzintzuntzan, Mexico: photographs by George Foster" (http://hearstmuseum.berkeley.edu/exhibitions/tzin/01.html). Other assessments of Foster's career include M. Foster (2001); Kemper (1991, 2006); Kemper and Brandes (2007); Weaver (2002); and Zamora (1983).

REFERENCES

Brandes, S. 2003. Mary LeCron Foster (1914-2001). *Am. Anthropol.* 105(1):218-221.

Clark, M., R. V. Kemper, and C. Nelson, eds. 1979. From Tzintzuntzan to the "Image of Limited Good": Essays in Honor of George M. Foster. *Kroeber Anthropol. Soc. Pap.* 55-56.

Foster, M. L. 2001. *Finding the Themes: Family, Anthropology, Language Origins, Peace and Conflict,* an oral history conducted in 2000 by S. Riess, Regional Oral History Office, Bancroft Library. Berkeley: University of California. (http://content.cdlib.org/xtf/view?docId=kt4s2003sn&brand=calisphere. Accessed May 24, 2007.)

Hammel, E. A. 2000. Introduction. In *George M. Foster, An Anthropologist's Life in the Twentieth Century: Theory and Practice at UC Berkeley, the Smithsonian, in Mexico, and with the World Health Organization*, pp. iv-viii. An oral history conducted in 1998 and 1999 by S. B. Riess. Regional Oral History Office, Bancroft Library. Berkeley: University of California. (http://content.cdlib.org/xtf/view?docId=kt7s2005ng&brand=calisphere. Accessed May 24, 2007.)

Hammel, E., and L. Nader. 1979. Will the Real George Foster Please Stand Up? A Brief Intellectual History. In *From Tzintzuntzan to "The Image of Limited Good: Essays in Honor of George M. Foster,* eds. M. Clark, R. V. Kemper, and C. Nelson. *Kroeber Anthropol. Soc. Pap.* 55-56:159-164.

Kemper, R. V. 1991. Foster, George M. In *International Directory of Anthropologists* (compiled by Library-Anthropology Resource Group, C. Winters, gen. ed.), pp. 212-13. New York: Garland Publishing.

Kemper, R. V. 2006. George M. Foster (1913-2006). *SfAA Newsl.* 17(4):3-15. (http://www.sfaa.net/newsletter/nov06nl.pdf. Accessed May 24, 2007.)

Kemper, R. V., and S. Brandes. 2007. George McClelland Foster, Jr. (1913-2006). *Am. Anthropol.* 109(2):427-433.

Kemper, R. V., and A. Peterson Royce, eds. 2002. *Chronicling Cultures: Long-term Field Research in Anthropology*. Walnut Creek, Calif.: Altamira Press.

Rollwagen, J. (producer). 1992. *Tzintzuntzan in the 1990s: A Lakeside Village in Highland Mexico* (Module 1: Introduction, 22 minutes; Module 2: Change in Tzintzuntzan, 33 minutes; Module 3: part 1, Religious Calendar, 14 minutes). Brockport, N.Y.: The Institute Inc.

Vogt, E. Z. 2002. The Harvard Chiapas Project: 1957-2000. In *Chronicling Cultures: Long-term Field Research in Anthropology*, eds., R. V. Kemper and A. Peterson Royce, pp. 135-159. Walnut Creek, Calif.: Altamira Press.

Weaver, T. 2002. George M. Foster: Medical anthropology in the post-World War II years. In *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers,* ed. T. Weaver, pp. 170-186. Oklahoma City: Society for Applied Anthropology.

Zamora, M. D., ed. 1983. Social change in India, Pakistan, and Bangladesh: Essays in honour of George M. Foster. *S. Asian Anthropol.* 4(2):63-125.

SELECTED BIBLIOGRAPHY

1942

A Primitive Mexican Economy. Monographs of the American Ethnological Society V. New York: J. J. Agustin.

1944

A Summary of Yuki Culture. University of California Anthropological Records 5(3):155-244. Berkeley: University of California Press.

1948

With G. Ospina. *Empire's Children: The People of Tzintzuntzan*. Washington, D.C.: Smithsonian Institution, Institute of Social Anthropology Publication No. 6. México, D.F.: Imprenta Nuevo Mundo.

1952

Relationships between theoretical and applied anthropology: A public health program analysis. *Hum. Organ.* 11(3):5-16.

1958

Problems in Intercultural Health Programs. Social Science Research Council Pamphlet No. 12. New York: Social Science Research Council.

1960

Culture and Conquest: America's Spanish Heritage. Viking Fund Publications in Anthropology No. 27. New York: Wenner-Gren Foundation for Anthropological Research.

1961

The Dyadic Contract: A model for social structure of a Mexican peasant village. *Am. Anthropol.* 63:1173-1192.

1962

Traditional Cultures and the Impact of Technological Change. New York: Harper & Bros.

1963

The Dyadic Contract. II: Patron-client relationship. *Am. Anthropol.* 65:1280-1294.

1965

[1] Peasant society and the Image of Limited Good. *Am. Anthropol.* 67:293-315.

[2] The sociology of pottery: Questions and hypotheses arising from contemporary Mexican work. In *Ceramics and Man,* ed. F. R. Matson, pp. 42-61. Viking Fund Publications in Anthropology No. 41. New York: Wenner-Gren Foundation for Anthropological Research.

1967

Tzintzuntzan: Mexican Peasants in a Changing World. Boston: Little, Brown and Co.

1969

[1] *Applied Anthropology.* Boston: Little, Brown and Co.

[2] Godparents and social networks in Tzintzuntzan. *Southwest. J. Anthropol.* 25:261-278.

1972

The anatomy of envy: A study in symbolic behavior, and reply [to commentators]. *Curr. Anthropol.* 13:165-186, 198-202.

1974

With R. V. Kemper, eds. *Anthropologists in Cities.* Boston: Little, Brown and Co.

1976

Graduate study at Berkeley: 1935-1941. In "Paths to the Symbolic Self: Essays in Honor of Walter Goldschmidt," eds. J. P. Loucky and J. R. Jones. *Anthropol. UCLA* 8(1-2):9-18.

1978

With B. Gallatin Anderson. *Medical Anthropology.* New York: Wiley & Sons.

1979

With T. Scudder, E. Colson, and R. V. Kemper, eds. *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

1982

Applied anthropology and international health: Retrospect and prospect. *Hum. Organ.* 41:189-197.

1985

South Seas cruise: A case study of a short-lived society. *Ann. Tourism Res.* 13:215-238.

1994

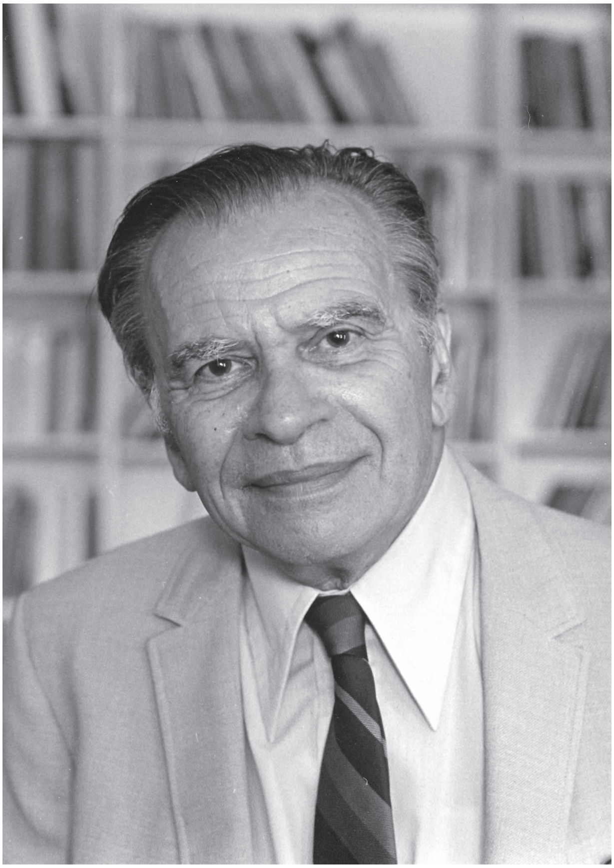
Hippocrates' Latin American Legacy: Humoral Medicine in the New World. Langhorne, Pa.: Gordon and Breach Science Publishers.

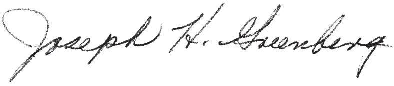
2000

An Anthropologist's Life in the Twentieth Century: Theory and Practice at UC Berkeley, the Smithsonian, in Mexico, and with the World Health Organization. An oral history conducted in 1998 and 1999 by S. B. Riess. Regional Oral History Office, Bancroft Library. Berkeley: University of California. (http://content.cdlib.org/xtf/view?docId=kt7s2005ng&brand=calisphere. Accessed May 24, 2007.)

2002

A half century of field research in Tzintzuntzan, Mexico: A personal view. In *Chronicling Cultures: Long-term Field Research in Anthropology*, eds. R. V. Kemper and A. Peterson Royce, pp. 252-283. Walnut Creek, Calif.: Altamira Press.





JOSEPH HAROLD GREENBERG

May 28, 1915–May 7, 2001

BY WILLIAM CROFT

JOSEPH H. GREENBERG, ONE OF THE most original and influential linguists of the 20th century, died at his home in Stanford, California, on May 7, 2001, three weeks before his 86th birthday. Greenberg was a major pioneer in the development of linguistics as an empirical science. He came of intellectual age at a time when linguistics was establishing itself as an independent academic discipline, and helped to shape the field. Greenberg's work was always founded directly on quantitative data from a single language or from a wide range of languages. His chief legacy to contemporary linguistics lies in the areas of language universals and historical linguistics. Greenberg is the founder of the modern typological approach to language universals, in which language universals are discovered inductively by the examination of a worldwide sample of languages, and explained in terms of the function of language, including the meanings conveyed by grammatical structures and constraints imposed by our abilities to comprehend and produce utterances. The typological approach has been tremendously influential, and is often compared to the generative approach of

This is a revised version of an obituary that appeared in Language 77(2001):815-830 and is used by permission of the Linguistic Society of America.

Chomsky. Greenberg's paper "Some Universals of Grammar with Particular Reference to the Order of Meaningful Elements" (1963) is one of the most cited papers in the field of linguistics. Greenberg also single-handedly reformed the genetic classification of the languages of Africa, and aroused major controversies (which still continue) in his later work in the genetic classification of the languages of Oceania, Eurasia, and the Americas—in other words, the languages of the entire world. Greenberg also shifted historical linguistics toward the study of universals of language change, including the study of grammaticalization, one of the most active areas in historical linguistics today. In addition to these seminal and far-reaching contributions, Greenberg also made major contributions to sociolinguistics, psycholinguistics, phonetics and phonology, morphology, and especially African language studies.

Joe Greenberg was born on May 28, 1915, in Brooklyn, New York, the second of two children. His father was a Polish Jew, his mother a German Jew. His father's family name was originally Zyto, but in one of those turn-of-the-century immigrant stories, his father ended up taking the name of his landlord. Joe Greenberg's early loves were music and languages. As a child he sat fascinated next to his mother while she played the piano, and one day he asked her to teach him. She taught him musical notation and then found him a local teacher. Greenberg ended up studying with a Madame Vangerova, associated with the Curtis Institute of Music. Greenberg even gave a concert at Steinway Hall at the age of 14, and won a citywide prize for best chamber music ensemble. But after finishing high school Greenberg chose an academic career instead of a musical one, although he continued to play the piano every evening until near the end of his life.

Greenberg's fascination with languages began equally early. He went to Hebrew school, which offered only an elementary education in Hebrew, essentially how to read the script. But Greenberg got hold of a Hebrew grammar and taught himself the language. He studied Latin and German at James Madison High School. He had a friend at the Erasmus High School who studied Classical Greek, but James Madison High School didn't offer Greek. Greenberg learned as much Greek as he could from a parallel-text edition of Sophocles' plays and the etymologies of the Oxford dictionary, and asked his father if he could transfer to Erasmus High School. They went to see the principal, who asked Joe why he wanted to study Greek, and he simply said, "I'd like to study Greek," and the principal refused his transfer. On the way home Joe cried, and his father took him into town and bought him a Greek grammar and dictionary from a used bookstore. With those books, he taught himself Greek—in fact, that was the usual way he learned languages.

When Greenberg began college at Columbia University in 1932, he continued studying Latin and Greek and also taught himself Classical Arabic. He signed up for classes in obscure languages, such as Akkadian and various Slavic languages, annoying professors who thought they could get away without teaching by offering classes they thought nobody would take. He discovered comparative linguistics in his junior year and anthropology in his senior year. Also in his senior year he audited a class given by Franz Boas on American Indian languages and, on his own, read all the grammars in Boas's *Handbook of American Indian Languages* (Boas, 1911, 1922). Because of his Classical and Semitic background, Greenberg entertained the idea of becoming a medieval historian specializing in contacts between Christianity and Islam in Africa. But opportunities in the humanities were nonexistent during the Depression. His anthropology professor, Alexander Lesser, suggested he apply for a Social Science Research Council Ph.D. grant to study under Melville Herskovits, a major Africanist at Northwestern University. Lesser obtained references for him from Boas and Ruth Benedict. Greenberg received the grant and studied with Herskovits at Northwestern. In his third year Greenberg did fieldwork among the Hausa in Nigeria (learning Hausa in the process), and wrote a dissertation on the influence of Islam on one of the few remaining non-Islamic Hausa groups.

Greenberg's intellectual interests continued to expand. Herskovits encouraged him to spend his second year at Yale (1937-1938), where he studied with the anthropologists Leslie Spier and Robert Lowie and the linguists Edgar Sturtevant and Franklin Edgerton. (He never met the linguist and anthropologist Edward Sapir, always Joe's hero; Sapir was ill at the time and died in 1940.) The linguistics courses were all on comparative Indo-European. It was not until he returned to Yale with a postdoctoral fellowship in 1940 that he made his acquaintance with American structuralism, auditing courses with Bernard Bloch, George L. Trager and Benjamin Lee Whorf, all leading structuralists. Greenberg also met Leonard Bloomfield, considered the founder of American structuralist linguistics, at around this time, though not at Yale. Bloomfield suggested to Greenberg that he read the analytical philosopher Rudolf Carnap and thereby introduced Greenberg to logical positivism. Greenberg studied Alfred North Whitehead and Bertrand Russell's *Principia Mathematica,* even taking it with him when he was drafted into the Army in 1940. Logical positivism had a significant influence on Greenberg, not only in the general rigor of its argumentation; he published axiomatizations of kinship systems and phonology.

Greenberg took the postdoctoral position at Yale because there were no academic positions in the Depression, especially for Jews. Being drafted into the Army in 1940 solved the employment problem for five years. Before he left for the war, he married Selma Berkowitz, whom he had met when she was finishing high school and he was starting at Columbia; she remained his companion and support for the rest of his life. Greenberg entered the Army Signal Corps and was eventually sent to North Africa, participating in the landing at Casablanca. In North Africa he and his colleagues got up in the middle of the night and deciphered the German or Italian code by the early morning. After the Allied invasion of Italy he was sent to Italy, where he remained until the end of the war—and where he learned Italian, of course.

Conditions for academic employment were completely different after World War II. The GI Bill offered funding for GIs to go to college, and universities expanded. This expansion continued as the postwar baby boom eventually made its way to college. Greenberg was appointed at the University of Minnesota in 1946 and moved to the anthropology department at Columbia University in 1948. The leading European structuralists Roman Jakobson and André Martinet, recent arrivals from Europe, had founded the Linguistic Circle of New York. Through them Greenberg was exposed to the structuralism of the Prague school, including Nicholas Trubetzkoy's work on markedness. (Greenberg also coedited *Word*, the journal of the Linguistic Circle of New York, from 1950 to 1954.)

Greenberg's intellectual roots included all of the major strands of linguistics, philosophy, and anthropology at the time: American structuralism, Prague-school structuralism, comparative historical linguistics, logical positivism, and cultural anthropology. (This is not to mention his Classical and Semitic background, or his awesomely broad reading, which continued to the end of his life.) At the time, the first linguistics departments in America were being established, and Greenberg was in a position to help shape the field of linguistics. At that time, the field was still largely divided among philologists working on historical linguistics and anthropological field linguists working on "exotic" languages. Linguistics, chiefly in the form of structuralism, was still in the process of declaring its academic and intellectual independence from philology and anthropology. Greenberg made major contributions to the establishment of linguistics as an independent science.

Greenberg's first major work brought the young American linguist immediate fame and controversy. Greenberg's Ph.D. research was as an Africanist, and he turned his attention to African languages. His first large project was nothing if not ambitious: the genetic classification of the languages of Africa. After a preliminary publication in American Anthropologist in 1948, a complete classification was published in serialized form in the *Southwestern Journal of Anthropology* in 1949-1950. At the time, African languages were classified into five families: Semitic, Hamitic, Sudanic, Bantu, and Bushman (Newman, 1995, p. 3). Greenberg classified them instead into 16 families, on the basis of two fundamental principles.

His first principle is the exclusion of typological features from genetic classification. That is, properties purely of form (phonological patterns or grammatical patterns) or of meaning (semantic patterns) are too likely to diffuse, are too small in number, and are too likely to result from independent convergence to act as indicators of genetic descent. Instead, the arbitrary pairings of form and meaning in both morphology and lexicon provide the best evidence for genetic classification. This separation of typological and genetic traits of languages provided the key to genetic classification, and almost as a by-product produced the independent development of typology a few years later in Greenberg's career. His second principle is the exclusion of nonlinguistic evidence from the establishment of linguistic genetic families. Both of these principles were violated by the accepted classifications of African languages. Typological traits, such as the presence or absence of gender, and nonlinguistic factors, such as race, played a role in the accepted classification.

Greenberg's proposals cut across the accepted classification but established the basic principles for genetic classification of languages. As a young American scholar he upset the senior British and German scholars in the field. Greenberg knew he was overthrowing established academic views, as is clear from the preliminary article in American Anthropologist, in which he fearlessly takes on the authorities in his field.

Greenberg did not stop at African language classification. He turned to the study of the languages of the Americas, Australia, and other parts of the world. While North American languages were at the time grouped into a small number of large families, South American languages were not, and so Greenberg began with South America, where he identified seven families. In Australia he identified one widespread family, which he called General Australian (identical to Pama-Nyungan), and a large number of small families. Some of these observations were published in "Historical Linguistics and Unwritten Languages" (1953). In response to criticisms of that paper and his other work, Greenberg explicitly formulated his third and final principle of genetic classification, namely the simultaneous comparison of the full range of languages and forms for the area under study (mass comparison, later called multilateral comparison). In

1955 Greenberg's African classification was reprinted, and he had consolidated the 16 families to 12. He continued his classification work, proposing 14 families for the non-Austronesian, non-Australian languages in Oceania, in a report to the Wenner-Gren Foundation in 1958.

Greenberg did not fully realize the ramifications of his method until, as he described it later, "One day, probably in early 1959, as I put my foot on the pavement to cross Amsterdam Avenue on my way to Columbia, an idea flashed before me. Why shouldn't I just look at all of my then twelve families in Africa together?" (Greenberg 1996a, p. 147). He did so. His final classification of African languages (1963) consists of four families: Afroasiatic, Khoisan, Niger-Kordofanian, and Nilo-Saharan. This classification has been broadly accepted but not until after a lengthy and heated debate. In this debate many of the British and German Africanists defended their typological and nonlinguistic classifications of African languages. A crucial number of mostly American Africanists accepted Greenberg's results and the method that he used. Other linguists, in particular Americanists and Indo-Europeanists, argued that only reconstruction of the protolanguage would "prove" the genetic classification of languages (see Croft, 2005a). Almost all of these arguments would recur from the late 1980s up to the present, as Greenberg's classifications in other parts of the world were published.

As a matter of fact, Greenberg's more controversial classifications of the languages of Oceania, the Americas and Eurasia were all arrived at by the early 1960s. Greenberg presented evidence in 1960 that the 14 families he identified in Oceania belong to a single group. In the same year, a paper originally presented in 1956 was published, proposing that the American Indian languages fall into three genetic groupings. (Interestingly, the linguists Sydney Lamb and

Morris Swadesh independently arrived at the same conclusion at about the same time.) In the course of his research on the American Indian languages, he compared them with languages in northern Eurasia and by the early 1960s had identified another large family ranging from Indo-European in the west to Eskimo-Aleut in the east. But Greenberg did not publish the evidence for these proposals until many years later, and for this reason I will return to the 1950s to take up the other strands of Greenberg's contributions to linguistics.

Much of Greenberg's earlier work is on African linguistics, and he was recognized early on as one of the leading African language scholars. (In fact, he was an unnerving presence at African language conferences, where he would sit in the front row and ask questions revealing his knowledge of virtually every African language that was the topic of a paper.) In addition to the classification of African languages, he wrote numerous articles on phonology and morphology, particularly in Afroasiatic, and on language contact in Africa. In "The Patterning of Root Morphemes in Semitic" (1950), Greenberg displayed his characteristic thorough, quantitative approach to a linguistic problem. He examined 3775 Arabic triliteral roots and surveyed roots in other Semitic languages in order to formulate a number of constraints on the occurrence of phonemes and phonological features across the root consonants of Semitic. The article also displays his breadth of knowledge of the literature and citation of antecedents and parallel discovery. It is a pathbreaking work, one that has been repeatedly cited in later research on morpheme structure conditions and phonotactic constraints.

Greenberg described himself as being in a state of intellectual ferment, or even crisis, from 1952 to 1954 (Greenberg, 1994, p. 23). Although not formally trained as a linguist, he had been influenced by American structuralism and its seeming philosophical counterpart, logical postivism. Yet he had recognized some of structuralism's weaknesses, partly through influences such as the Prague School and comparative-historical linguistics (which had much greater prestige then than now). In particular, he questioned American structuralism's lack of interest in meaning and use, the strict separation of synchrony and diachrony, and the methods for uncovering basic linguistic units such as the phoneme.

Greenberg recalled another formative experience that occurred in 1953. He was part of an interdisciplinary seminar on linguistics and psychology organized by the Social Science Research Council. He presented the current state of linguistics, that is, the rigorous methodology of American structuralism, about which he already had misgivings. The psychologist Charles Osgood told Greenberg that that was quite impressive, but that if Greenberg could tell him something true of all languages, then psychologists would be interested. Greenberg later said that "this remark . . . brought home to me the realization that all of contemporary American linguistics consisted of elaborate but essentially descriptive procedures" and that Osgood's question "helped determine the direction of much of my future work" (Greenberg, 1986, pp. 13-14). Greenberg then turned to the problem of universals of language.

It would be four years, however, before Greenberg published his first paper on language universals. Instead, his next major publication in synchronic theory was in the area of typology (1954,1). At that time typology was the study of language differences, not similarities, based on phonological and morphological traits. The morphological typology of the 19th century, dividing languages into isolating, agglutinative, and inflectional types, was the only major typological classification of languages until Trubetzkoy's work on phonological systems. The morphological typology had been refined and elaborated by Edward Sapir, Joe's linguistic hero. Greenberg's essay was a refinement and quantification of Sapir's typology, accompanied by an insightful analysis of the fundamental segmentation of utterances into words and morphemes.

But Greenberg's interest was chiefly in universals. In 1954 he also published a paper on linguistic relativity (1954,2), in which he expressed skepticism about relativistic claims. (Joe told me that the editor of the volume invited him, expecting to receive an article supporting relativity, and instead received a rather unexpected contribution.) In 1957 Greenberg published his first paper on language universals, the last essay in his volume *Essays in Linguistics* (1957). (This collection also contains Greenberg's classic articles on language classification.) The last essay contains the germs of Greenberg's future work on language universals. Greenberg establishes the basic principle that universals must represent generalizations over historically independent cases of the phenomenon to be studied, and makes the first link between typology as practiced then and language universals. It is this link that represents Greenberg's great insight into the study of grammar. He notes that there are very few unrestricted universals of the form "All languages have X," and those that exist are not terribly interesting. Instead, the search for universals must focus on the distribution of types found in languages, that is, significant universals are to be found in constraints on crosslinguistic variation not in crosslinguistic uniformity. Greenberg also asserts that such universals "require some explanation, one which inevitably takes into account functional, psychological and social factors underlying all language behavior" (1957, p. 86)—an early statement of what has come to be called the functionalist approach to language.

The following year (1958-1959) Greenberg was invited as a fellow to the Center for the Advanced Study in the Behavioral Sciences at Stanford, along with Osgood and James Jenkins, the psychologists with whom he collaborated. It was an exciting year at the center: Next door to Greenberg the philosopher of science Thomas Kuhn was writing *The Structure of Scientific Revolutions*, and the philosopher W. V. O. Quine was writing *Word and Object*. Greenberg himself was working on language universals and planning a conference with Osgood and Jenkins, which was held at Dobbs Ferry in 1961.

At the Dobbs Ferry conference Greenberg first presented what became his most famous and far-reaching contribution to linguistics: "Some universals of grammar with particular reference to the order of meaningful elements." The same paper was presented in the following year at the Ninth International Congress of Linguists at MIT (where Noam Chomsky also presented his ideas to an international audience for the first time), and then published in 1963 along with the other Dobbs Ferry papers (1963, 1966). This paper remains one of the most widely cited papers in linguistics.

In this paper Greenberg goes beyond his 1957 essay to represent universals in logical form, namely, as implicational universals ("If a language has X, then it also has Y") and biconditional universals ("a language has X if and only if it also has Y"). He constructs an areally and genetically diverse sample of 30 languages in order to infer empirically valid universals, arguing that grammatical categories must be compared across languages on an ultimately external, semantic basis. He then applied this method to word order and morphological categories, constructing a total of 45 universals. In the concluding section Greenberg offers more general principles to account for the word order universals. In other words, Greenberg's paper establishes the basic methodology of what is now known as the typological approach to grammar, derives major empirical results, and offers a type of explanation used widely to this day in typological analyses.

The impact of Greenberg's paper was dramatic. At the time, the field of linguistics was also being challenged by Noam Chomsky. Chomsky argued that linguistics should focus its attention on syntax, rather than just phonology and morphology as the American and European structuralists were doing up to that time. Chomsky also argued that linguists should seek language universals, and contrary to the beliefs of many American structuralists, that there are significant language universals to be discovered. Chomsky sought language universals through deductive reasoning in the analysis of individual languages into their "deep structure" and transformations of that deep structure into surface structure.

At the same time, Greenberg produced a large number of substantive universals of syntax, derived by inductive empirical generalization over "surface" structure across a wide range of languages. Some of Chomsky's disciples immediately incorporated word order typology (e.g., McCawley, 1970). But the fact remains that Chomsky and Greenberg at about the same time proposed opposing theories about universals of grammar (particularly syntax), how they are to be defined and discovered, and how they are to be explained. These later became known as the Chomskyan and Greenbergian approaches to language universals, and still later characterized more broadly as the formalist and functionalist approaches to language (though in fact the latter labels encompass a broader range of theories than their historical founders would accept, and embrace theories that precede both of them). The formalist and functionalist approaches are the leading approaches to the study of language today.

During the 1960s, however, despite the great interest in his word order universals, Greenberg worked largely alone, while Chomsky's generative grammar came to dominate the American linguistic scene. This was partly due to institutional arrangements. In 1962 Greenberg moved from Columbia to the department of anthropology at Stanford University. Stanford had only a committee on linguistics at the time, and as a result Greenberg had very few graduate students. Greenberg was instrumental in establishing a department of linguistics at Stanford in 1973. In 1967 Greenberg and his colleague Charles Ferguson received a National Science Foundation grant for research into language universals that lasted until 1976. As a result, Greenberg and Ferguson were able to fund research by many postdoctoral fellows, including major figures in the next generation of typologists, such as Talmy Givón, Leonard Talmy, and Edith Moravcsik. This project resulted in a series of 20 Working Papers in Language Universals and the four-volume *Universals of Human Language* (Greenberg et al., 1978).

Greenberg himself produced a number of important studies of language universals during this time, on consonant clusters, glottalic consonants (1970, often cited), word prosodic systems and numeral systems, not to mention numerous general essays on typology and universals (especially 1974). The most influential synchronic study, after his word order research, is his article on universals of markedness and markedness hierarchies (1966). The theoretical concept of markedness was developed by the Prague School of European structuralism. Greenberg was first exposed to the Prague School's ideas during his early years teaching at Columbia. In Prague School theory, however, markedness is a property of language-specific grammatical categories, and the markedness of a category, such as “singular,” can vary from language to language. Greenberg reinterpreted markedness as a property of crosslinguistic categories, that is, conceptual categories, so that for instance it is a universal that the singular is unmarked compared with the plural. Greenberg constructed a series of universals of formal expression based on markedness relations, and also argued that the morphological (though not phonological) universals are ultimately explainable in terms of token frequency. Again, Greenberg’s work anticipates later developments in functionalist linguistic theory, now described as the usage-based model.

Greenberg’s theoretical interests were taking a new turn as early as the 1960s. He began to explore diachronic typology, that is, universals of language change as well as universals of synchronic language structure. Greenberg was no doubt also inspired by his extensive comparative-historical research in Africa and in other parts of the world. He realized that the constraints on patterns of crosslinguistic variation are ultimately constraints on paths of change of language, and so synchronic typology can and should be reanalyzed as diachronic typology. His first full statement of diachronic typology is found in his lectures from the Linguistic Society of America Summer Institute at the University of Californa at Los Angeles in 1966, published as “Some Methods of Dynamic Comparison in Linguistics” (1969). Greenberg demonstrates how synchronic typologies can be reinterpreted as diachronic ones, how comparative-historical studies can be used to develop hypotheses of universals of language change, and proposes a model for the representation of diachronic patterns. Greenberg’s diachronic approach to language is presented more generally in his Linguistic Society of America Presidential Address, “Rethinking Linguistics Diachronically” (1979).

Greenberg also published many studies of universals of language change. In addition to case studies from the Summer Institute lectures, he published papers on numeral constructions (e.g., 1972, 1977), gender markers (in 1978), word order, and pronouns. Of these the study on gender markers helped to stimulate the tremendous explosion of research on grammaticalization, which is the chief area of research in diachronic typology today. In a later paper (1991) Greenberg proposes a further process, regrammaticalization, by which a highly grammaticalized marker is employed in other grammatical functions, for example, a noun marker is employed as a verbal nominalizer. This process is identical to Lass's independently proposed mechanism of exaptation (Lass, 1990).

It is now de rigueur for typological studies to examine diachronic as well as synchronic universals. There is now much discussion of emergence in grammar and of the dynamic usage-based model in the functional-typological approach, and some of the linguists working in this area, including the late Keith Denning, Suzanne Kemmer, and myself, were part of a group of graduate students that worked with Greenberg around the time that he retired from Stanford. Nevertheless, it is accurate to say that the consequences of Greenberg's rethinking linguistics diachronically have yet to have their full impact in the intellectual development of linguistics.

But Greenberg's interest in genetic classification of languages never left him. From the beginning of his research on language universals to the end of his life, Greenberg argued that a prerequisite for typological research, synchronic as well as diachronic, is the establishment of the genetic classification of languages. One must know how closely languages are related in constructing proper language samples, and the establishment of genetic families allows the diachronic typologist to identify and compare grammaticalization processes in different language families. Greenberg's interest in the topic of language classification did not cease during the 1960s and 1970s, when most of his attention was focused on typology and universals. As was noted above, Greenberg had already identified the major families of Oceania, Eurasia, and the Americas by the early 1960s. However, he did not begin to publish the results of this research until later.

The first new publication on language classification outside Africa was "The Indo-Pacific Hypothesis" (1971,1). Between 1960 and 1970 Greenberg gathered all of the material then published on Indo-Pacific languages and was able to examine some unpublished data as well. He organized the data in 12 notebooks of 60-80 languages each, with up to 350 lexical entries for each language, and also made detailed grammatical comparisons in three further notebooks. To check against the possibility of borrowing from Austronesian, Greenberg prepared vocabularies of similar length for 50 Austronesian languages, particularly those in proximity to Indo-Pacific languages. Both lexical and grammatical evidence were presented, as with the African classification. Indo-Pacific contains the 14 subgroups originally identified in 1958, which were further divided into sub-subgroups. There were a handful of small groups and isolates that Greenberg identified as Indo-Pacific but was not able to assign to specific subgroups. The article concluded with proposals for the internal grouping of the 14 subgroups.

Greenberg's Indo-Pacific hypothesis met a different fate from his African hypothesis, which had become accepted by this time. After a few initial reactions—some positive and others negative—Greenberg's hypothesis was largely ignored (see Croft, 2005a,b).

Greenberg did not publish anything further on Indo-Pacific. Instead, he returned to his three-way classification of the languages of the Americas into Eskimo-Aleut, Na-Dene, and Amerind. Over nearly three decades Greenberg assembled 23 notebooks of around 80 languages each, with up to 400 lexical entries for each language, and six additional notebooks with grammatical comparisons. In 1987, one year after retiring from Stanford University, Greenberg published *Language in the Americas* (1987). In it he presented evidence, again both lexical and grammatical, for 11 subgroups of Amerind and for Amerind itself. He did not present evidence for Eskimo-Aleut (that being accepted), and presented only a response to an attack on Sapir's Na-Dene family. The book begins with a general defense of his method and a critique of the comparative method, and concludes with a suggestion that all of the contemporary languages of the world may form a valid genetic unit (the monogenesis hypothesis).

Greenberg's Amerind hypothesis, although anticipated by others, met yet another fate from this African and Indo-Pacific hypotheses. Between the 1950s when Greenberg proposed his African classification and the 1980s when he published his classification of the languages of the Americas, historical linguistic research moved away from establishing new genetic families. In fact, historical linguists engaged in deconstructing previously accepted language families, such as Sapir's Hokan and Penutian families in North America, or the Altaic family (Turkic, Mongolian, Tungusic) in Eurasia. In this atmosphere Greenberg's proposal that all of the languages of the Americas except Eskimo-Aleut and Na-Dene form a valid genetic grouping was vehemently rejected. Greenberg's proposal launched a debate—which has not yet ended—about the validity of the hypothesis and Greenberg's methods of linguistic genetic classification.

Greenberg participated vigorously in this debate, contributing some 20 responses, replies, commentaries, and reviews of his critics. He consistently maintained his position on his genetic classifications. He argued that a quantitative probabilistic argument is required to "prove" an empirical scientific hypothesis, in response to the historical linguists who argued that reconstruction of the protolanguage was necessary. Greenberg further argued that, in fact, his method necessarily precedes reconstruction, since reconstruction presupposes a classification He noted that he used the same methods for linguistic genetic classification in the Americas as he did in Africa, now generally accepted, and that this method was the same used to identify the now-accepted language families in the 18th and 19th centuries. Finally, he argued that alternative, nongenetic hypotheses for the widespread similarities in form and meaning in words and grammatical elements across languages, such as extensive language mixing, extensive borrowing of basic vocabulary and grammatical inflections, or sound symbolism, were either sociolinguistically implausible or not persuasively supported by attested sociohistorical developments in shallower, widely accepted language families.

Another controversial aspect of Greenberg's Amerind hypothesis was the support it received from physical anthropology and from genetics. Stephen Zegura and Christy Turner independently hypothesized a three-migration pattern into the Americas based on dentition and genetic evidence; the three of them published their results together (1986). In addition, the geneticist Luigi Luca Cavalli-Sforza compared genetic groupings of humans and Greenberg's linguistic classification, and found a high degree of similarity (Cavalli-Sforza et al., 1988; see also Greenberg, 1996b), which led to further controversy. Greenberg was of course encouraged by this convergence of independent evidence, but he always insisted that the linguistic classification must be established on linguistic evidence alone.

Greenberg continued to publish on diachronic, typological, and other topics, but the main focus of his research after his retirement from Stanford was genetic classification. His next area of study was Eurasia. He continued to gather lexical and grammatical evidence for a family he called Eurasiatic, consisting of Indo-European, Uralic, Altaic, Korean-Japanese-Ainu, Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut. Although many scholars had compared pairs of families (e.g., Indo-European and Uralic, Uralic and Altaic, Altaic and Japanese), Greenberg argued that all of the aforementioned groups together constitute a valid genetic unit. He published a number of articles presenting parts of this evidence, and eventually included 72 independent pieces of grammatical evidence in a monograph, *Indo-European and Its Closest Relatives: The Eurasiatic Language Family, vol. 1, Grammar* (2000,1).

Although Greenberg did not know at the time that he had only two years to live, at the age of 84 he proceeded to write the second volume (the lexical evidence) at a frantic pace. "I am fighting against time to get the second volume finished," he wrote me in January 2000. He submitted the final etymologies to Merritt Ruhlen for typing on October 27, 2000, and went into the hospital that evening. He was diagnosed with pancreatic cancer, and stayed home with his wife, Selma, from then until his death. But he worked until mid-March 2001 with Ruhlen on finalizing the lexical evidence.

Up to the last month of his life Joe was still incredibly active. Even in my last conversation with him, a week before he died, he could joke that he could have written five papers in the months since he had been diagnosed. Joe didn't want to stop. He wanted to pursue the classification of languages all the way up to the human language family, which he thought possible; and of course there were all those fascinating processes of language change that he encountered on the way. He planned to turn next to a southern group consisting most likely of Niger-Kordofanian, Nilo-Saharan, Elamo-Dravidian, Indo-Pacific, and Australian. He retrieved his old notebooks but realized that he needed more sources and did not have the time and energy to proceed. Fortunately, Joe also recognized that he had lived as full a scholarly life as one could ask for and that his published work (including the work to appear after his death) would leave a legacy that would extend far into the future.

During his long life Greenberg received many accolades: twice fellow at the Center for Advanced Study of the Behavioral Sciences, thrice Guggenheim fellow, elected to the American Philosophical Society and the National Academy of Sciences (in 1965), president of the Linguistic Society of America, the African Studies Association, the West African Linguistic Association, and the Linguistic Society of America Collitz Professor. Greenberg gave the first Distinguished Lecture of the American Anthropological Association, and received the Haile Selassie Award for African Research, the New York Academy of Sciences Award in Behavioral Science, and the American Academy of Arts and Sciences Talcott Parsons Prize in Social Science. Stanford University planned a conference to honor Greenberg, "Global Perspectives on Human Language," which Joe hoped to take part in, but sadly it became a conference in his memory in April 2002.

Despite the controversial positions he took from the beginning of his career to the end, and the stature he gained in the field, Joe Greenberg was one of the most mild-mannered and self-effacing scholars imaginable. He was unbelievably modest and unassuming for such a brilliant scientist. The reason, I believe, was that he always had a completely genuine curiosity and wonder at language and, indeed, at everything in the world. He also had an unpretentious, down-to-earth way of talking about languages—reinforced by his thick Brooklyn accent, no doubt, and the equally down-to-earth similes he used. He once said, "A speaker is like a lousy auto mechanic: Every time he fixes something in the language, he screws up something else." Another memorable remark came when Joe revived his typology class in the fall of 1984, while I was still a graduate student at Stanford. One day Joe was describing some interesting fact about a language, and he suddenly stopped and said, "You know, you gotta muck around in grammars. You can't just focus on one specific thing and pick it out. You read around and you discover things you never would have thought of."

Joe was a completely independent intellectual spirit. He was not so much an iconoclast as someone who considered nothing above questioning or beneath consideration. He absorbed comparative historical linguistics from Bloomfield, Sturtevant, and Edgerton, but did not let its strictures about reconstruction prevent him from pursuing genetic linguistic classification. He learned American structuralism from Bloch, Trager, and Whorf, but did not accept their ban on meaning nor their antiuniversalist stance. He continued his typological approach to universals, developed at the same time as generative grammar, while the rest of American linguistics fell under Chomsky's spell.

Joe sometimes attributed his independence to the fact that he didn't study linguistics in a linguistics department. But Joe was deeply knowledgeable about the history of linguistics. (I never had the opportunity to take the history of linguistics from him, but he told me that he usually got to around the Renaissance by the end of the course.) He could quote freely from the great 19th-century German historical linguists; but he also followed developments in contemporary linguistic theories and, of course, read the specialist language journals. In fact, most of Joe's learning came from reading: logic, philosophy, languages, linguistics, anthropology, history, culture, biology, and so on. Joe lamented to me that students no longer received the broad humanistic education that he did—but he largely gave that education to himself.

Joe was the scholar's scholar. His office was Green Library at Stanford, where he worked all day six days a week, always reading and making notes in pencil in his famous notebooks. The library staff one day surprised him by installing a brass plaque on the oak reading table where he worked, inscribed "The Joseph H. Greenberg Research Table." Joe's erudition was awesome, but he wore it lightly. He could recall obscure facts about languages anywhere in the world, though in later years he said, "Every time I learn the name of a new student, a fact about Nilo-Saharan flies out of my head."

Although his mind was as sharp as ever, age did slow Joe down. He no longer scampered down the stairs from his office. He shuffled ever more slowly from home to Green Library and back. He even stopped working in the library on Saturdays in the last decade of his life, going in "only" five days a week, and stopped working at home at night (!). In his seventies he was unhappy that he would read a grammar of a language and not remember everything in it. He complained that he shouldn't have waited until the age of 65 to start learning Japanese, but at 85 admitted he could read an Ainu-Japanese dictionary without that much difficulty. When he reviewed his African notebooks at the end of life, over four decades after he wrote them, he was disappointed that he couldn't remember the specific word forms.

"I learned more from languages than from linguists," Joe used to say. He was first and foremost an empirical scientist of language. Both his controversial work on language universals and his even more controversial work on genetic classification were based on the same method: a nearly exhaustive examination of all the linguistic data he could get his hands on. His language universals and genetic classification—dramatic and far beyond what anyone else had done as they are—were always presented as provisional and subject to revision.

Joe was also a very kind-hearted and generous soul. He always lent me his notebooks, even the notebook on which his famous word order paper was based. He lent his Indo-Pacific notebooks to a student who wanted to reanalyze his classification; fortunately, they were returned. Joe was also remarkably cheerful, although he was very hurt by the numerous ad hominem attacks on his Amerind classification by the various political machinations in the Stanford linguistics and anthropology departments and by the premature death of his last student, Keith Denning, in 1998. After Joe was diagnosed with cancer he told me he was depressed and added that it was the first time in his life that he had felt depressed. He was devoted to his wife, Selma, to whom he was married for over 60 years, and who was his greatest support throughout his extraordinary career. Selma outlived Joe by over five years; she died in Palo Alto, California on January 28, 2007.

I would like to thank Paul Newman, Merritt Ruhlen, Michael Silverstein, and Selma Greenberg for their help in preparing this memoir. I would also like to thank John Rawlings of Stanford University Library for making available to me a transcript of two interviews he conducted with Greenberg in March 2001. And of course my greatest thanks are to Joe Greenberg himself for sharing the unpublished reminiscences and thoughts reported here, and above all for my education in linguistics.

REFERENCES

Boas, F., ed. 1911. *Handbook of American Indian Languages*, Part 1. Smithsonian Institution Bureau of American Ethnology Bulletin 40-1. Washington, D.C.: U.S. Government Printing Office.

Boas, F., ed. 1922. *Handbook of American Indian Languages*, Part 2. Smithsonian Institution Bureau of American Ethnology Bulletin 40-2. Washington, D.C.: U.S. Government Printing Office.

Cavalli-Sforza, L. L., A. Piazza, P. Menozzi, and J. Mountain. 1988. Reconstruction of human evolution: Bringing together genetic, archeological and linguistic data. *Proc. Natl. Acad. Sci. U. S. A.* 85:6002-6006.

Croft, W. 2005a. Editor's introduction. *Genetic Linguistics: Theory and Method*, Joseph H. Greenberg, pp. x-xxxv. Oxford: Oxford University Press.

Croft, W. 2005b. Bibliography of works related to Joseph H. Greenberg's theory and methods for genetic linguistics. *Genetic Linguistics: Theory and Method*, Joseph H. Greenberg, pp. 389-410. Oxford: Oxford University Press.

Greenberg, J. H. 1986. On being a linguistic anthropologist. *Annu. Rev. Anthropol.* 15:1-24.

Greenberg, J. H. 1994. The influence of Word and the Linguistic Circle of New York on my intellectual development. *Word* 45:19-25.

Greenberg, J. H. 1996a. The genesis of multilateral comparison. *Mother Tongue* 2:145-148.

Greenberg, J. H. 1996b. Genes, languages and other things: Review of *History and Geography of Human Genes* by L. L. Cavalli-Sforza, P. Menozzi, and A. Piazza. *Rev. Archaeol.* 16(2):24-28.

Lass, R. 1990. How to do things with junk: Exaptation in language change. *J. Linguist.* 26:79-102.

McCawley, J. D. 1970. English as a VSO language. *Language* 46:286-299.

Newman, P. 1995. On being right: Greenberg's African linguistic classification and the methodological principles which underlie it. Bloomington: Institute for the Study of Nigerian Languages and Cultures, African Studies Program, Indiana University.

SELECTED BIBLIOGRAPHY

1950

The patterning of root morphemes in Semitic. *Word* 6:162-181.

1953

Historical linguistics and unwritten languages. In *Anthropology Today*, ed. A. L. Kroeber, pp. 265-286. Chicago: University of Chicago Press.

1954

[1] A quantitative approach to the morphological typology of language. *In Method and Perspective in Anthropology*, ed. R. F. Spencer, pp. 192-220. Minneapolis: University of Minnesota Press.

[2] Concerning inferences from linguistic to nonlinguistic data. In *Language in Culture*, ed. H. Hoijer, pp. 3-18. Chicago: University of Chicago Press.

1955

Studies in African Linguistic Classification. New Haven, Conn.: Compass Press.

1957

Essays in Linguistics. Chicago: University of Chicago Press.

1963

[1] *The Languages of Africa.* Bloomington: Indiana University Press.

[2] Some universals of grammar with particular reference to the order of meaningful elements. In *Universals of Grammar*, ed. J. H. Greenberg, pp. 73-113. Cambridge, Mass.: MIT Press.

1966

Language Universals, With Special Reference to Feature Hierarchies. Janua Linguarum, Series Minor 59. The Hague: Mouton.

1969

Some methods of dynamic comparison in linguistics. In *Substance and Structure of Language*, ed. J. Puhvel, pp. 147-203. Berkeley: University of California Press.

1970

Some generalizations concerning glottalic consonants, especially implosives. *Int. J. Am. Linguist.* 36:123-145.

1971

[1] The Indo-Pacific hypothesis. *Curr. Trends Linguist.* 8:808-871.

[2] *Language, Culture and Communication, Essays by Joseph Greenberg,* selected and introduced by A. S. Dil. Stanford: Stanford University Press.

1972

Numeral classifiers and substantival number: Problems in the genesis of a linguistic type. *WPLU* 9:1-39. Reprinted in *Linguistics at the Crossroads* ed. Adam Makkai. Padua: Livinia Editrice, 1977 pp. 276-300.

1974

Language Typology: A Historical and Analytic Overview. Janua Linguarum, Series Minor 184. The Hague: Mouton.

1978

With C. A. Ferguson and E. A. Moravcsik, eds. *Universals of Human Language.* 4 vols. Stanford: Stanford University Press.

1979

Rethinking linguistics diachronically. *Language* 55:275-290.

1980

Circumfixes and typological change. In *Papers from the 4th International Conference on Historical Linguistics*, ed. E. C. Traugott et al., pp. 233-241. Amsterdam and Philadelphia: John Benjamins.

1986

With C. G. Turner II and S. Zegura. The settlement of the Americas: A comparison of the linguistic, dental and genetic evidence. *Curr. Anthropol.* 25:477-497.

1987

Language in the Americas. Stanford, Calif.: Stanford University Press.

1990

On Language: Selected Writings of Joseph H. Greenberg, eds. K. Denning and S. Kemmer. Stanford: Stanford University Press.

1991

The last stages of grammatical elements: Contractive and expansive desemanticization. In *Approaches to Grammaticalization*, eds. E. Traugott and B. Heine, pp. 301-314. Amsterdam: John Benjamins.

2000

[1] *Indo-European and Its Closest Relatives: The Eurasiatic Language Family*, vol. 1, Grammar. Stanford, Calif.: Stanford University Press.

[2] From first to second person: The history of Amerind *k(i). In *Functional Approaches to Language, Culture and Cognition*, eds. D. G. Lockwood, P. H. Fries, and J. E. Copeland, pp. 413-425. Amsterdam: John Benjamins.

2002

Indo-European and Its Closest Relatives: The Eurasiatic Language Family, vol. 2, Lexicon. Stanford: Stanford University Press.

2005

Genetic Linguistics: Essays on Theory and Method, ed. W. Croft. Oxford: Oxford University Press.

JAMES BENNETT GRIFFIN

January 12, 1905–May 31, 1997

BY HENRY T. WRIGHT

James bennett griffin was one of the leading North American archaeologists of his day. Known to everyone—even his children—as Jimmy, he was the man most responsible for reshaping the archaeology of eastern North America, for building an enduring center of research on long-term cultural change at the Museum of Anthropology of the University of Michigan, and for fostering many innovations in archaeological method and theory throughout his long career.

Born in Atchison, Kansas, and raised in Denver, Colorado, and Oak Park, Illinois, Griffin was steeped in the traditions and perspectives of the American Midwest, the land to whose prehistory he brought systematic order. He received his bachelor of arts from the University of Chicago in 1927. He gained excavation experience in the Illinois field school of the polymathic anthropologist Faye Cooper Cole in the summer of 1930 while working in Fulton County near Peoria, and this fieldwork led to one of his first publications (1934). Later that year he received a master of arts with a thesis on mortuary variability in eastern North America.

There were few posts open for young archaeologists in the tumultuous first years of the Great Depression. Griffin sought research positions in Pennsylvania, Hawaii, Guatemala, and Iraq with varying success. In 1932, however, Griffin was fortunate to find support as a research fellow in charge of the North American ceramic collections at the University of Michigan's Museum of Anthropology, which was directed by Carl Guthe. His fellowship was funded by the pharmaceutical entrepreneur Eli Lilly, an Indiana native fascinated by American Indian cultural traditions.

In 1936 Griffin married Ruby Fletcher in the University of Chicago chapel. They raised three sons—John, David, and James C.—in Ann Arbor and traveled widely together. Their long and productive marriage ended with Ruby's death in 1979.

Up until the mid-1940s there was little appreciation of how long the Americas had been occupied. Archaeological assemblages were often ascribed to late ethnic groups mentioned by early European explorers. This approach had broken down as more and different assemblages were found in each subregion. Griffin joined those who argued for the purely archaeological classification of material, without reference to putative ethnic groups mentioned in historic accounts and travelers' reports. Samples of well-excavated ceramics from meaningful contexts—at first from excavations occasioned by federal reservoir construction in the Tennessee Valley and then from other Depression-era projects—came to Michigan's Ceramic Repository for description and classification. With Lilly's funding Griffin drove from project site to project site studying ceramics in the field and making suggestions to excavators. Griffin brought order to the mountains of sherds with a binomial system in which larger groupings based on clay body and inclusions were subdivided into smaller groupings based on surface treatment and decoration; this improvement produced not only precise descriptive studies but also became the basis of Griffin's 1938 doctoral dissertation at the University of Michigan. That was but the first of many syntheses of the prehistory of eastern North America (1946) based on ceramic sequences and correlations. The binomial system ultimately developed into the type-variety approach to ceramics used throughout the Americas today.

Just as ceramics could be formally classified in hierarchical taxonomies, so could entire material assemblages. Griffin became a partisan of the Midwest Taxonomic System (McKern, 1937) and produced its finest exemplification, a study of the latest prehistoric sites of the middle portion of the Ohio River drainage. The trait lists from individual sites were compared, sites with similar assemblages were grouped into a focus, and the foci of this region were grouped into a Fort Ancient Aspect, an element in a broader Mississippian Pattern. Only after formal classification did Griffin (1943) consider the chronological, sociological, and ethnic affiliation of these units.

In 1940 and 1941 Griffin joined Philip Phillips of Harvard University and James A. Ford of the American Museum of Natural History (New York) to undertake an archaeological survey of the lower valley of the Mississippi River. Hundreds of sites were systematically recorded and the recovered ceramic fragments, classified by Griffin and Phillips, were grouped into sequences of chronological units using statistical and graphical techniques developed by Ford (Phillips et al., 1951). Griffin and Phillips attempted to assign an absolute chronology to their lower valley sequence based on the association of sites with prehistoric meandering channels of the Mississippi River, to which absolute dates had been ascribed based on changes evident on dated maps from the past three centuries (Fisk, 1945). As it did not account for changes in climate and hydrology during the Holocene, this approach yielded dates that later proved to be too young, which led to the incorrect assessment that rates of cultural change were relatively rapid.

In 1946 Griffin was appointed director of Michigan's Museum of Anthropology, a post he was to occupy for almost three decades. In 1949 he became a professor in the Department of Anthropology. The postwar years saw an expansion of archaeology within new anthropology departments. Griffin used Michigan's Department of Anthropology to provide advanced academic training to archaeologists already experienced in the Depression-era programs or in salvage archaeology occasioned by postwar pipeline, highway, and reservoir construction so they could fill newly established posts.

With the limited resources a museum director could assemble, Griffin turned to unresolved problems in archaeological research. The first of these was the issue of absolute chronology. Before 1949 the dating of prehistoric sites depended on tenuous correlations across the Great Plains to the southwestern U.S. cultures dated by the newly developed tree-ring or dendrochronological method or on geological arguments. Griffin was well aware of the promise of Willard F. Libby's work on radiocarbon dating at the University of Chicago, and he provided Libby with some Eastern Woodland samples. When he received the results, Griffin was puzzled that the age determinations made in Chicago were in several cases the reverse of what he expected. He and his colleague in physics, H. R. Crane, were convinced that the problems had two sources: the imprecision of Libby's technique of measuring the radioactivity of solid carbon and the use of samples that had been contaminated during the excavation and/or during the time they were in storage at the museum. Crane built his own lab, which accepted only samples that met Griffin's standards of unambiguous context, which pretreated samples as carefully as then current knowledge permitted, and which measured the radioactivity of gaseous carbon dioxide rather than solid carbon. In its years of operation more than 2000 age determinations were made and published, mostly in the journals *Science* and *Radiocarbon*. It was shown that the archaeological sequences proposed in Griffin's various syntheses were correct but that the time spans involved were longer than suspected. The lab also pioneered the dating of Formative cultures of Central and South America, the very early Jomon ceramics of Japan, and materials from many other areas.

North American archaeologists had long discussed cultural contacts between Mexico and the Mississippian cultures, bringing such crops as maize and beans as well as social patterns and symbolic representations to the Mississippi Valley. In 1946 Griffin spent six months in Mexico working with Eduardo Noguera, then director of the Museo de Antropología in Mexico City, Miguel Covarrubias, Alfonso Caso, Ignacio Bernal, Antonieta Espejo, and other Mexican scholars. Griffin studied collections, visited sites, and applied his binomial method to Mesoamerican ceramics (1947). He became, however, less and less convinced that direct contacts existed between Mesoamerica and the U.S. Southeast.

As editor of a massive festschrift for his mentor Cole, *The Archaeology of the Eastern United States* (1952), Griffin oversaw the ordering of much of the cultural evidence from the entire region in terms of McKern's scheme but given a chronological dimension not only from classical stratigraphic evidence but also from new statistical techniques and from radiocarbon dating. The "Green Bible," as it was termed by generations of graduate students and colleagues, went through five printings and remains a useful reference to this day.

It was during this period that interests in the Siberian roots of North American cultures led Griffin to travel periodically to Western Europe and in 1961 to visit Poland and Russia. He demonstrated to his satisfaction that while Siberian cultures had an impact on Alaska ceramics, centers of ceramic innovation farther south were independent (1960, 1970); he indefatigably visited sites and museums and learned much about the new European approaches to studying the environmental contexts of archaeological sites. He made many friends, launched collaborative projects in Poland and then Yugoslavia, and became a U.S. representative to the International Union of Pre- and Protohistoric Sciences, for many years serving on its Executive Committee.

In the later 1950s, with the basic framework of North American prehistory well established, Griffin turned to the problem of understanding cultural change, particularly the impact of environmental change on human communities, which he viewed in rather direct cause-and-effect terms. He planned research on this problem with Albert Spaulding in the Great Lakes region, where the uplift of Holocene beaches had left magnificent archaeological landscapes available for study. That proposal received one of the first National Science Foundation grants ever awarded to an archaeology project. In this research he could draw on Michigan's geologists and paleobotanists, on the museum's own strong Laboratory of Ethnobotany under Volney Jones, and on an energetic generation of graduate students. The specifics of the field research were largely in the hands of Lewis Binford and Mark Papworth. The resulting influential studies of human ecology (Cleland, 1966; Yarnell, 1964), artifact variability (Binford, 1963), and social organization (McPherron, 1967) mark a transition toward a new approach to archaeology in North America.

Foreseeing the accelerating changes within the field, Griffin transformed the Museum of Anthropology from an institution focused on North American culture history to an institution that continues to conduct research on cultural evolution throughout the world. Beginning in the mid-1960s, he added curators with research interests in Mesoamerica and the Andes, Europe, and the Near East. The long-standing program in ethnobotany was complemented by others in ethnozoology and human biology. Individuals with strong skills in statistical analysis and computerized data management replaced the departed Spaulding. If his museum in Ann Arbor became a center for new developments toward a processual archeology, however, Griffin was not about to shirk his responsibilities as an intellectual patriarch. He made it plain that he saw little value in evolutionary or behavioral theory. Ever supportive with resources and requests for time away for field research, he was firm in his criticism of what he saw as overblown or patently wrong theory, inadequate evidence, or impolite behavior.

Griffin's work with the material remains of Eastern Woodlands cultures, both the Mississippian peoples and the preceding Woodland peoples, particularly the Hopwellian florescence of the first few centuries of our era, revealed many possible cases of trade in unusual raw materials. His first effort to track the import of obsidian into the Midwest in Hopewell times (1965) led him to search for more precise methods of source identification. Working with the newly developed technique of neutron activation analysis, Griffin and Adon Gordus (a member of the University of Michigan's Department of Chemistry) succeeded in characterizing the trace elements in obsidian sources and archaeological samples from all over the world, and definitively established that Hopewell obsidian originated in Yellowstone Park, Wyoming (1969). What social mechanisms facilitated the transport of obsidian from the Rocky Mountains to Ohio remains unresolved to this day.

By the early 1970s Griffin was deeply involved in a project designed to provide data adequate to evaluate ideas about the classification of the major communities of the Mississippian culture as chiefdoms, an idea that he regarded with deep skepticism. It seemed logical to him that only a strategy combining the complete settlement excavation (used previously only in a few salvage projects in eastern North America) with detailed plotting of artifacts in and around houses and screening and floating for subsistence remains, could show the enduring differences in social rank thought to characterize chiefdoms. In southeastern Missouri, James Price, then a student at the University of Missouri, had discovered a series of Mississippian villages burned after only a few years of occupation. Griffin obtained funds for a near complete excavation of two hamlets, two villages, and part of the ceremonial center of the Powers Phase (1979). Final analysis of these excavations by a team under Bruce Smith of the Smithsonian Institution is nearing completion. The massive interstate highway program gave archaeologists trained in the Powers Phase project and many others the opportunity to apply the same approach of complete excavation and intensive debris sampling to the hamlets and centers of the greatest of the Mississippian societies, that at Cahokia near modern St. Louis, where Griffin sponsored excavations as long ago as 1950. In his own overview of his career Griffin (1985) makes little of his contribution as an adviser to the later work at Cahokia, but his stamp not only on the names of pottery types and cultural phases but also on the basic research approach—the excavation of whole communities and analysis and reporting of every aspect of the material remains—continues to be profound. The prompt publication of almost 20 detailed monographs on this work is due in no small part to his encouragement. Perusal of the recent overviews edited by Timothy Pauketat and Thomas Emerson (Pauketat and Emerson, 1997) and written by George Milner (Milner, 1998) or a visit to the magnificent interpretive center at Cahokia itself is certain to fascinate any serious scholar of archaeology.

During his long career Griffin received many honors. He received the Viking Fund Medal from the Wenner-Gren Foundation in 1957. He was elected to the National Academy of Sciences in 1968. In 1971 he received an honorary doctorate from Indiana University. From the Society for American Archaeology, of which he was a founding member, he received the Fryxell Award for Environment and Archaeology in 1980 and the Distinguished Service Award in 1984.

In his last years Griffin was a Regents' scholar at the Smithsonian, working on synthetic articles and overviews of conferences, both with the humor and the acerbic criticism for which he was famous. Moreau Maxwell (Maxwell, 1977, p. xi) once described Griffin as follows: "With a remarkably retentive mind, back-stopped by voluminous cross-indexed files, he has been quick to pick up, reassemble, and make useful to students of prehistoric behavior a myriad of devices, techniques, and data gleaned from his eclectic contacts" and "from what was, in the thirties, a chaotic assemblage of discrete variables, particularly in the prehistoric treatments of clay, he was able to store vast numbers of these variables, from them to abstract the key ones, and to see the relationships to similar key variables over hundreds of miles of space." Many remember best, however, his inimitable ability to pause, to look at you, and leave you thinking about the issue in a completely new way, with hardly a word spoken.

James Bennett Griffin died quietly in his sleep in Bethesda, Maryland, in the loving company of his wife, Mary Marsh Dewitt Griffin, and his sons and their families on May 31, 1997.

Today the destruction of our limited and irreplaceable archaeological record throughout the world by new agricultural technologies and suburban sprawl is vastly worse than the destruction wrought by reservoirs, pipelines, and roads in Griffin's time. Future archaeologists will have a basis for evaluating new theories of cultural change in human history because of the eastern North American collections Griffin assembled and so patiently catalogued, the chronological framework to which he contributed so much, and the standards of rigor he imposed in the assessment of evidence throughout his life. If Griffin were speaking today, he would decry the destruction of sites, fight for the integrity of museums and university programs, assiduously seek to increase funding for fieldwork (still limited given the scale of the challenges), and sharply criticize any theoretical construct that was unsupported by hard evidence. His contributions are exemplary accomplishments, deserving of emulation by future generations.

THE FOREGOING PROFITED FROM Griffin's own writings, from unpublished assessments by Richard Ford and Jeffrey Parsons, from discussions with many of his friends and family members, and from the editorial skills of Joyce Marcus. An earlier version appeared in the British journal Antiquity (Wright, 1998). The errors and deficits are entirely my own.

REFERENCES

Binford, L. R. 1963. The Pomranky Cache. In Miscellaneous Studies in Typology and Classification. Anthropological Papers No. 1, eds. A. Montet-White, L. R. Binford, and M. L. Papworth. Ann Arbor, Mich.: Museum of Anthropology.

Cleland, C. E. 1966. Prehistoric animal ecology and ethnozoology of the upper Great Lakes., Anthropological Papers No. 29. Ann Arbor, Mich.: Museum of Anthropology.

Fisk, H. N. 1945. Geological investigation of the alluvial valley of the lower Mississippi River. Mississippi River Commission, United States Army Corps of Engineers. Vicksburg, Miss.: M.R.C. Printing.

McKern, W. C. 1937. The Midwestern taxonomic system. *Am. Antiquity* 3(2):138-143.

McPherron, A. 1967. *The Juntunen Site and the Late Woodland Prehistory of the Upper Great Lakes Area.* Anthropological Papers No. 30. Ann Arbor, Mich.: Museum of Anthropology.

Milner, G. 1998. *The Cahokia Chiefdom.* Washington, D.C.: Smithsonian Institution Press.

Pauketat, T. R., and T. E. Emerson. 1997. *Cahokia: Domination and Ideology in the Mississippian World.* Lincoln: University of Nebraska Press.

Phillips, P., J. A. Ford, and J. B. Griffin. 1951. *Archaeological Survey in the Lower Mississippi Valley 1940-47.* Papers of the Peabody Museum of American Archaeology and Ethnology, vol. 25. Cambridge, Mass.: Harvard University.

Price, J. E., and J. B. Griffin. 1979. *The Snodgrass Site of the Powers Phase of Southeast Missouri.* Anthropological Papers No. 66. Ann Arbor, Mich.: Museum of Anthropology.

Smith, B. D. 1978. *Prehistoric Patterns of Human Behavior: A Case Study in the Mississippi Valley.* New York: Academic.

Wright, H. T. 1998. James Bennett Griffin 1905-1997. *Antiquity* 72(275):10-13.

Yarnell, R. A. 1964. *Aboriginal Relations between Culture and Plant Life in the Upper Great Lakes Region.* Anthropological Papers No. 23. Ann Arbor, Mich.: Museum of Anthropology.

SELECTED BIBLIOGRAPHY

1934

Archaeological remains in Adams County, Illinois. *Illinois State Acad. Sci.* 2:97-99.

1935

Aboriginal methods of pottery manufacture in the eastern United States. *Penn. Archaeol.* 5:19-24.

1936

The Cultural Significance of the Ceramic Remains from the Norris Basin. Ph.D. dissertation. University of Michigan.

1938

The ceramic remains from Norris Basin, Tennessee. In *An Archaeological Survey of the Norris Basin in Eastern Tennessee,* ed. W. S. Webb. Bull. Am. Ethnol. 118:253-358.

1939

Report on the ceramics of Wheeler Basin. In *An Archaeological Survey of Wheeler Basin on the Tennessee River in Northern Alabama,* ed. W. S. Webb. Bureau Am. Ethnol. Bull. 122:127-165.

1941

Additional Hopewell material from Illinois. *Indiana Historical Society Prehistory Research Series* 11(3):165-223.

1942

Adena pottery. *Am. Antiquity* 7:344-358.

1943

The Fort Ancient Aspect: Its Cultural and Chronological Position in Mississippi Valley Archaeology. Ann Arbor: University of Michigan Press.

1944

The Iroquois in American prehistory. *Papers of the Michigan Academy of Science, Arts, and Letters* 29:357-375.

1945

An interpretation of Siouan archaeology in the piedmont of North Carolina and Virginia. *Am. Antiquity* 10:321-30.

1946

Cultural change and continuity in eastern United States archaeology. In *Man in Northeastern North America*. Archaeology Paper No. 3, ed. F. Johnson, pp. 37-95. Andover, Mass.: Robert S. Peabody Foundation.

1947

With A. Espejo A. La alfarería correspondiente al último período de ocupación nahua del Valle de Mexico, Part I. In Tlatelolco a través de los tiempos. In *Academia Mexicana de la Historia correspondiente de la Real de Madrid. Memorias.* Tome 6: 131-147.

1950

With A. Espejo A. La alfarería correspondiente al último período de ocupación nahua del Valle de Mexico, Part II. In Tlatelolco a través de los tiempos. In *Academia Mexicana de la Historia correspondiente de la Real de Madrid. Memorias.* Tome 9: 118-167

1951

With P. Phillips and J. A. Ford. Archaeological Survey in the Lower Mississippi Valley, 1940-1947. *Papers of the Peabody Museum of American Archaeology and Ethnology, vol. 25.* Cambridge, Mass.: Harvard University.

1952

Archaeology of the Eastern United States. Chicago: University of Chicago Press.

1960

Some prehistoric connections between Siberia and America. *Science* 131(3403):801-812.

1969

With A. A. Gordus and G. A. Wright. Identification of the sources of Hopewellian obsidian in the Middle West. *Am. Antiquity* 34:1-14.

1970

Northeast Asian and northwestern American ceramics. *Proceedings of the 8th International Congress of Anthropological and Ethnological Sciences* 3:327-30. Tokyo/Kyoto: Science Council of Japan.

With R. E. Flanders and P. F. Titterington. *The Burial Complexes of the Knight and Norton Mounds in Illinois and Michigan.* Memoir No. 2. Ann Arbor: University of Michigan Museum of Anthropology.

1971

The study of early cultures. In *Man, Culture, and Society*, ed. H. L. Shapiro, pp. 22-46. Oxford: Oxford University Press.

1974

The ceramic affiliations of the Ohio Valley Adena culture. In *Adena People*, eds. W. S. Webb and C. E. Snow, pp. 220-246. Knoxville: University of Tennessee Press.

1979

With J. E. Price. *The Snodgrass Site of the Powers Phase of Southeast Missouri.* Anthropological Papers No. 66. Ann Arbor, Mich.: Museum of Anthropology.

1980

The Mesoamerican-Southeastern U.S. connection. *Early Man* 2(3):12-18.

1985

An individual's participation in American archaeology, 1928-1985. *Annu. Rev. Anthropol.* 14:1-23.

MALCOLM ROBERT IRWIN

March 2, 1897–October 12, 1987

BY RAY D. OWEN

M. R. ("BOB") IRWIN DEVOTED his scientific research life to two related areas. First, contrary to what he believed to be the prevailing opinion when he began, he maintained that genetic susceptibility or resistance of the host affects the processes of infection by a pathogen. Inventing the term "immunogenetics," he became recognized as a pioneer in that vital field, a leader over many decades. Second, he reasoned that antibodies provide tools for defining antigens segregating as inherited variations within and among species. His assumed one gene-one antigen concept developed insight into evolutionary relationships difficult to assess in other ways. Working at first with pigeons and doves, he and his group extended their studies to domestic birds and animals, into areas of important agricultural concern. As a leader he achieved important goals for the University of Wisconsin and for science in the nation and the world.

Born in Artesian, South Dakota, three-year-old Bob Irwin moved in 1900 with his family to an Iowa farm near the town of Ireton. He attended a country school but transferred at sixth grade to the larger school in Ireton, where there were six in his graduating class. "My father," he wrote, "died when I was 15 years old, and since each of the three children wished to attend college, I spent three years at work to acquire enough money for a part of the cost of a college education."[1] He valued growing up on a family farm in a rural area, learning about work, industry, and efficiency on the farm and making lifelong friends in the community.

In 1916 he entered Iowa State College but remembered that on graduation in 1920 no interest in natural science had been awakened by his undergraduate experience. He liked reading, history, and mathematics, but his main interest was baseball. Later in life handball and tennis provided regular, welcome relief from the stresses of work.

On graduating from Iowa State and faced with uncertainty of what career path to follow, he chose to spend three years at the American Farm School at Salonika in Greece. That experience stimulated thoughts of using scientific procedures to improve farm animals and plants. Returning to Iowa State in 1924, he began graduate studies to that end, inspired especially by Professor E. W. Lindstrom of the Department of Genetics. It was then that his first area of lifetime interest took form. In the main part of his Ph.D. thesis research he reported that rats, surviving generally lethal induced infections with *Salmonella enteritidis*, produced progeny more resistant to the pathogen than the average of the original population. Surviving parents, therefore, passed on to their offspring a degree of inherited resistance. His first research paper briefly reporting this result was published in the *Iowa State College Journal of Science* in 1928. A long extension followed in *Genetics* in 1929.

He believed that the next step should be to determine the physiological basis for natural resistance by applying the methods of immunology and genetics. He was awarded a National Research Council Fellowship to study with W. E. Castle at the Bussey Institution of Harvard University, and for a second year, 1929, with L. T. Webster at the Rockefeller Institute for Medical Research in New York City. Although it resulted in no publication, the Rockefeller experience was definitive, especially his interactions with Karl Landsteiner, O. T. Avery, and Michael Heidelberger, whom he always admired. He had further thoughts of studying genes and their effects by immunological techniques, which he carried to the University of Wisconsin when he took a position there in the summer of 1930.

His appointment at Wisconsin was jointly with bacteriology and genetics, in the College of Agriculture and the U.S. Department of Agriculture Experiment Station. The Department of Genetics had been founded 20 years earlier by L. J. Cole, the first genetics department at an American university. E. W. Lindstrom, who was later to influence young Bob Irwin at Iowa State, had joined Cole at Wisconsin in 1919 but left for Iowa State in 1922, the year R. A. Brink came to Wisconsin. Irwin was therefore only the fourth member of the genetics faculty over its first two decades, and one of only three in residence in 1930.

The Genetics Department had originally been established in the College of Agriculture in the expectation that the emerging science of heredity would make important contributions to the productivity of farm animals and plants. However, Cole, its chair, was not at home in an agricultural environment; he was known as a basic scientist devoted to comparisons of species and using pigeons and doves rather than farm animals for his observations. When R. A. Brink joined the faculty in 1922, his appointment was partly to make the connection of genetics with agriculture, in his case cultivated plants, more realistic and valuable.[2] Irwin's appointment, too, was regarded as strengthening the practical basis of genetics, and his interest in the hereditary aspects of disease resistance seemed a good fit, especially when he turned his attention to brucellosis, contagious abortion in dairy cattle. The first of his lifetime areas of research was therefore well suited to the position at Wisconsin.

The second area found even better opportunity. Cole maintained extensive breeding collections of backcross and species hybrids of pigeons and doves. Irwin saw these populations as ideal for his goal of using antibody reagents to identify inherited antigens on blood cells as an approach to understanding the genetic basis of species relationships in evolution. The first set of a long series of papers was published in 1936, and achieved wide recognition.

Meanwhile, the study of genes and their physiological effects on disease resistance was proving difficult. Contagious abortion, caused by *Brucella abortus* in cattle, was a major concern in Wisconsin's dairy industry. In 1936 Irwin, with veterinarians B. A. Beach and F. N. Bell and in 1937 with E. W. Shrigley, published laborious studies on the bactericidal action of blood and the activity of serum complement without evident relation to variations in disease susceptibility. I became a graduate student under Cole in 1937, and was given the opportunity to earn for my education by working in Irwin's laboratory during the summer. I recall long hours with a hand-cranked Burroughs calculator and a sorter for punched cards, enumerating the various blood cells in differential counts and testing for correlations with *Brucella* infection. It was not work that stimulated intellectual enthusiasm, and the consequent publication, by Irwin and Bell in the *Journal of Infectious Diseases* in 1938, escaped notice of my routine part in the analysis. There was no significant correlation in the proportions of the various types of leukocytes with resistance or susceptibility, or with any aspect of reaction to the infection. Irwin's regretful conclusion in 1951 that "there is at present no known substance in the blood which may be used as an index of the response to an infection of a normal or immunized animal"[1] had to await other approaches, based on molecular genetics of the immune system. In those later approaches he was to play no part.

Working in the second main field of his interests, using antibodies to define inherited cellular antigens in species comparisons, proved to be much more rewarding. The initial approach was straightforward: Blood from a species of dove injected into a rabbit produced an antiserum that reacted with cells from the donor species. It also reacted with cells of related doves. But when the antibodies that reacted with, for example, the related ring dove were removed, there remained antibodies specific for the original donor. These donor-specific antigens were individually recognizable when, in Cole's collection of backcross hybrids, genetic segregation and assortment had separated one from another. Irwin could conclude that any particular antigen, say d-1 of the pearlneck dove, was a unit if all of the backcross hybrids having it reacted to the same antibodies. Under the one gene-one antigen hypothesis this reflected a gene in the pearlneck dove distinguishing it from other doves. Another antigen, to be labeled d-2, could be similarly recognized, independent both serologically and genetically of d-1. His 1939 paper in *Genetics* listed nine such units distinguishing pearlneck from ring doves, and two others not yet fully defined. Other papers over that interval, most with Cole as a coauthor, reported similar studies with other species. Irwin received the Daniel Girard Elliot Medal of the National Academy of Sciences in recognition of that work.

The idea that the cellular antigens were closely related to their corresponding genes was based mainly on the absence of gene interaction in their appearance; one gene-one antigen was the rule. The gene-antigen effect was expressed without modification by developmental or environmental factors—a strikingly direct relationship. In his 1939 paper Irwin quoted J. B. S. Haldane's 1937 suggestion that "the gene is a catalyst making a particular antigen, or the antigen is simply the gene or part of it let loose from its connection with the chromosome."[3] But as early as 1932 Irwin had encountered a clear exception to the one-to-one relationship. When an antiserum to the cells of a species hybrid was absorbed to remove all of the antibodies to which either parent reacted, there remained antibodies specific only for the hybrid. This hybrid substance reflected the interaction of genes from the parent species and was not the direct result of a gene in either of them. In 1976 Irwin recalled Haldane's reaction on being told of the hybrid substance: "There goes a beautiful theory exploded by a single fact." Again, real understanding of genes and their actions had to await later developments by others in molecular genetics and immunology.

The extension of Irwin's program into studies of inherited individual similarities and differences in farm animals and birds became the prime lasting source of his laboratory's preeminence. In a herd of dairy cattle kept for the studies of contagious abortion, blood from one cow could be injected into another. This gave rise to antibodies specific for inherited antigenic differences segregating within the species. With L. C. Ferguson, a veterinarian working postdoctorally in his laboratory, and graduate student C. Stormont, Irwin published in the 1942 *Journal of Immunology* the definitive follow-up of the initial publication on the immunogenetics of cattle blood cell antigens. Two dairy cattle breed associations, the Holstein-Friesian Association and the American Guernsey Cattle Club, saw very practical uses for this work. For example, a purebred cow bred to a purebred bull could produce a purebred, registered calf, but if the sire of the calf was in question, the calf could not be registered and was less valuable. Blood tests in Irwin's lab could offer reliable evidence in cases of questionable paternity. About this time artificial insemination from selected bulls began to play a large role in the improvement of dairy cattle. Blood tests could now identify the progeny of these bulls when questions arose. The tests became a vehicle for individual identification.

The support of the breed associations, in those days before the National Institutes of Health and other sources of grants, greatly implemented the work. When I took up postdoctoral work in the laboratory in 1941, it was the cattle program I joined, with Stormont as my mentor. We provided paid services to the breed associations, and in the process collected a great deal of information from the blood samples they shipped to us, often including whole herds and large families of cattle, ideal for our basic genetic and immunological studies.

Others in the laboratory initiated extensions of the methods to chickens, ducks, swine, and sheep; we even studied bison. The Wisconsin laboratory became an internationally recognized resource for research and training in the immunogenetics of domestic animals. The many younger people who passed through his laboratory and the Department of Genetics—undergraduate and graduate students, postdoctorals, and participants from all over the world, and fellow faculty members—remember "his concern for the healthy growth of science and his innate generosity—a loyal friend and colleague."[4] My own recollections include the memory that he was not, in a formal sense, a particularly good lecturer. "When I took my first Genetics course at Wisconsin in 1937, Irwin was the teacher. His first lecture was largely a detailed listing, written on the blackboard, of genera and species of birds and the results of crosses among them. My lecture notes have a marginal comment: 'If you ever teach Genetics, don't start this way.'"[5] But "he was exceedingly loyal to the University and to the genetics program. His was a participating loyalty, not lip service … He was modest … with an unfailing pleasant manner and sense of humor."[6] Elected to the National Academy of Sciences in 1950, he succeeded R. A. Brink as chair of the Genetics Department in 1951. Irwin's period in that position, to 1965, was marked by a great expansion of the department, including a new building completed in 1963. He was involved in bringing several distinguished scientists to the genetics faculty, including Sewall Wright and Joshua Lederberg, among others. Outside the university he served as treasurer, vice president, and president of the Genetics Society of America, and in active roles in other societies and on editorial boards of several journals. Not a seeker for honors, he nevertheless was honored by the Royal Swedish Academy of Agriculture, American Society of Animal Science (the Morrison Award), and Deutsch Gesellschft f. Zuchtungskunde (the H. von-Nathusius Medal). At the time of his death he was survived by his wife, Margaret ("Peggy"); his daughter, Harriet Anne; his son, Joseph Robert; and four grandchildren.

When Bob Irwin was elected to the National Academy of Sciences, there was no Genetics Section. He, R. A. Brink, and others worked to have the emerging discipline of genetics recognized with its own section, and in the early 1960s their efforts bore fruit. Irwin served as de facto chair of the new Section 26 until an elected chair could be installed. All through his life until very near the end, he continued to serve others in many unselfish ways, and he deserves to be long remembered for that, as well as for his research and other professional achievements. He disappears into the past, but his influence spreads widely, becoming increasingly dilute with time.

NOTES

1. A manuscript copy of Irwin's autobiographical sketch was submitted to the National Academy of Sciences in 1951. The quotation is from that document.
2. O. E. Nelson and R. D. Owen. Royal Alexander Brink, 1897-1984. In *Biographical Memoirs*, vol. 66, p. 8. Washington, D.C.: National Academy Press, 1995.
3. J. B. S. Haldane. The biochemistry of the individual. In *Perspectives in Biochemistry*, eds. J. Needham and D. Green, pp. 1-10. Cambridge: Cambridge University Press, 1937.
4. W. H. Stone. In memoriam, M. R. Irwin 1897-1987. *Immunogenetics* 30(1989):1-4.
5. R. D. Owen. M. R. Irwin and the beginnings of immunogenetics. *Genetics* 123(1989):1-4.
6. J. Adler, J. F. Crow, O. Nelson, and R. M. Shackelford. Memorial Resolution of the Faculty of the University of Wisconsin-Madison. On the Death of Emeritus Professor Malcolm R. Irwin. 1987.

SELECTED BIBLIOGRAPHY

1928

The inheritance of resistance to the Danysz bacillus in the rat. *Iowa State Coll. J. Sci.* 2:213-218.

1931

With T. P. Hughes. Differences in bactericidal power of the blood within an inbred strain of rats. *Proc. Soc. Exp. Biol. Med.* 28:295-297.

1932

Dissimilarities between antigenic properties of red blood cells of dove hybrid and parental genera. *Proc. Soc. Exp. Biol. Med.* 29:850-851.

1936

With B. A. Beach and F. N. Bell. Studies on the bactericidal action of bovine whole blood and serum towards *Brucella abortus* and *Brucella suis*. *J. Infect. Dis.* 58:15-22.

With R. T. Hill. Parabiotic twins as a means of determining cellular individuality. *Proc. Soc. Exp. Biol. Med.* 33:566-568.

With L. J. Cole and C. D. Gordon. Immunogenetic studies of species and of species hybrids in pigeons, and the separation of species-specific characters in backcross generations. *J. Exp. Zool.* 73:285-308.

1937

With E. W. Shrigley. On the differences in activity of serum complement from various animal species. *J. Immunol.* 32:281-290.

With L. J. Cole. Immunogenetic studies of species and of species hybrids in doves; the separation of species-specific substances in the second backcross. *J. Immunol.* 33:355-373.

1938

Immuno-genetic studies of species relationships in Columbidae. *J. Gen.* 35(3):351-373.

With F. N. Bell. The interrelationships of the blood cells of cattle in health and during *Brucella* infections. *J. Infect. Dis.* 63:263-268.

1939

A genetic analysis of species differences in Columbidae. *Genetics* 24:709-721.

1940

With R. W. Cumley. Speciation from the point of view of genetics. *Am. Nat.* 74(752):222-231.

1942

With R. W. Cumley. Immunogenetic studies of species: Segregation of serum components in backcross individuals. *Genetics* 27(2):177-192.

With L. C. Ferguson and C. Stormont. On additional antigens in the erythrocytes of cattle. *J. Immunol.* 44(2):147-164.

1943

With R. W. Cumley. Interrelationships of the cellular characters of several species of *Columba*. *Genetics* 28(1):9-28.

1944

With R. W. Cumley. The correlation between antigenic composition and geographic range in the Old or New World of some species of Columba. *Am. Nat.* 78:238-256.

With R. D. Owen and C. J. Stormont. Differences in frequency of cellular antigens in two breeds of dairy cattle. *J. Anim. Sci.* 3(4):315-321.

1945

With L. J. Cole. Evidence for normal segregation of species-specific antigens in the backcross of species hybrids in doves. *Genetics* 30(6):487-495.

1946

Antigens, antibodies and genes. *Biol. Rev.* 21:93-100.

1949

Immunological studies in embryology and genetics. *Q. Rev. Biol.* 24(2):109-123.

1959

With W. J. Miller. Interrelationships and evolutionary patterns of cellular antigens in Columbidae. *Evolution* 15:30-43.

1962

With J. Palm. Interaction of nonallelic genes on cellular antigens in species hybrids of Columbidae. *Genetics* 47:1409-1426.

1965

With M. S. Osterhoudt. An embryo-specific red cell antigen in a dove species, *Streptopelia risoria. Vox Sang.* 10:493-505.

1966

Interaction of nonallelic genes on cellular antigens in species hybrids of Columbidae. III. Further identification of interacting genes. *Proc. Natl. Acad. Sci. U. S. A.* 56:93-98.

1976

The beginning of immunogenetics. *Immunogenetics* 3:1-13.

Photo Courtesy Marian Koshland Science Museum

Marian E. Koshland

MARIAN ELLIOTT KOSHLAND

October 25, 1921–October 28, 1997

BY RUTH LEVY GUYER

MARIAN ELLIOTT KOSHLAND WAS AN eminent immunologist. She was spirited, practical, insightful, and inventive, and she had tremendous integrity, energy, and smarts. She was also a caring and generous person. I was fortunate to be one of her graduate students at the University of California at Berkeley in the early 1970s.

Marian had become a professor in the bacteriology and immunology department at Berkeley shortly before I joined her group. Her laboratory was in the old Life Sciences Building near the (in)famous eucalyptus grove, a campus landmark redolent of medicinal oils and site of commonplace conversations, licit and illicit trysts, and spurned lovers' fist fights.

Marian's laboratory was large and classic: Rows of tall, stately black benches spanned much of the width of the room, and at the back near the windows that looked out onto the building's courtyard was a hulking rectangular conference table. That table was where we—the four graduate students, the postdoc from Lausanne, the two technicians, and Marian—gathered every day at lunchtime to eat and to talk about J chain—the joining protein of immunoglobulin molecules that Marian & Co. had recently identified and were characterizing—the structural peculiarities of secretory antibodies and their distinctive activities, the immune system's newly distinguished T and B cells, our families, and politics.

George McGovern was running for president, and one graduate student was taking large chunks of time off to run McGovern's California campaign. America was still involved in Vietnam. Harvard's George Wald had recently returned from a fact-finding trip to China, and he was traveling around the country giving talks at colleges and universities—including ours—called "Acupuncture for McGovern" and donating his honoraria to the McGovern campaign. (Two vivid images from that talk have remained in my mind: Wald said that [1] after he watched a surgeon resect a grapefruit-size tumor from a woman who was awake and smiling throughout the surgery [smiling, thanks to the acupuncture needles], the surgeon had sent the tumor on a platter from the operating theater up to Wald in the observation gallery where he, like a client in a gourmet restaurant, had then sent down to the surgeon his admiring "compliments to the chef," and [2] just believing in acupuncture was not what made it work, and he had been persuaded of its value by watching surgery on a horse for whom acupuncture had been an efficacious pain-killing anesthetic.)

Everyone in Marian's lab, except me, was working on J chain. In a small room at the front of the lab, glass chromatography columns were sorting the constituent parts of J chains and dropping the fractions into humming fraction collectors. The starting materials—colostrum and myeloma protein preparations—retained the names of their sources. So, people would say, "Anne Good is on column two" or "so-and-so is on column one." Our laboratory's immunochemistry had a very human feel.

Marian arrived at the lab each day late in the morning and stayed until early evening. We all liked each other, and we ate lunch together every day. (The Swiss postdoctoral fellow and I alternated packing lunches for each other, so that we didn't each have to face the daily annoyance of preparing our own meals. She made more cosmopolitan sandwiches than I did, making me the key beneficiary of that collaboration.)

Marian cared about us. She was focused, of course, on our research but she also took a genuine interest in what else we were doing and what we were thinking about our futures. She would talk to us about what might best be labeled "juggling," helping us—the young women and the young men—look hard at how we each could combine meaningful careers with the sorts of overall lives we wanted to live. Two of the graduate students were married and had small children; two of the people in the lab were single, and three of us were married and hoped later to have children. We all were passionate about immunology, and Marian helped us understand that there was not just one path we might follow in order to have an interesting career in science.

Marian shared with us the lessons of her personal and professional experiences as well as her thought processes and her wisdom. She had interesting reflections on everything. She was a successful scientist at a time when that was not easy for women, she was the mother of five (with one son still at home, which was why she worked only part-time in those days), she was married to another successful scientist, and although she was wealthy by the time we were within her sphere, she had not grown up privileged.

She was born in New Haven, Connecticut, in 1921. Her mother—Margrethe Schmidt Elliott—was a teacher who had come to the United States from Denmark; her father—Walter Elliott—was a hardware salesman with a Southern Baptist background and the prejudices that came with it.

Marian had a little brother who developed typhoid fever when his big sister was just four years old. She pointed to her brother's illness as the first of several factors that contributed to the "sheer luck" that led her to eventually become an immunologist. While her baby brother was languishing in the hospital and her parents were holding a vigil at his bedside, Marian became the special project of two young girls who lived next door. They taught her to read and to do math, and then they took her to their school and let her "perform." This, she said, was a heady intellectual experience for a four-year-old (1996).

When Marian's brother got back home, the Elliotts quarantined both of their children for a year to protect their immunologically compromised son from the many childhood infections that were afoot in the town, the neighborhood, and the school. Marian's father, like the girls next door, tutored and drilled his daughter at home—she described his style as that of a martinet—and by the time she finally got to a regular classroom, she was well ahead of the other children and had great confidence in her innate ability to learn.

In high school Marian encountered her second piece of luck: Her closest friends—three boys—were intellectuals and sophisticates, and it was simply routine for her to do everything that the boys did academically and culturally. Academically that meant enrolling in the hardest courses in the school. Culturally her activities fell into the "high" version—attending productions of the Metropolitan Opera—and the "low"—holding a long black snake in the biology classroom and eating canned rattlesnake meat. She braved any challenge.

She attended Vassar College, where she supported herself with scholarships and jobs and lived in a co-op dormitory. She made all of her own clothing. She graduated from Vassar in 1942 with a B.A. degree in bacteriology. Her immuno-luck at Vassar centered on Catherine Dean, whose lectures about the fledgling field of immunology were intriguing. In addition, Dean expected her students to be creative, to pose questions, to design experiments, and to develop strategies for solving problems. Marian wrote that it was Dean who "hooked" her on immunology, and she marveled fifty-plus years later: "I look back now and wonder how, at age 19, I could have had the sense to select a field that has grown more exciting and intellectually challenging every year."

Money remained tight, but Marian wanted to leave the east coast. So, for post-baccalaureate work, she went to Chicago ($16 for the overnight train), where she spent one year in medical school before switching to research and a graduate program. She earned both her M.S. (in bacteriology in 1943) and her Ph.D. (in immunology in 1949) from the University of Chicago. She had no idea when she headed out to the University of Chicago that it was "a hub of wartime research, not only for the development of the atom bomb . . . but also for the control of infectious diseases among the troops."

Marian worked on two projects at Chicago. One involved the development of a vaccine for cholera, which was needed for protecting American soldiers who were stationed in the Far East. The vibrio that cause cholera reside in the gut, and thus getting rid of them was going to require an oral vaccine. (She pointed out that the successful cholera vaccine set a precedent that was important later in the development of the oral polio vaccine.) Proof of protection was associated with the production of fecal antibodies. Marian identified this research as the springboard for her lifelong interest in secretory antibodies, those antibodies that are made by cells in mucous membranes and are responsible for immune reactions at all of the body's orifices, and which in milk and colostrum protect newborns "passively" until their own immune systems kick in.

Her other project focused on finding ways to reduce the spread of respiratory diseases in soldiers during basic training. Strep infections were rampant, and the most susceptible soldiers were those who came from rural areas and had little exposure to strep on the farm. The sequelae to these infections were often serious—rheumatic fever, glomerulonephritis, and other autoimmune complications. The solution turned out to be more mechanical than medical—spraying oil droplets on bedding and clothing, so that fewer bacteria became airborne in the soldiers' barracks.

Marian met another graduate student in Chicago, Daniel E. Koshland, Jr. Work on the bomb took Dan to Oak Ridge in 1942, but they corresponded and often saw each other, and in 1945 they got married.

"Marrying her was by far the most important thing I did in my life," Dan told an interviewer at the University of California who conducted a series of interviews with colleagues and family members after Marian died and collected them into a thesis-style publication.[1] "Both of us being so mutually sufficient affected our life in many ways. As long as I could have an evening with her, that was it. I always enjoyed talking to her more than I enjoyed talking to anybody else."

Marian joined Dan at Oak Ridge in 1945 and spent a year there working on the Manhattan District Atomic Bomb Project. She was one of just a few women scientists who worked on the Manhattan Project—conducting research on the biological effects of radiation.

One of the wonderful stories that Marian told me about Oak Ridge illustrated how hush-hush everyone's work was at that time. Researchers had no idea what the person in the next room was doing. They also were forbidden to talk to their spouses about their research.

There wasn't much to do at night in Oak Ridge, and often couples would get together to play charades after dinner. One evening the women were pitted against the men, and someone had rigged the game such that Marian had to act out the word "plutonium." She was the only scientist on the women's team that night, and when her teammates finally guessed the word they still didn't know that they had solved the puzzle, because none of them had ever heard of plutonium. This was proof that the male scientists were not talking to their wives (or talking in their sleep).

Marian and Dan finished up in Chicago in 1949 and moved to Boston, where she had a postdoctoral fellowship in the Department of Bacteriology at Harvard Medical School (and Dan had a postdoctoral position at Harvard as well). Two years later they moved to Long Island, where both did research at the Brookhaven National Laboratory until they left for Berkeley in 1965.

When the Koshlands arrived at Brookhaven, the head of the department reneged on his promise of a job for Marian ("We are not going to have the wife of anybody."[2]) They had four children under five at the time and Marian seriously considered giving up science altogether. (The third "child" turned out to be twins, a situation Marian characterized as an "unexpected complication.") She wrote that her "luck" at that time was that Dan convinced her that she could remain competitive as a scientist by working part-time, by "being creative and undertaking high-risk projects that a tenure-track scientist could less afford to do" (1996).

Marian learned that the person in charge of the publications that followed the various Brookhaven symposia was a physicist who knew nothing about biology. She suggested that, in exchange for a small lab and a technician, she would edit the collections in biology for Brookhaven, and that was how she got back into the laboratory during that period.

James Allison, who was a colleague of Marian at Berkeley from 1984 until her death, characterized her 50-plus-year scientific career as "spectacular." He summarized her work decade by decade in a memorial eulogy, showing how Marian had made major contributions to understanding of the immune system for half a century:

As a graduate student at the University of Chicago in the 1940s, Bunny, as she was known, worked on a vaccine for Asiatic cholera. This work not only demonstrated the importance of mucosal antibodies in immunity but also led to her lifelong interest in the structure and origin of antibodies.

By the early 1950s and before the formal definition of antibody classes, Bunny had shown that secreted and serum-borne forms of antibodies were discrete molecules.

By the 1960s, she began to address one of the central problems in immunology—the origin of antibody specificity. There was a raging debate between instructive models, which held that antibody proteins were all the same and just fold around their target antigens, and selective models, which argued that they were the products of different cells. Bunny analyzed polyclonal antibodies directed against two different haptens, and on the basis of exquisitely careful amino acid composition analyses, convincingly showed that these antibodies had different amino acid compositions and therefore must differ in their amino acid sequences. These data had a profound effect on theories of antibody formation and how antibody specificity was generated. Legend has it that at the annual meeting of the American Association of Immunology where she first presented her data, her talk was received by a standing ovation—quite high praise indeed.

By the end of the 1960s, Bunny's work had become part of the mainstream of an emerging idea that is now one of the cornerstones of immunology, that is, that antigen receptors, both of T cells and B cells, are encoded by multiple rearranging gene segments. Her work in this area was seminal…

By the 1970s, Bunny had returned to her studies of secreted versus serum-borne antibodies. She identified a novel antibody subunit called the J chain, characterized it, and showed that it played a central role in antibody assembly and secretion and that the beginning of its expression marked a clear, discrete step in the maturation of B cells. This work led to the central theme of the remainder of her scientific career: understanding the way in which a B cell becomes an active player in the immune response.

In the late 1970s, Bunny did a sabbatical stay in David Baltimore's laboratory at MIT to learn molecular biology, as she felt that the future of the field lay in this area. While at MIT she collaborated in cloning the gene encoding the J chain, and brought the gene and her knowledge of the emerging technology of molecular biology back to the immunology group at Berkeley.

In the 1980s, Bunny turned her attention to regulation of transcription of the J chain gene by B cell growth factors.

By the 1990s, her work had extended to the more general area of events that accompany and direct B cell activation and maturation. In an invited talk at the national meeting of the American Association of Immunologists in February 1997, she presented a wonderful description of recent work from her lab demonstrating that the action of a transcription factor, BSAP, was very complex and dynamic, and that it could have both positive and negative effects, extinguishing some genes whose products were no longer needed, while turning on new genes with roles important to the emerging antibody-producing arm of the immune system. This talk was a marvel, and put together complex biochemical phenomena in an understandable context of biological function.

If there is any single feature that marked Bunny's work, it was her ability to reduce complex phenomena to experimentally addressable components. She did this by putting a very high emphasis on experimental rigor and absolute integrity. She was not affected by fads in science, but only by the bottom line—how well hypotheses hold up to hard experimental scrutiny.[3]

I felt fortunate that Marian—I never was comfortable calling my thesis adviser Bunny, although a number of her other students were—was not dazzled solely by faddish research. My project was something quite out of the mainstream of immunology in general and also different from what everyone else in the lab was doing. I was interested in how newborns absorb maternal antibodies during the period when they are nursing and before they have active immune protection of their own. Mouse myeloma proteins had recently become available, and I was feeding baby mice radioactively tagged populations of pure IgM and IgA and various subclasses of

IgG (as well as the Fc and Fab fragments of these immunoglobulins) and following their trajectories in the body in order to discover the locations and natures of the binding sites for these molecules. This biological system looked not at the production of secretory antibodies but at their uptake and absorption. Marian was totally supportive of my work and encouraged and guided my research as enthusiastically as she did the others' work on J chain.

She had high expectations of her students, and she guided us at every step of our training. She helped us become thoughtful and careful experimenters (the expression "if you don't have time to do it right this time, when will you have time to do it again?" was something of a mantra in our lab), she insisted that we write precisely and with integrity (She was an excellent and careful writer, although she said she found the task difficult. Her colleague Anne Good said of her publications—"She was very rigorous and meticulous. She would really think things through… Papers did not come out of her lab without her having thoroughly reviewed them."[1]) and she worked with us so that we would become confident speakers. I went with Marian to Atlantic City one year to present my Ph.D. research at the meeting of the Federation of American Societies for Experimental Biology. She had me rehearse my presentation for her several times and then she told me to practice it a few more times in my hotel room. I still recall saying my speech aloud in my empty, bargain-price hotel room and then being totally humiliated when the person next door clapped as I wrapped up my presentation. (For the rest of that week I slipped stealthily out of my room in order not to face the person who had been listening. When I later moved to the National Institutes of Health, I told that story to a colleague and he said, "Where were you staying. . . The Carolina Inn?" and I said, "How did you know that?" and he said, "It's the cheapest hotel in Atlantic City, that's where I always stay, and the walls are paper thin.")

I was not alone in thinking that Marian was a truly dream-come-true mentor. Chip Wilde, who was a graduate student at the same time I was and now teaches microbiology in Indianapolis at the University of Indiana, recently told me that he had gone to Berkeley to work with another professor, but after taking Marian's immunology course, he realized that immunology was what he wanted to do. Chip said, "There were so many unanswered questions, so many neat possibilities. And I loved to go to the lab every day. I liked the interplay of the people—I can still remember everyone. Marian was so meticulous and she was demanding but not tyrannical. I was in awe of her—her intellect, her work ethic, the way she felt it important to dot every i and cross every t. She felt that before you presented your work it was necessary to have everything wrapped up so there were no questions."

Students of other eras admired Marian too. I spoke to Marcy Blackman, an immunologist at the Trudeau Institute, who graduated from Berkeley more than a decade after Chip and I did. At the time Marcy was in Berkeley (she got her Ph.D. in 1985), Marian had just come back from her sabbatical at MIT. Marcy was eager to work with her because "she seemed to be the one who was moving the department into the future. I was totally impressed that at her age she would go on sabbatical and learn a new technology. I had a wonderful time in the lab. She always brought her lunch and half a candy bar (she said there were too many calories in a whole bar). We all sat around that table. I was strongly influenced by her—she was a woman, she had a family, and she managed to juggle it all and have status in the university. She taught me to be very critical. I was very inspired by her."

Other immunologists talked about Marian's high standards and acumen. James Allison said in his eulogy that "Bunny was well known for her impatience for and willingness to challenge half-baked ideas. I am sure that there are many immunologists who, like me, can recall times when we were forced to defend our hypotheses to this formidable devil's advocate. Bunny was not at all shy in attacking and probing every assumption, every finding, every control. Merely surviving an encounter with Bunny always gave me confidence that I could defend my ideas to anyone." Henry Metzger described her as "a very forceful, tough and clear-headed interlocutor but in a non-self-serving way."

Marian became the chairperson of the Department of Microbiology and Immunology in 1982 and remained the chairperson until 1989. She recruited prominent scientists for the department during that period (James Allison was one of them) and also made substantive changes that improved conditions for students in the department. In 1994 she became the head of the Graduate Affairs Office of the Department of Molecular and Cell Biology (the university had undergone a major rearrangement and that was where the immunologists were located) and that allowed her to continue working intensively on behalf of students.

Marian also developed a close relationship with Haverford College during the 1980s and 1990s. Both of her sons had done their undergraduate studies at Haverford, and in 1982 she became a member of Haverford's Board of Trustees and its Educational Affairs Committee. She stayed on the board until 1994. Elaine Hansen, who was provost of Haverford and one of the faculty members on the board during Marian's tenure and who is now president of Bates College, told me that Marian "connected us to the broader academic world and helped us see the college in that landscape. She was confident and forthright. She held the college to high standards. For her to take time to serve on the Board of a small college was a bit unusual for someone from a large university."

Judy Owen, an immunologist and biology professor at Haverford, knew Marian well both from her immunology research and from her work on the board. "She really cared passionately about the college," Judy said. "She was very impressive. The Board had people with different roles—academic, financial, etc. When I was the faculty representative to the Board, she would absolutely grill me . . . in all the right ways. She never overstepped the line of a Board member. She said what she thought and then she was very supportive. She gave a Philips lecture at Haverford and right before it she was pacing up and down, smoking, really nervous; but then she gave a lecture that was clear as a bell, blistering, and people were absolutely blown away by her."

"She was also stunning," Judy said, "and there was something absolutely appealing about that: you didn't have to give it all up to be a successful scientist—you didn't have to carry a pocket protector! She did elegant work and she was a dynamo. She had a firm reputation among the oldsters. I was inspired by her fearlessness—to learn whatever it took at whatever age. She was tough, had high expectations, and she had a real warmth for students. Her example affirmed for me that you can be tough and not hard—this was something I saw in her and wanted to emulate."

While Marian was on the board, the college developed a plan to bring all of the sciences under one roof in order to promote more interaction and collaboration among faculty members and students and more interdisciplinary research. Today the 140,000-square-foot science complex—the Marian E. Koshland Integrated Natural Sciences Center (KINSC)—houses Haverford's biologists, chemists, physicists, mathematicians, psychologists, astronomers, and computer scientists.

In 2001 I was invited by a chemistry professor at Haverford to give a talk about the work I was doing in bioethics, and by chance I was the first person to give a talk in the auditorium in KINSC. My host did not know that I had a connection with Marian, and I had not been aware of Marian's involvement with the college. It was touching, though, and seemed appropriate, for me to give my talk in her building. The next year I became a visiting professor at Haverford, and now in my sixth year there it gives me great pleasure to think about Marian as I walk by or into KINSC.

Haverford awarded Marian an honorary doctor of science degree in 1995. In 1998 the college established the Marian E. Koshland Prize in biology for a student showing excellence in research in biology.

Marian was the ideal professor for me. She was three dimensional in ways that others were not. Everything she did was well reasoned. She encouraged me to be resourceful, and she showed me ways to carve out an interesting life. (I, too, was married to another scientist, and we would be needing two positions in the same location. She proactively shared all of the lessons she had learned with me, and they have been invaluable to me over the years.)

Marian was the most rational person I'd ever met. For example, she explained how, having been surprised to have four children, she decided to go on and have a fifth. Her two oldest children were girls; the next two were a girl and boy—the twins. She worried about her son and decided that his life would be greatly improved if he had a brother and that the difference between having three sisters or four would not be significant. As it turned out, the gamble paid off, and her fifth child was a boy.

Another time she explained how she had solved a problem of painful cramps in her calf. She thought this might be a simple vitamin deficiency, so she began taking a daily multivitamin, and the cramps went away. Anyone else would have been satisfied with the pain relief, but Marian had to prove to herself that the vitamins were really causal in stopping the cramps. She stopped taking the vitamins and was pleased when her cramps came back.

Marian was funny. One night she came to my home for a small dinner party. One of my husband's relatives, a researcher from the National Institutes of Health, was in town, and we also invited our closest friends—a graduate student in my husband's lab and her physicist husband, Keith. We had a gala evening, and Marian looked especially glamorous. When she got ready to leave, she put on her glasses. The physicist said, "Marian, you look so much better without your glasses." And, without a moment's hesitation, Marian said, "Keith, you look a lot better without my glasses, too."

Marian invited us to parties at her home. These included festive holiday gatherings for our small lab group and Dan's large gang and also intimate dinner parties, where my husband and I had an opportunity to meet some of Berkeley's luminaries. It was at one of these parties, sitting next to Bruce Ames, that I observed that black cloth napkins were orders of magnitude classier than white ones. Marian was a stylish and gracious hostess and also a gourmet cook.

She invited us out to her home to pick apples from her orchard and to swim in her pool. And when I graduated, she had a brunch in my honor and told me to invite any friends I wanted to invite. That was a wonderful party, elegant and comfortable at the same time.

I never met any of Marian's children. Four of them had gone off to college or beyond by the time I entered her lab, and the ones who went to Haverford were there before I became affiliated with the college. I had no idea what sort of mother she was. Her oldest daughter, Ellen, who lives in Australia, recently wrote me, "My mother had a great influence on my life by her indomitable moral force. It was this force more than her capacity to juggle home and work that stays with me to this day. She conveyed that there were things to be tackled to improve the world and no time to waste in getting on with it. She demanded high quality but she was at heart a true egalitarian, believing everyone deserved a fair go and was capable of real achievement."

Interviews with several of her children are included in Berkeley's retrospective volume, and it is clear that they all admired and appreciated their mother.[1] Gail described Marian as being "definitely Protestant ethic, New England, brought up with the idea that you work hard, and that's part of the purpose of life." Gail's twin brother, James, said that his mother was "very interested in development and always said that her kids were a lifetime experiment. . . I think she clearly had a lot of interest in parenting. . . My parents were scientists. We always say science was their religion. . . They had a very scientific approach to everything, and really believed in that. . . I think what they really wanted to impose on us was a rigor and an intellectual approach in the scientific way. . . You couldn't be superficial about issues. You really had to think it through. . . My mother was a trailblazer who didn't care about recognition. She just wanted excellence in everything."

Douglas, the youngest of the Koshland children, was asked whether his mother had influenced his decision to become a scientist: "My oldest sister is a writer, my next sister is a sculptor, my brother is a lawyer, and my next sister started out in physical therapy. When I showed an interest in science, my mom joked that . . . by the time she got to me she no longer had the energy to direct me elsewhere." He also said, "People were attracted to her because of her tremendous sense of fairness. . . She had extremely high standards . . . in all aspects—her social behavior, science, and everything. If you were going to do something, you were going to do it right. . . She demanded [the same] of herself, so she wasn't being hypocritical. . . I think my mom was born with determination. . . she was a woman with tremendous energy, and a leader and a go-getter. Let's go. Let's do it."

The volume includes an interview with James's wife, Catherine, who graduated from Haverford and, like her mother-in-law, is a professor at the University of California and a member of the Board of Trustees of Haverford College. Catherine told the interviewer: "One of the things that Bunny had encouraged me to think about was not being afraid to do something somewhat unorthodox. . . She probably was the most important mentor in my life in terms of how to do this. Number one was not being afraid to take risks and go on a somewhat unorthodox path. . . There's an interesting combination there of risk taking and judicious selection of 'back water' problems. . . When Bunny went full time on the faculty at age fifty, she had enormous energy and enormous interest. She wasn't burned out. She was not ready to retire. She was ready to take the world by storm."

She also commented on Marian's understanding and appreciation of art and beauty. "[Bunny] could as easily have been in sculpture or landscape architecture as she could have been a scientist. . . She could have pursued some of those things with equal success. She cared a lot about her physical surroundings. She enjoyed having beautiful things. She didn't need a lot. She was the opposite of a pack rat. She never accumulated that much stuff. But what she did acquire or did choose to have around was beautiful. . . I think probably the most dramatic and extravagant expression in some way was her garden, which was really spectacular and to which she devoted a lot of time and energy and which was just an absolute pleasure to look at and a work of art. . . She loved arranging flowers. Like Bunny, her arrangements were highly controlled and very formal."

Marian was elected to the National Academy of Sciences in 1981. The citation described her as "an imaginative and original investigator who was among the first to employ biochemical methods to examine the immune response." She served on the Committee on Science, Engineering, and Public Policy; the Commission on Life Sciences; the Council; and the Committee on Election Procedures. She was the president of the American Association of Immunologists (1982-1983) and served on the association's Council and various other committees for many years. She was on the Executive Council of the American Academy of Arts and Sciences, the Fellowship Screening Committee of the American Cancer Society in California, and the Postdoctorate Fellowships Screening Committee for the Jane Coffin Childs Memorial Fund for Medical Research. She was on the National Science Board of the National Science Foundation, the National Council of the National Institute of Allergy and Infectious Diseases, the Interdisciplinary Cluster on Immunology and Microbiology of the President's Biomedical Research Panel, the Director's Advisory Committee of the National Institutes of Health, and the Allergy and Immunology Study Section of the National Institutes of Health. She was on the editorial boards of the Annual Review of Cell Biology and the Journal of Immunology, and she was an associate editor of Biochemistry and a regional editor of Immunochemistry. She published some 200 articles. She was the recipient of many honors and awards.

Marian died of lung cancer on October 28, 1997. The headline of the Daily Cal, the campus newspaper, was "Staff Recalls Biology Prof: Colleagues Knew Marian Koshland as 'Superwoman.'"

In 2004 the National Academy of Sciences opened its new Marian E. Koshland Science Museum around the corner from the Keck Center in Washington, D.C. The Koshland family endowed the museum in acknowledgment of Marian's interest in and fire-in-the-belly commitment to public education about science. The exhibits are connected with reports that are produced by the National Academy of Sciences.

I visited the museum not long ago. It is a lovely, small museum, currently featuring exhibits on global warming and DNA technology. The installations are engaging, visually striking, intelligent, and elegant, much like Marian Elliott Koshland herself. It is a perfect tribute to her.

NOTES

1. J. P. Allison, A. H. Good, C. P. Koshland, D. E. Koshland Jr., D. E. Koshland, J. M. Koshland, H. O. McDevitt, G. Koshland Wachtel. Marian E. Koshland (1921-1997): Retrospectives on a Life in Academic Science, Family, and Community Activities. Regents of the University of California, 2003.

2. E. Wasserman. The Door in the Dream: Conversations with Eminent Women in Science. Washington, D.C.: Joseph Henry Press, 2000.

3. J. Allison. In memoriam: Marian Koshland, 1921-1997. J. Immunol. 161(2)(1998):545-546.

Many of the quotations in this biographical memoir are, as the text indicates, from conversations that I had with James Allison, Marcy Blackman, Elaine Hansen, Daniel E. Koshland Jr., Douglas Koshland, Ellen Koshland, Henry Metzger, Judy Owen, and Charles E. Wilde III.

SELECTED BIBLIOGRAPHY

1946

With W. Burrows, A. N. Mather, and S. M. Wagner. Studies on immunity to Asiatic cholera. I. Introduction. *J. Infect. Dis.* 79:159-167.

1947

With W. Burrows, A. N. Mather, and I. Havens. Studies on immunity to Asiatic cholera. IV. The excretion of coproantibody in experimental enteric cholera in the guinea pig. *J. Infect. Dis.* 81:261-281.

1950

With W. Burrows. Quantitative studies of the relationship between fecal and serum antibody. *J. Immunol.* 65:93-103.

1953

The origin of fecal antibody and its relationship to immunization with adjuvant. *J. Immunol.* 70:359-365.

1957

Mechanism of antibody formation. I. Fate of I^{131} labeled diphtheria toxoid at the site of antibody formation. *J. Immunol.* 79:162-171.

1963

With F. M. Englberger. Differences in amino acid composition of two purified antibodies from the same rabbit. *Proc. Natl. Acad. Sci. U. S. A.* 50:61-68.

1964

With F. M. Englberger and R. Shapanka. Differences in the amino acid composition of a third rabbit antibody. *Science* 143:1330-1331.

1966

Primary structure of immunoglobulins and its relationship to antibody specificity. *J. Cell Physiol.* 67(suppl. 1):33-50.

1967

Location of specificity and allotypic amino acid residues in antibody Fd fragments. *Cold Spring Harb. Sym.* 32:119-127.

1968

With R. Reisfeld and S. Dray. Differences in amino acid composition related to allotypic and antibody specificity of rabbit heavy chains. *Immunochemistry* 5:471-483.

1969

With J. Davis and N. J. Fujita. Evidence for multiple gene control of a single polypeptide chain: The heavy chain of rabbit immunoglobulin. *Proc. Natl. Acad. Sci. U. S. A.* 63:1274-1281.

1970

With M. Halpern. Novel subunit in secretory IgA. *Nature* 228:1276-1278.

1972

With S. L. Morrison. Characterization of the J chain from polymeric immunoglobulin. *Proc. Natl. Acad. Sci. U. S. A.* 69:124-128.

1973

With C. E. Wilde. Molecular size and shape of the J chain from polymeric immunoglobulins. *Biochemistry* 12:3218-3224.

1975

Structure and function of the J chain. *Adv. Immunol.* 20:41-69.

1976

With R. L. Guyer and P. M. Knopf. Immunoglobulin binding by mouse intestinal epithelial cell receptors. *J. Immunol.* 117:587-593.

1977

With E. L. Mather. The role of J chain in B cell activation. In *Regulation of the Immune System*, eds. C. F. Fox and E. Sercarz, pp. 727-733. New York: Academic Press.

1983

Presidential address: Molecular aspects of B cell differentiation. *J. Immunol.* 131:i-ix.

1985

The coming of age of the immunoglobulin J chain. *Annu. Rev. Immunol.* 3:427-455.

With M. A. Blackman. Differentiation-specific methylation of the immunoglobulin heavy chain locus. In *Biochemistry and Biology of DNA Methylatio*n, eds. G. G. Cantoni and A. Razin, pp. 201-208. New York: Alan R. Liss.

1986

With L. Matsuuchi and G. M. Cann. The immunoglobulin J chain gene from the mouse. *Proc. Natl. Acad. Sci. U. S. A.* 83:456-460.

1989

The immunoglobulin helper: The J chain. In *The Immunoglobulin Gene*, eds. T. Honjo, F. Alt, and T. Rabbits, pp. 345-359. San Diego: Academic Press.

1996

Sheer luck made me an immunologist. *Annu. Rev. Immunol.* 14:ix-xv.

With S. L. Gaffen and S. Wange. Expression of the immunoglobulin J chain in a murine B lymphoma is driven by autocrine production of interleukin 2. *Cytokine* 8:513-524.

With J. L. Rinkenberger, J. J. Wallin, and K. W. Johnson. An interleukin-2 signal relieves BSAP(Pax5)-mediated repression of the immunoglobulin J chain gene. *Immunity* 5:377-386.

HENRY SHERWOOD LAWRENCE

September 22, 1916–April 5, 2004

BY SALAH AL-ASKARI

HENRY SHERWOOD LAWRENCE WAS A distinguished physician, a master teacher, and a pioneer in research on cell-mediated immunity. At a time when scientists focused on the more popular study of humeral immunity and the nature of immunoglobulins in experimental animals, Lawrence emphasized the role of cellular immunity in human responses to disease and antigenic agents. Utilizing man as his study model he discovered that lymphocytes from sensitive individuals produce an active product, "transfer factor," that played a major role in cellular immunity. He was a highly regarded clinician with a special expertise in infectious diseases, and a dedicated teacher and role model for students, residents, fellows, and young physicians.

Lawrence, known to his friends and colleagues as either Sherwood or Jerry, was born on September 22, 1916, in Astoria, New York. His father, Victor John Lawrence, was a Pennsylvania Railroad man and his mother, Agnes Whalen, was a homemaker.

Lawrence attended Public School 6 in Astoria and Townsend Harris High School and then transferred to the prestigious Stuyvesant High School in New York City, where he became interested in biology. On the advice of his biology teacher he enrolled in New York University at "the Heights."

Upon the death of his father in 1937, Lawrence, while he was in his third year of college, became the sole supporter of his mother and of several cousins whose fathers were out of work during the Depression. He had to transfer to the New York University campus at Washington Square in order to complete his studies at the night school. Always personally fastidious and a sharp dresser, he had no time to change after work and attended classes in his work clothes. His day job was with the Pennsylvania Railroad at their Sunnyside rail yard in New York as a straw boss, an assistant foreman of a work gang. He rejected a position as clerk typist, as it paid $5 less per week. His resolve to find a way out of Sunnyside never faltered.

In 1938 Lawrence was accepted into the School of Medicine at New York University and was offered a scholarship of $200 per annum. He informed the school representative that he needed a full scholarship or he would not be able to attend. He left the meeting feeling his life's dream fading. Fortunately, his mother came to the rescue; she sold her life insurance policies and covered the difference. This gift was the beginning of a lifelong devotion to his students, residents, young physicians, and patients at "his hospital," Bellevue.

At medical school Lawrence was one of the poor boys who brought their lunch from home to eat under the pipes in the basement of the old Bellevue Hospital. During medical school he became a very good friend of Winthrop ("Win") Sands, who had sold his seat on the New York Stock Exchange and had begun a career in medicine. This was a friendship that changed Lawrence's life.

On December 8, 1941, Lawrence enlisted in the navy, but he had to sign a waiver stating that because he was 27 pounds underweight, the navy would not be responsible for any illness he incurred while in the service.

At the end of the academic year 1941 Sands casually asked Lawrence about his summer plans. He was stunned to learn that Lawrence would be spending the summer unloading freight cars at Sunnyside rail yard. Sands, as Jerry said in later years, became ”the founder of this feast” by secretly arranging a job for Lawrence through Sands's banker and guaranteeing him a salary of $40 a week. Lawrence did not find out about this until years later when his wife, Dorothea, told him that she had discovered the correspondence between Sands and his banker while clearing out the desk of her boss, who was the manager of the United China Relief Campaign.

Lawrence met his wife, then Dorothea Wetherbee, while she was working in the accounting department of the United China Relief Campaign. One morning her boss said, “Miss Wetherbee, this is Mr. Lawrence. Please show him how to use the adding machine.” Soon they were dating and eventually they married at the chapel of Bellevue Hospital on November 13, 1943. It was the beginning of a long and happy life. Dorothea summed up their mutual feeling by saying, “He was the nicest man I ever met.”

After graduating from medical school in 1943 Lawrence served as an intern, 3rd division Medicine at Bellevue Hospital with an annual salary of $216. Win Sands advised him to buy his navy uniform from Brooks Brothers, a high-class store, saying ever cheerfully that if Lawrence had to die at least he would die dressed like a gentleman.

After completing his internship Lawrence was on active duty from 1944 until 1946 in the U.S. Navy and attained the rank of lieutenant, senior grade, Medical Corps. He was in the Amphibious Service on landing ship tanks (LST), and participated in the invasions of Normandy, Southern France, Okinawa, and the Philippines. After unloading, the LST would be transformed into a hospital ship and part of the tank deck would become the operating room where Lawrence treated the wounded. He was awarded the Bronze Star and other citations for his services. In later years he would suddenly become sad at the thought of "all the boys I could not save, the boys I had to leave at the beaches." The abrupt end of the war spared him a fifth invasion—Japan.

With the war won Lawrence was assigned to Pier 96 Naval Station on the Hudson River in New York. Following his discharge from the Navy in 1946 he began his residency training as an assistant resident in internal medicine at Bellevue—without compensation.

In 1946 his first child, Dorothea Wetherbee Lawrence, was born, followed soon after by Victor John and Geoffrey Douglas.

After completing his residency training Lawrence was appointed as the John Wyckoff Fellow in Medicine (1948-1949). During his early years he became very good friends with his mentor, Alvin M. Pappenheimer, whose help and encouragement were unfailing.

The discovery of cellular transfer of delayed-type hypersensitivity (DTH) in guinea pigs by K. Landsteiner and M. W. Chase had a special affect on Lawrence's interest in immunity. His early studies in human subjects involved the transfer of DTH to tuberculin with viable blood leukocytes (WBC) from sensitive subjects. This was a specific and durable transfer of DTH to nonimmune recipients. It is of interest that during his early work with William S. Tillett, chair of the Department of Medicine while Lawrence was the John Wycoff Fellow, he had only a bench space in the hospital ward's routine laboratory. His only equipment—tuberculin syringes, needles, vials of PPD, a glass jar with cotton pledgets in alcohol, and a brass syringe container that could be sterilized in the ward sterilizer—was kept in the bottom drawer (the one with the lock) of Clair Gautier's desk. Ms. Gautier was the clerk who typed the discharge summaries for the house staff. The meager pay from the fellowship and a small income from working as the director of the Student Health Office were all that sustained Lawrence and his family for several years.

Lawrence's early studies showed that (4.2×10^6) viable WBC from donors with marked cutaneous sensitivity to tuberculin failed to transfer the sensitivity to tuberculin-negative recipients. However, (85×10^6) WBC from such donors consistently transferred the sensitivity. Controls showed that (170×10^6) WBC from tuberculin negative donors did not confer tuberculin sensitivity to tuberculin-negative recipients. This was also observed in the transfer of DTH to streptococcal M-substance. Only WBC from donors with DTH to streptococcal M-substance was capable of transferring the reactivity to negative recipients.

Lawrence's studies on DTH confirmed the observations of Landsteiner and Chase. However, in their studies Landsteiner and Chase utilized pooled cells from 5-7 sensitive guinea pigs to sensitize one animal, and the transferred sensitivity lasted only for a short period, 5-7 days.

After his success in transferring DTH with live WBC, Lawrence proceeded to use disrupted WBC to transfer DTH to tuberculin and streptococcal M-substance. The cells were disrupted either by lysis, following incubation in distilled water at 37°C for 4-6 hours, or by 7-10 cycles of freezing (with dry ice alcohol mixture) then thawing at 37°C. He found that WBC extracts from sensitive donors successfully transferred DTH to tuberculin, streptococcal M-substance, and diphtheria toxoid to the antigen-negative recipients. For convenience he labeled this moiety "transfer factor" (TF).

Together with Felix Rapaport, Lawrence used the transfer of DTH to coccidioidin to determine whether the passive transfer of DTH conferred de novo reactivity to the recipient or whether it was merely boosting subliminal response already present. They found that DNAse-treated WBC extracts from individuals in California who were coccidioidin sensitive successfully transferred the reactivity to East Coast recipients who were never exposed to that antigen (as the fungus does not exist in the East), thus proving that the DTH was passively transferred to the antigen-negative recipients.

To eliminate the role of DNA and RNA in the transfer of DTH, leukocyte extracts from sensitive donors were treated with RNAse, DNAse, or trypsin. The results showed that such treatment did not affect the capacity of the cell extract to transfer DTH to the antigen-negative recipient.

In other studies Lawrence and Pappenheimer used WBC extracts from SK-SD sensitive donors to transfer DTH to diphtheria toxoid. They found that such extracts transferred DTH to the toxoid, but unlike the case in naturally sensitive individuals there was no transfer of either primary or secondary antibody response to the toxoid. Lawrence also showed that DNAse treated WBC from coccidioidin-sensitive donors transferred only DTH to coccidioidin but not the complement-fixing antibody response to coccidioidin. The failure of TF to transfer antibody forming capacity was also confirmed by other investigators. These observations ruled out the possibility that TF acts as a superantigen.

In two elegant experiments Lawrence attempted to determine whether the recipient of TF replicates the active moiety or whether it is merely a signal to activate a change in the recipient's system. He used disrupted WBC from donors with DTH to tuberculin and streptococcal M-substance to transfer DTH to a negative recipient A, and then used disrupted WBC from Recipient A to transfer the acquired DTH to recipient B. The results showed that recipients A and B exhibited strong DTH to tuberculin and streptococcal M-substance, which argued against the notion that TF is a super antigen and lent credence to the concept that a new population of cells arise in recipients of TF and that such cells can transfer the acquired DTH to other negative recipients.

Having established the transfer of DTH to tuberculin, fungus, and bacterial toxin, Lawrence turned his attention to the study of allograft immunity in man. Working with Felix Rapaport and J. M. Converse, the head of plastic and reconstructive surgery at New York University, Lawrence demonstrated that WBC extracts, TF, from human subjects hypersensitized by multiple skin allografts were capable of transferring the specific immunity to HL-A antigens to unrelated individuals. They observed that the recipients of WBC extracts rejected skin allografts from the immunizing donors in an accelerated fashion. These studies established the existence of TF in transplantation immunity and attracted the attention of the leading scientists in the field, like Jean Dausset and Sir Peter Medawar, who later won a Nobel Prize.

Lawrence had a special relationship with Sir Peter Medawar and wrote the following in one of his speeches:

> "I first met Peter at a New York Academy of Science symposium in the early 50's. We had each presented data to support the notion that homograft rejection occurred via a cellular mechanism without, as Peter suggested, the intercession of antibody. In polite immunological circles of the time, an immunological transaction in the absence of antibody was an aberrant idea that would soon succumb to the weight of data. We were each delighted to meet a kindred spirit and became fast friends from then onwards."

That incident led to the appointment of Lawrence in 1959 as a Commonwealth Foundation Fellow to work with Sir Peter Medawar in England and was followed by many visits by Lawrence or Medawar to the other's laboratories. The following excerpt from a letter that Medawar sent to Lawrence demonstrates his appreciation of Lawrence's work:

> I admire your work enormously and the way you have always gone about it. Not many people would have persevered as you have done with such an enormously difficult task to work out the mode of action with transfer factor using clinical materials only.

With the existence of TF documented by many investigators, Lawrence proceeded to purify and characterize it. He and his colleagues showed that TF can pass through Visking cellulose dialysis membrane and that the dialysate could be concentrated by lyophilization. The lyophilized powder was active for five years at 4°C. In these studies 4 ml of DNAse-treated, frozen and thawed WBC extracts were dialyzed against an equal volume of distilled water for 18 hours in a cold room and then lyophilized. The lyophilized powder was reconstituted to its original volume with distilled water and passed through a Swinney or Millipore filter prior to it's injection into the negative recipient. Such reconstituted lyophilized dialysates conferred tuberculin and coccidioidin sensitivity to the negative recipients. The integrity of the dialysis sacs were tested by adding Benes-Jones protein or papain-digested Y-globulin fragments to the sac's contents, and then testing for them in the dialysates. Neither the Y-globulin fragments nor any protein were detected in the dialysates. The potency of the dialysate preparations was increased by increasing the ratio of the dialysant to the dialysate to 1:50. This was shown by the increased intensity of DTH to coccidioidin in the negative recipients. Further purification of transfer factor was achieved by passing the reconstituted lyophilized dialysate through Sephadex G-25 columns. Fractions collected under peak II (molecular weight <10,000) transferred DTH to coccidioidin in negative individuals, indicating that the active moiety in transfer factor is likely to be a small poly-peptide-polynucleotide. This observation was confirmed by other investigators.

The lack of an animal model for the in vivo characterization of TF made Lawrence turn his attention to the in vitro study of cellular immunity. In their studies of DTH in guinea pigs, M. George and J. H. Vaughan used the migration of peritoneal macrophages from capillary tubes in small culture chambers. They observed inhibition of migration of macrophages from tuberculin-sensitive guinea pigs when PPD was added to the culture chambers. However, they had difficulty with their technique, because their macrophage preparations were contaminated with red blood cells. Lewis Thomas, who had visited their laboratory, was impressed by their approach. He suggested to Lawrence that John David and I (fellows in the lab) try it. I reproduced the culture chambers in my basement workshop and I was able to obtain blood-free, mineral-oil-induced peritoneal macrophages by not feeding the test animals for 24 hours and exanguinating them prior to harvesting the peritoneal macrophages. After David and I confirmed the observations of George and Vaughan, Thomas informed them of our technical improvement and insisted that they publish their paper first.

David subsequently discovered that the inhibition of macrophage migration was due to the production of a heat-stable, nondialysable protein by lymphocytes from guinea pigs with DTH when they are exposed to the specific antigen. He labeled this moiety “migration inhibitory factor” (MIF). In subsequent studies we also observed inhibition of migration of peritoneal macrophages from guinea pigs sensitized with multiple skin allografts when mixed with sessile lymphoid cells from the sensitizing donors. Lymphocytes from other animals did not affect the migration of the sensitive cells.

Similar results were obtained in inbred strains of mice. These observations indicated that the inhibition of macrophage migration from animals exhibiting allograft sensitivity by the specific histocompatibility antigens is an in vitro correlate for transplantation immunity.

In collaboration with D. C. Dumonde, a newly arrived fellow from the Mill Hill Institute in London, we found that spleen microsomes function as histocompatibility antigens when administered by the intradermal or the intraperitoneal routes. We also found that supernatants from cultures of sensitive lymphocytes plus donor antigens, lymphocytes, or spleen microsomes inhibited the migration of peritoneal macrophages from normal animals, indicating the production of MIF by sensitive lymphocytes upon exposure to the specific sensitizing histocompatibility antigens. It also demonstrated the nonspecific action of MIF in transplantation immunity. These findings were similar to the in vitro observation on classic DTH models, suggesting a common mechanism.

W. Borkowsky and other investigators found that TF (or dialyzable leukocyte extract, DLE) contained a small (<10K but >3.5 K) peptide with a blocked N-terminus that originated from CD4 T-helper cells and could bind to CD8 T-cells and to antibodies directed at MHC class II antigens. It could also bind to the antigen for which it demonstrated specificity and to antibodies directed at anti-VH regions. In the course of this work it was shown that in addition to a TF which would deliver instant CMI to a specific antigen, there was also a TF (or DLE) which could abrogate antigen-specific CMI. This "suppressor TF" originated from immune CD8 T-cells and could bind to CD4 T-cells, as well as anti-MHC II and anti-VL antibodies. It could also bind to antibodies directed at the antigen that was the target of the cellular immune response. This sort of information suggested that TF acted in a sort of idiotype/anti-idiotype network of immunity.

Lawrence and F. T. Valentine used thymidine incorporation to further characterize the in vitro action of TF. They found significant increase in thymidine incorporation when tuberculin-sensitive lymphocytes were cultured in the presence of PPD. In addition, when supernatants from such cultures were added to cultures of tuberculin-negative lymphocytes, there was also a significant increase in thymidine incorporation. The active moiety in the supernatant was nondialyzable, antigen-dose dependant and could not be sedimented at 100,000 g. They named it lymphocyte transforming factor (LTF).

The ability of TF to confer instant CMI provoked many immunologists to use TF to treat diseases ranging from chronic infectious diseases to congenital immunodeficiency and even malignancies. Reports of cure of chronic mucocutaneous candidiasis, Wiscott-Aldrich syndrome, generalized vaccinia, disseminated fungal infections, and leprosy appeared in many journals and generated great interest in TF. Nevertheless, it must be admitted that TF does not seem to relate to any of the known soluble immune mediators, and its nature remains a fascinating but obscure puzzle.

To explain the role of TF in immunological homeostasis Lawrence proposed, in 1959, the self+x hypothesis as the modus operandi in CMI. He postulated that foreign antigens (bacterial, viral, fungal, and other nonself components) ingested by macrophages caused alterations in the self antigens on host cell surfaces self+x. Ingestion of such cells by other reticuloendothelial cells stimulated the production of TF against the self+x complex. It seems that Lawrence had anticipated the findings of Doherty and Zinkernagle (i.e., antigenic recognition in the context of self major histocompatibility antigens).

Lawrence's contributions to the understanding of DTH were documented in his publication of over 180 articles, books, and chapters in books. The following is a poem by Lewis Thomas written in honor of Jerry's birthday in 1979. It reflects his appreciation of Jerry's professional achievements.

In glass and in life
Investigations are rife.

This year has been great
For TF we can state,
Has all critics tamed,
At least as of late.

Lawrence has shown
It was specific in man.

Demonstrate that they can
Cause specific inhibition
Of cells wandering condition.

To this one might say
"Why isn't that nice"
But for immunological skeptics
It works even in mice.

A good year for TF,
And for you too, Jerry
We hope that your birthday
Proved to be merry.

As a clinician Jerry was highly regarded as an expert in infectious diseases and as a unique role model in bedside manners. He always emphasized a humanitarian, compassionate, and respectful approach in patient management.

During his tenure at New York University Medical Center he served as an attending physician at University Hospital, Bellevue Hospital, and Manhattan Veterans Administration

Hospital. He was also head of the Infectious Diseases and Immunology Division from 1959 to 2000, codirector of medical services from 1964 to 2000, director of the Cancer Center from 1974 to 1979, and director of the AIDS Research Center from 1989 to 1994.

Lawrence's contributions and discoveries earned him honors and recognition at home and abroad. He was elected to numerous distinguished societies as a member, charter member, or honorary member. These included the American Association of Immunologists, Society of Experimental Biology and Medicine, Harvey Society, Infectious Disease Society, Peripatetic Clinical Society, Interurban Clinical Club, American Academy of Allergy, Transplantation Society, American College of Physicians, Royal Society of Medicine (England), Royal College of Physicians and Surgeons (Glasgow), and the Société Française d'Allergie (France). He was an invited lecturer to the Société Française d'Immunologie (Institute Pasteur). He was elected to membership in the National Academy of Sciences in 1972.

He was also appointed consultant to or chairman of many prestigious scientific councils, including the American Thoracic Society; Health Research Council (City of New York); Armed Forces Epidemiological Board, Streptococcal and Staphylococcal Commission; National Institute of Health, Institute of Allergy and Infectious Diseases, Allergy and Immunology Study Section; American Rheumatism Association; and the National Research Council (member, Committee on Cutaneous System and chair of the Committee on Transplantation).

Lawrence served on many editorial boards for scientific journals, including *Transplantation*, *Proceedings of the Society of Experimental Biology and Medicine*, and *Annals of Internal Medicine*. He was the founder and editor in chief of Cellular Immunology.

Lawrence received many awards and prizes for his contributions, including the von Pirquet Gold Medal for Scientific Advancement in Immunology from the Forum on Allergy; the New York Academy of Medicine Medal for Outstanding Contributions to Science; the American College of Physicians Award for Outstanding Contributions to Science; the Lila Gruber Award for Cancer Research from the American Academy of Dermatology; and the Distinguished Teacher's Award from the New York University School of Medicine. In 1979 he was named the Jeffrey Bergstein Professor of Medicine at New York University.

Lawrence was a gracious, humble, and modest man who treated others with respect and dignity regardless of their position in society. He was a man of honor who loved family and friends. Never forgetting his roots, he loved the work of Charles Dickens, because the people he met in the wards of Bellevue Hospital were the kind of people about whom Dickens wrote. He was so taken with his British experience after his visits with Medawar that he adopted some continental habits, such as afternoon breaks. He preferred English tweed, ascots, bow ties, Irish doc caps, shirts with French cuffs, and odd jackets with nipped waists and side vents. His office was decorated with a segment of the Bayeux Tapestry, a gift from his wife, Dorothea, showing the embarkation of William the Conqueror from a point not far from where Jerry's LST had landed on D-Day. When asked about it, Jerry would say that he was "the last of the Plantagenets." His remarkable resemblance to Alec Guinness was so striking that a birthday gift from his laboratory staff was a poster of the Star Wars character Obi-Wan Kenobi. People often joked that they might be twins separated at birth.

I met Jerry when I had just finished my training in urology and was interested in kidney transplantation. It was my privilege to be Jerry's first research fellow after his return from his fellowship in England. I was followed by B. Zweiman, John David, D. C. Dumonde, F. T. Valentine, R. S. Holzman, W. Borkowsky, and many others.

Jerry was especially kind to young researchers and was keenly interested in training, mentoring and advancing the careers of those who worked with him. He took his research fellows to exclusive meetings and conferences, such as those of the Streptococcal Commission of the Armed Forces Epidemiological Board and the National Academy of Sciences. To advance their careers he would introduce his fellows to leading scientists during the meetings. He was very tolerant and never angry. When it was time to publish the discovery of MIF he insisted that John David should take the credit by publishing it alone, a very noble and generous act. Lawrence's laboratory needed a lot of equipment. John David and I would go with him to the meetings of the Federation of the Society of Experimental Biology and Medicine and shop at the exhibits like children at an FAO Schwartz toy store.

He taught his fellows how to write scientific papers and grant applications and how to utilize the subtleties of the English language. He stressed the use of the passive voice when deemphasizing a controversial point.

Jerry was very tolerant of the families and children of his fellows. Robert Holzman recalls that he used to bring his son, Dan, to the laboratory on Saturdays. Dan always remembered Dr. Lawrence as "the man who was in the laboratory on Saturdays," and on hearing of his passing, wrote a full page memoir in his blog.

Jerry was a loving and devoted father. His daughter, Dorothea, remembers that he allowed his children to have their own voice—he was never opinionated or authoritarian—and he seemed genuinely eager to listen to them, even when they were quite young. He was a devoted husband and hated to be away from his beloved wife, Dorothea ("Dot").

On one occasion he was going to stay in Paris for one week after his talk at the Pasteur Institute. However, he surprised his family when he returned the next day after giving his talk. He simply missed them too much to stay away. He brought Dot flowers several times a week and regularly left her love notes all around the house. His down-to-earth attitude was exemplified in the statement of an African American elevator operator at Bellevue to Jerry's son Geoffrey: "You know, son, your father is a great man. I've been told that he is a good physician, and he's starting to be recognized for his research. Your father never failed to speak to me—many of these doctors I knew when they were students and most do not even look at me—cause they are too important now."

Unfortunately, Jerry suffered a serious accident, which forced his retirement in 2000 and lead to his death in 2004. He will always be remembered as a member of what Tom Brokaw called "the greatest generation."

I AM INDEBTED TO THE Lawrence family in compiling the history for Jerry's story. The contributions of Drs. John David, William Borkowsky, and Robert Holzman were invaluable in the preparation of this memoir.

SELECTED BIBLIOGRAPHY

1949

The cellular transfer of cutaneous hypersensitivity to tuberculin in man. *Proc. Soc. Exp. Biol. Med.* 71:516-522.

1954

The transfer of generalized cutaneous hypersensitivity of the delayed tuberculin type in man by means of constituents of disrupted leukocytes. *Abstract. J. C1in. Invest.* 33:951.

1956

With A. M. Pappenheimer Jr. Transfer of delayed hypersensitivity to diphtheria toxin in man. *J. Exp. Med.* 104:321-336.

1959

With F. T. Rapaport, J. W. Millar, D. Pappagianis, and C. E. Smith. Transfer of delayed coccidioidin hypersensitivity with leukocyte extracts in man. *Fed. Proc.* 18:593.

1960

With F. T. Rapaport, J. M. Converse, and W. S. Tillett. Transfer of delayed hypersensitivity to skin homografts with leukocyte extracts in man. *J. Clin. Invest.* 39:185.

1963

With S. Al-Askari. J. David, E. C. Franklin, and B. Zweiman. Transfer of immunological information with dialysates of leukocyte extracts in humans. *Trans. Assoc. Am. Phys.* 77:84-91.

1964

With S. Al-Askari, D. C. Dumonde, and L. Thomas. Subcellular fractions as homograft antigens. *Ann. N. Y. Acad. Sci.* 120:201-269.

With J. R. David, S. Al-Askari, and L. Thomas. Delayed hypersensitivity in vitro. I. The specificity of inhibition of cell migration by antigens. *J. Immunol.* 93:264-273.

1965

With S. Al-Askari, J. R. David, and L. Thomas. In vitro studies of homograft sensitivity. *Nature* 205: 916-917.

Transfer factor and autoimmune disease. *Ann. N. Y. Acad. Sci.* 124:56-60.

1968

With W. H. Marshall and F. T. Valentine. Antigen stimulated lymphocyte transformation in vitro—evidence for clonal proliferation. *Clin. Res.* 16:322.

Transfer factor and leprosy. Editorial. *N. Eng. J. Med.* 278:333-334.

1970

Transfer factor and cellular immune deficiency disease. *N. Eng. J. Med.* 283:411-419.

1971

With S. Al-Askari. The preparation and purification of transfer factor. In *In Vitro Methods in Cell-Mediated Immunity*, eds. B. R. Bloom and P. R. Glade, pp. 531-546. New York: Academic Press.

1972

Reconstitution of immunodeficiency states. In *Immunologic Intervention*, eds. J. Uhr and M. Landy, pp. 20-27. New York: Academic Press.

Immunotherapy with transfer factor. Editorial. *New Eng. J. Med.* 287:1092-1094.

With S. Al-Askari. In vitro studies on transplantation immunity. I. M.I.F. production by sensitive lymphocytes in mice. *Cell. Immunol.* 5:402-409.

1973

With S. Al-Askari. In vitro studies on transplantation immunity. II. The migration inhibition in homograft reactions in guinea pigs. *Cell. Immunol.* 6:292-299.

1979

With W. Borkowsky. Effects of human leukocyte dialysates containing transfer factor in the direct leukocyte migration inhibition (LMI) assay. *J. Immunol.* 123:1741-1749.

1983

With W. Borkowsky, J. Berger, and R. Pilson, Antigen-specific suppressor factor in human leukocyte dialysates: A product of Ts cells which binds to anti-V region and anti-Ia region antibodies. In *Immunobiology of Transfer Factor*, eds. C. H. Kirkpatrick, D. R. Burger, and H. S. Lawrence, pp. 91-114. New York: Academic Press.

With R. S. Holzman, W. Borkowsky, and R. Pilson. Isolation and purification of antigen-specific inducer and suppressor factors from pooled leucocyte dialysates of unrelated donors by affinity adsorption. In *Immunobiology of Transfer Factor*, eds. C. H. Kirkpatrick, D. R. Burger, and H. S. Lawrence, pp. 117-125. New York: Academic Press.

1989

With W. Borkowsky. The nature and functions of inducer factor and suppressor factor in T cell dialysates. *Immunol. Lett.* 21:75-80.

1996

With W. Borkowsky. Transfer factor—current status and future prospects. *Biotherapy* 9:1-5.

EDWARD CRAIG MORRIS

October 7, 1939–June 14, 2006

BY JOYCE MARCUS

EDWARD CRAIG MORRIS, KNOWN AS "Craig" to his many friends, was the leading Inka archaeologist of his time. His studies of governmental storage and the expansionist strategies employed by the Inka are considered classics in the field. Morris was best known for his excavations in Peru, at the archaeological sites of Huánuco Pampa (the most completely preserved highland Inka site) and La Centinela (a major coastal ruin in the Chincha Valley). There he documented the operation of the economic, social, political, and religious institutions of the Inka. Although many of his predecessors had relied exclusively on written texts (e.g., 16th-century Spanish documents), Morris sought to evaluate those documents with excavation and settlement pattern data, thereby obtaining a richer and more accurate view of the Inka empire. Morris's multiyear excavation projects supplied abundant empirical evidence to generate new models of how the Inka empire succeeded in integrating diverse ethnic groups occupying altitudinal zones from sea level to 4000 meters.

Morris was a modest and unassuming scholar who nevertheless managed to transform Andean archaeology, altering our views of Inka institutions and mode of governance. Unfailingly gracious and generous, he was the kind of person usually referred to as a gentleman scholar. As Ellen

V. Futter, president of the American Museum of Natural History, told The New York Times[1], "He was a pillar of our community personally and intellectually." For many, Craig Morris was a Rock of Gibraltar, always providing encouragement, insights, and reliable solutions. In 2004, he stepped down from a decade of service as the dean of science at the American Museum, hoping, as he said, "to devote myself during the next several years to research, writing, publishing, and fieldwork; finally, I will be getting back to all the things I love."[2] Unfortunately, he only had a couple of years to do the things he loved.

Edward Craig Morris was born October 7, 1939, at Murray-Calloway County Hospital in Murray, Kentucky. His parents were Alwin Wybert Morris and Rubye Craig Morris. Their first child was a daughter, Emily Dale Morris (Luther), already 13 years old when her brother Craig was born. At birth Craig was found to have a serious heart condition; his doctors thought that he might not survive past childhood. They advised the family to keep him away from sports and strenuous activity, encouraging him to focus instead on reading and educational interests. His family followed this advice and Craig became an avid reader, excellent student, and ultimately a brilliant social scientist who in 1998 was elected to both the American Academy of Arts and Sciences and the National Academy of Sciences.

Morris grew up on a 170-acre farm in east Calloway County. His father grew most of the family's food, raising cattle and hogs to supply his family with enough beef and pork each year. The family garden provided the vegetables and fruits, especially strawberries and blackberries. The family also harvested apples, peaches, and pears, as well as grapes from the vines that grew along the garden fence. Craig often mentioned how much he had liked his life on the farm, especially feeding the animals.

Morris's elementary school years were spent at the Murray Training School. During these years he was very active in the 4-H Club, and one of his projects was raising Black Angus steers in different weight classes. Morris won several blue ribbons during 4-H competition, and at age 14, he raised two Black Angus steers that went on to become state champions. He used to say that developing these champion steers gave him a real sense of accomplishment.

In June 1958 Morris graduated from high school, where he was both valedictorian and editor of the yearbook. In the fall of that year he entered Murray State University, which was close to home. Later he was encouraged to take entrance exams to see if he could transfer to Vanderbilt University, where he was accepted. To finance Craig's tuition at Vanderbilt, an expensive private university, Craig's parents had to sell a portion of the family farm. Morris rewarded his parents' sacrifice by graduating magna cum laude from Vanderbilt in 1961 with a major in psychology and philosophy.

Craig's next stop was the graduate anthropology program at the University of Chicago, where his career path was determined and his intellectual goals were shaped. The next three summers were exciting. In 1962 he undertook his first archaeological fieldwork in Utah; in 1963 he went with F. Clark Howell to excavate the Paleolithic sites of Torralba and Ambrona in Spain; and in 1964 he went to the highlands of Peru to work on the Inka with John V. Murra. Peru was where his research took him from then on.

Morris began an investigation of the Inka storage system as part of Murra's project, "A Study of Inka Provincial Life." This project was designed to examine Inka storage at three levels: the local community, the provincial network, and the imperial level directed from the Inka capital of Cuzco. Craig was interested in identifying both the kinds and quantities of goods stored in these different contexts, arguing that the

Inka empire was made possible by an extremely successful storage technology that integrated different levels of the political hierarchy. He began in the central highlands of Peru, where he documented more than 2000 storerooms and excavated 112 of them.

In addition to Inka storage systems, Morris had become fascinated by the nature of Andean cities. He wrote, "My interest in early cities dates from graduate school courses with Robert McC. Adams, and my research on Inka cities began with my doctoral dissertation on Inka warehousing in 1967."[3] After completing his doctoral dissertation in 1967, Morris was hired by Northern Illinois University, where he stayed for one year before moving on to Brandeis University (1968-1975). In 1975 he was hired as an assistant curator of anthropology at the American Museum of Natural History, where he remained for the rest of his career.

Morris's doctoral dissertation, "Storage in Tawantinsuyu," occupies a pivotal place in the study of the Inka empire, primarily because it steered the field in a new direction. Morris showed that imperial warehousing was one of the ways he could discover the Inka empire's infrastructure and logistics. He noted that storage systems had attracted the theoretical interest of substantive economist Karl Polanyi but had yet to become a major focus for Andean archaeologists.

Morris's best-known data on storage came from Huánuco Pampa, an Inka administrative center established at a previously unoccupied locale in north-central Peru, some 3800 meters above sea level. He worked at Huánuco Pampa from 1971 to 1981, spending a total of 36 months at the site. That extensive fieldwork led to the publication of his 1985 book, Huánuco Pampa: An Inka City and Its Hinterland (coauthored with Donald E. Thompson).

Much of Huánuco Pampa's food and other products had to be grown at lower elevations and transported to the site.

During his research, Morris found both rectangular and circular storehouses, and he wondered whether their contrasting shapes and different hillside locations could be related to different functions. After several seasons of excavation, he was able to show that tubers (such as potatoes) were stored in rows of rectangular storehouses constructed on the lower part of the hill, while grains (such as maize) were generally found in circular structures higher up. Thus the groupings and arrangements of storehouses did indeed relate to function. Craig's work also showed that perhaps 50 percent of the storage space was devoted to highland tubers (potatoes and other root crops); roughly 5 percent to 7 percent was devoted to maize; and that at least 28 percent was available to store cloth, military equipment, and other nonsubsistence goods. He went on to show that more than 12 percent of the 4000 structures at Huánuco Pampa had originally been devoted to storage.

Looking back at Morris's Ph.D. dissertation work, we can see how key it was to our understanding of imperialism. Craig not only investigated the environmental conditions and technology that had facilitated the preservation of perishable items but he also assessed the role storage had played in the political and economic operation of an empire. Along the way he discovered the building blocks of the largest native empire of the New World and the way the Inka coordinated the multiple subject ethnic groups who produced goods for them at different altitudinal zones. This ability to see the big picture and the larger processes at work was typical of Morris's research. As further evidence we can cite a 1992 book chapter titled "Huánuco Pampa and Tunsukancha: Major and Minor Nodes in the Inka Storage Network," where he concluded that "a warehousing system such as that documented in this chapter does not emerge full-blown as the result of the command of some powerful and brilliant ruler. The sophistication of its environmental understandings are too great and its organizational scope too vast. It can only be the product of long development processes and can only be understood in terms of the larger patterns of technology and organization from which various aspects of the storage system were drawn." This desire to document the long development processes became a blueprint for Morris's future fieldwork.

While maintaining his interest in the storage and distribution of goods, Morris began to focus on the wider range of integrative mechanisms employed by the Inka empire. After excavating a sector of Huánuco Pampa that produced thousands and thousands of beer-brewing and beer-drinking vessels, Morris turned to a series of 16th-century Spanish documents providing eyewitness accounts of the way the Inka used public hospitality (especially the serving of maize beer) to attract laborers for state-sponsored projects, such as the terracing of hillsides, the digging of irrigation canals, and the planting and harvesting of crops. He noted that in Quechua, the language of the Inka, these beer feasts were regarded as generous acts by the local ruler, even though the actual relationship of rulers to workers was exploitive and asymmetrical.

Craig explored this theme in subsequent publications, such as a 1979 book chapter titled "Maize Beer in the Economics, Politics, and Religion of the Inka Empire." He was ultimately able to show that the Inka state's involvement in the redistribution of food was limited mostly to specific feasting occasions, designed to ensure labor service on a massive scale. Huánuco Pampa, which brewed maize beer just for such purposes, was only one of the places built by the Inka in order to create a node of social, political, and economic control (a process that Morris called compulsory urbanism). He concluded that Huánuco Pampa was an artificial urban

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center created for political purposes. At the same time such a concept was not original with the Inka, but was an elaboration of old Andean traditions of reciprocity used to secure labor, combined with a new goal, that of controlling a politically fragmented or balkanized region.

A second phase of Morris's career began in 1983, when he started a project on Peru's south coast. Even though he loved excavating archaeological sites in the Inka heartland, that high-altitude work put a strain on his heart and endangered his life. Thus, he turned to the Chincha Valley and the coastal site of La Centinela. Morris was struck by how dramatically La Centinela differed from Huánuco Pampa. La Centinela was an administrative center predating the Inka, rather than an artificial creation of the Inka state. His excavations at La Centinela suggested that after the Inka had conquered the area, they did something unusual for them: They established a complex form of parallel rule in which the local Chincha lord was allowed to continue living in his palace while the Inka governor built his own palace next door. This kind of co-rulership had never been documented for the Inka empire until Morris excavated the dual palaces at La Centinela. The ethnohistoric record might explain this unusual co-rulership: It revealed that the Chincha ruler had maritime trade relations with the Gulf of Guayaquil (Ecuador), importing Spondylus, a shell that was highly valued by the Inka. Morris suggested that the Inka did not want to disrupt the Chincha lord's trade.

Another striking difference between the dual palaces at La Centinela and their equivalent at Huánuco Pampa was a great reduction in public space. Huánuco Pampa had served as a meeting place where many different local leaders and ethnic groups gathered, requiring ample public space. At La Centinela the critical local leadership consisted of the Inka governor and the Chincha ruler, so royal hospitality could be provided in a far smaller space. A notable feature of the La Centinela administrative complex was that the Inka palace and that of the local Chincha lord were both built of Inka-style adobe bricks, suggesting that they both had been built by Inka labor. The adjoining palaces with their paired, interlocking spaces allowed the allied leaders to interact in surroundings appropriate to both the circumstances and their rank. A public plaza was indeed part of the Inka addition to La Centinela, but unlike the huge plaza at Huánuco Pampa it was not the central element of the city plan. Finally, significant stylistic differences in ceramics confirmed the dual character of rulership in the two palaces, with one having more Inka material than the other.

Over a period of four decades, until his lifelong heart problems finally cut short his career, Morris had tackled both a highland Inka settlement at 3800 meters and a sea-level Inka settlement, allowing him to document in rich detail two contrastive datasets that continue to have theoretical implications for Andean prehistory. The two settlements he investigated were worlds apart in terms of environment, architecture, and economics, but he showed that each made sense in terms of Inka imperial strategy. Huánuco Pampa was built to consolidate and reorganize a politically fragmented region, using a newly imposed administrative capital built on neutral ground at a distance from all local population centers. There the ethnically diverse peoples of the region, acting as "guests" working part-time for the Inka state, could participate in the rich ceremonial life of the empire; the hope evidently was that they could be controlled and incorporated into the Inka state. By throwing lavish beer feasts in symbol-laden settings, providing gifts of clothing, and arranging marriages between elite local women and Inka administrators, local groups were given a way to gain prestige and positions in a complex imperial system.

In contrast, La Centinela had already existed for centuries as the capital of a coastal state. Here the Inka had to formulate a different strategy; and out of deference to a local lord with important connections, the Inka worked out a way to co-rule the area, evidently convincing the lord of Chincha that it would be to their mutual benefit (at least in the short run). In two vastly different regions, therefore, the Inka emphasis was on installing administrative and ceremonial facilities designed to redefine political, economic, and religious relationships and to create new patterns of integration. Morris saw, before anyone else, the potential of these two case studies to increase our understanding of ancient empires. Three books that demonstrate his impressive ability to analyze Andean sociopolitical evolution include *Andean Ecology and Civilization* in 1985 (with Shozo Masuda and Izumi Shimada), *The Inka Empire and Its Andean Origins* in 1993 (with Adriana von Hagen), and *The Cities of the Ancient Andes* in 1998 (with Adriana von Hagen).

Less well known but equally impressive was Morris's ability to design museum exhibits. Morris spent the years from 1980 to 1989 designing and installing the Andean archaeology exhibit in the South American Hall at the American Museum of Natural History. Today it is still regarded as the best Andean exhibit in the United States. Although the years he devoted to designing such exhibits kept him from writing as much as he wished, Morris's museum exhibits live on because they continue to disseminate scientific and archaeological information to both lay persons and professionals.

PERSONAL QUALITIES OF THE MAN

To his friends Craig Morris was truly a special person: a loyal and selfless friend, an exceptional fieldworker able to produce results for decades, and a social scientist who knew how to juggle multiple variables en route to elucidating long-term processes. He persevered against heavy odds, overcoming his doctors' predictions that he would not survive childhood, even working for years at 3800 meters above sea level.

It was typical of Morris that he was genuinely surprised when he was elected to the National Academy of Sciences, but once inducted he became an active and enthusiastic member. Morris never missed the annual meeting, which he explained in terms of friendship and collegiality: "Being a member of the National Academy of Sciences is a wonderful honor," he said, "but the unexpected bonus is that I get to see all my best friends every April."[4] At the Academy meetings Morris enjoyed going out to dinner with his colleagues, and on such occasions he frequently drew on his past experience as a cattle raiser to select choice steaks. One of my most amusing memories was watching him instruct a waiter at Shula's Steakhouse about the criteria for a really good piece of beef.

One of Morris's last key contributions came as a participant in the Academy's May 2005 Sackler Symposium on Early Cities: New Perspectives on Pre-Industrial Urbanism. There he delivered a paper contrasting Inka strategies as they played out at the highland center of Huánuco Pampa and at the coastal center of La Centinela.

We will surely miss Morris at future annual meetings and symposia, not only for his wisdom but also for his modesty, warmth, humor, and kindness. His doctors may have worried about his heart, but his colleagues found it extraordinary.

MORRIS'S INTELLECTUAL LEGACY

Craig Morris expanded our knowledge of the Inka empire by focusing on its sociopolitical institutions and economic underpinnings. He found ways to get at its infrastructure, its strategies for territorial expansion, the mechanisms it used for assigning value to goods, and the way it integrated scores of ethnic groups from Ecuador to Chile. He combined archaeological excavations and documentary research to determine how the Inka took traditional Andean concepts of reciprocity and ecological complementarity and manipulated them to achieve tributary labor service to the state, the widespread resettlement of ethnic groups, and the construction of extensive agricultural terraces, roads, bridges, storehouses, and irrigation canals.

The models that Morris created included elements that had been missing from previous overviews, which often saw the Inka empire as the product of religious and military forces. He redirected a whole generation of Andean research, encouraging us to add storage systems, economic strategies, labor service, and public hospitality to extant models. His ultimate legacy was the creation of multivariate models of state bureaucracies that will guide future generations in the Andes and elsewhere.

SOME OF THE INFORMATION in this memoir was drawn from materials provided by Craig Morris's sister, Emily Morris Luther of Murray, Kentucky, and by his colleagues at the American Museum of Natural History: Robert L. Carneiro, Elsa M. Redmond, Charles Spencer, and David Hurst Thomas. I thank them all.

NOTES

1. John Noble Wilford, "Craig Morris, A Towering Figure in Inca Expeditions, Dies at 66," *New York Times,* June 16, 2006, Sec. C, P 11
2. Personal Communication: Craig Morris to Joyce Marcus, December, 2004.
3. Remarks at "Early Cities: New Perspectives on Pre-Industrial Urbanism." Arthur M. Sackler Colloquium, May 18-20, 2005.
4. Personal Communication: Craig Morris to Joyce Marcus, April, 2002.

CHRONOLOGY

1939	Born October 7 in Murray, Kentucky
1958	Graduated from Murray Training School (high school)
1961	B.A. in psychology/philosophy from Vanderbilt University
1964	M.A. in anthropology from the University of Chicago
1967	Ph.D. in anthropology from the University of Chicago
1967-1968	Assistant Professor, Northern Illinois University
1968-1975	Assistant Professor, Brandeis University
1975-1980	Assistant Curator of Anthropology, American Museum of Natural History, New York
1976	Visiting Associate Professor of Anthropology, Cornell University
1977	Visiting Professor of Archaeology, Universidad Nacional Mayor de San Marcos, Lima, Peru
1977-1992	Adjunct Professor, Cornell University
1983-1990	Chair, Department of Anthropology, American Museum of Natural History
1986	Visiting Professor of Anthropology, City University of New York Graduate Center
1989-1991	Guest Curator, "Art in the Age of Exploration (Inka Section)," National Gallery of Art
1990	Co-Director of "The Andean World: A Millennium of Achievement." Summer institute for college teachers; funded by National Endowment for the Humanities. Cornell University
1992-1997	Adjunct Professor of Anthropology, Columbia University
1994-2005	Dean of Science, American Museum of Natural History
1998-2005	Vice-President, American Museum of Natural History
1980-2006	Curator of Anthropology, American Museum of Natural History
2006	Died June 14 in New York City

AWARDS AND HONORS

1961 Phi Beta Kappa, Vanderbilt University
1969 Elected to the Institute of Andean Studies
1976 Elected to the Institute of Andean Research
1977 Fulbright-Hays Lectureship in Peru
1998 Elected to the American Academy of Arts and Sciences
1998 Elected to the National Academy of Sciences

OTHER POSITIONS

1978-1980 Anthropology Screening Committee, International Council for the Exchange of Fulbright Scholars
1980-1981 Anthropology Panel, National Science Foundation
1980-1992 Advisory Council, The Textile Museum
1982-2006 Advisory Committee on Visual Arts, Center for Inter-American Relations, The Americas Society
1984, 1988 Adviser, Inter-American Development Bank, Peruvian Museum Projects
1983-2006 Editorial Board, Armitano Arte, Caracas, Venezuela
1984-2006 Editorial Board, Andean Past
1987-1988 Adviser, Ford Foundation, Lima
1989-1992 Editorial Advisory Board, Science Year, World Book, Inc.
1994 Adviser for Qorikancha Archaeological Park, UNESCO

MEMBERSHIPS

National Academy of Sciences
American Academy of Arts and Sciences
Institute of Andean Research (vice-president, 1995-2006)
American Anthropological Association (fellow)
Society for American Archaeology
Council for Museum Anthropology
American Society for Ethnohistory
Society for American Archaeology

SELECTED BIBLIOGRAPHY

1966

El Tampu Real de Tunsucancha. *Cuadernos de Investigación, Antropología* 1:95-107. Universidad Nacional Hermilio Valdizán, Huánuco, Peru.

1967

Storage in Tawantinsuyu. Ph.D. dissertation, Department of Anthropology, University of Chicago.

1970

With D. E. Thompson. Huánuco Viejo: An Inca administrative center. *Am. Antiquity* 35(3):344-362.

1972

State settlements in Tawantinsuyu: A strategy of compulsory urbanism. In *Contemporary Archaeology: A Guide to Theory and Contributions*, ed. M. P. Leone, pp. 393-401. Carbondale: Southern Illinois University Press.

1974

Reconstructing patterns of non-agricultural production in the Inca economy: Archaeology and documents in institutional analysis. In *Reconstructing Complex Societies: An Archaeological Colloquium*, ed. C. B. Moore, pp. 49-60. Bull. Am. Sch. Oriental Res. 20(suppl.).

The identification of function in Inca architecture and ceramics. *Revista del Museo Nacional* 37:135-144, Lima.

1976

Master design of the Inca. *Nat. Hist.* 85(10):58-67.

1978

The archaeological study of Andean exchange systems. In *Social Archaeology: Beyond Subsistence and Dating*, eds. C. L. Redman, M. J. Berman, E. Curtin, W. Langhorne Jr., N. Versaggi, and J. Wanser, pp. 315-327. New York: Academic Press.

1979

Maize beer in the economics, politics, and religion of the Inka empire. In *Fermented Food Beverages in Nutrition*, eds. C. F. Gastineau, W. J. Darby, and T. B. Turner, pp. 21-34. New York: Academic Press.

1980

Huánuco Pampa: Nuevas evidencias sobre urbanismo Inca. *Revista del Museo Nacional* 44:139-152, Lima.

1981

Tecnología y organización inca del almacenamiento de víveres en la sierra. In *Runakunap Kawsayninkupaq Rurasqankunaqa: La Tecnología en el Mundo Andino, vol. 36*, eds. H. Lechtman and A. M. Soldi, pp. 327-375. Mexico, D.F.: Universidad Nacional Autónoma de México.

1982

The infrastructure of Inka control in the Peruvian central highlands. In *The Inca and Aztec States, 1400-1800: Anthropology and History*, eds. G. A. Collier, R. I. Rosaldo, and J. D. Wirth, pp. 153-171. New York: Academic Press.

1985

With D. E. Thompson. *Huánuco Pampa: An Inka City and Its Hinterland*. London: Thames and Hudson.

With S. Masuda and I. Shimada, eds. *Andean Ecology and Civilization*. Tokyo: University of Tokyo Press.

From principles of ecological complementarity to the organization and administration of Tawantinsuyu. In *Andean Ecology and Civilization*, eds. S. Masuda, I. Shimada, and C. Morris, pp. 477-490. Tokyo: University of Tokyo Press.

1986

Storage, supply, and redistribution in the economy of the Inka state. In *Anthropological History of Andean Polities*, eds. J. V. Murra, N. Wachtel, and J. Revel, pp. 59-68. New York: Cambridge University Press.

1987

Arquitectura y estructura del espacio en Huánuco Pampa. *Cuadernos del Instituto Nacional de Antropología* 12:27-45. Buenos Aires.

1988

A city fit for an Inka. *Archaeology* 41(5):43-49.

Más allá de las fronteras de Chincha. In *La Frontera del Estado Inca*, eds. T. Dillehay and P. Netherly, pp. 131-140. BAR International Series 442. British Archaeological Reports, Oxford, England.

1991

Signs of division, symbols of unity: Art in the Inka empire. In *Circa 1492: Art in the Age of Exploration*, ed. J. A. Levenson, pp. 521-528. Washington, D.C.: National Gallery of Art and Yale University Press.

1992

Huánuco Pampa and Tunsukancha: Major and minor nodes in the Inka storage network. In *Inka Storage Systems*, ed. T. Y. LeVine, pp. 151-175. Norman: University of Oklahoma Press.

The technology of highland Inka food storage. In *Inka Storage Systems*, ed. T. Y. LeVine, pp. 237-258. Norman: University of Oklahoma Press.

1993

With A. von Hagen. *The Inka Empire and Its Andean Origins.* New York: Abbeville Press.

The wealth of a Native American state: Value, investment, and mobilization in the Inka economy. In *Configurations of Power: Holistic Anthropology in Theory and Practice*, eds. J. S. Henderson and P. J. Netherly, pp. 36-50. Ithaca, N.Y.: Cornell University Press.

1998

Inka strategies of incorporation and governance. In *Archaic States*, eds. G. M. Feinman and J. Marcus, pp. 293-309. Santa Fe, N.Mex: School of American Research.

With A. von Hagen. *The Cities of the Ancient Andes.* London: Thames and Hudson.

2004

Enclosures of power: The multiple spaces of Inka administrative palaces. In *Ancient Palaces of the New World: Form, Function, and Meaning*, eds. S. T. Evans and J. Pillsbury, pp. 299-323. Washington, D.C.: Dumbarton Oaks.

2006

With R. A. Covey. The management of scale or the creation of scale: Administrative processes in two Inka provinces. In *Intermediate Elites in Pre-Columbian States and Empires,* eds. C. M. Elson and R. A. Covey, pp. 136-153. Tucson: University of Arizona Press.

George C. Pimentel

GEORGE CLAUDE PIMENTEL

May 2, 1922–June 18, 1989

BY C. BRADLEY MOORE

GEORGE PIMENTEL WAS AN INTENSE man with a contagious enthusiasm for science, teaching, sports, and all things new and challenging. He was a master of empirical physical models. Pimentel was always looking for the biggest challenges and for truly new phenomena. He was not easily discouraged. When a small spot on his retina kept him from becoming one of the first scientist astronauts, he built a new kind of infrared spectrometer to go look at Mars. In every aspect of his professional life he attacked the big problems head on, and yet at the personal level he always made time to bring along a student or help a friend. He was an enthusiastic and competitive sportsman. His level of exertion and commitment was at least the maximum possible in everything that he did.

George Pimentel's research has had a profound effect on chemistry.[1] The common thread of his research was a desire to understand unusual chemical bonding situations and their consequences for structure and chemical reactivity. The information he obtained on marginal species, on chemical reactions, and on photochemical processes is a key part of the base upon which our understanding of chemical reactions and molecular structure is founded. His fearless approach to exploiting new technology and developing new techniques led to pioneering work in hydrogen bonding (1960,2) and in the structure, bonding, and reactivity of free radicals and other highly reactive molecules (1956; 1960,1; 1963; 1964,2), to the creation of chemical lasers (1964,1; 1965,2; 1967), and to the infrared spectroscopy of the atmosphere and surface of Mars (1969; 1970,1,2; 1974). Pimentel pioneered the spectroscopy of molecules in solid rare gases and other inert matrices beginning in 1954. He observed the first spectra of several free radicals and of many species with unusual bonding (see Table 1). He has provided examples of selectivity for chemical reactions in matrices initiated by infrared excitation of single normal modes (Pimentel, 1958a; 1960,4; 1985,2).

There are few chemists or biochemists who have not benefited from Pimentel's early work (1954, 1957) and his authoritative book (1960,2) on the hydrogen bond. His matrix isolation techniques for trapping reactive molecules in solid rare gases or nitrogen are now used routinely in most chemical research laboratories in the world (1956, 1957, 1960,1). Pimentel pioneered the use of high-speed IR detectors in spectroscopy (1965,1). Few had the courage to copy the spinning grating and fast detectors of his rapid scan infrared spectrometer that extended flash photolysis into the infrared and yielded the reaction kinetics and vibrational spectra of free radicals as well as the discovery of the first chemical lasers (1964,1; 1965,1,2). In the process of developing the chemical laser he exploited it to produce a new level of understanding of energy release to the vibrations and rotations of reaction product molecules (1970,3; 1972; 1973; 1984). The next generation of this spectrometer incorporated a spinning filter wheel, a light-weight body, and a telescope and became the Mariner Mars IR spectrometer (1969; 1970,1,2; 1974). The spectra of Mars yielded concentrations of molecules in the Martian atmosphere, the Martian surface composition, and the topography of Mars (1969; 1970,1,2; 1974).

All in all, Pimentel is exceptionally highly regarded by chemists and spectroscopists for his creativity and insight, for his clear physical models, for his consistent record of opening new fields of great significance to chemistry, and for the care and thoroughness that made his work so eminently reliable.

Pimentel's truly outstanding contributions to science go well beyond his published research to include education from high school through graduate school, university and government service, and leadership in professional societies. He mentored 70 Ph.D. students[1] including 4 who are already members of the National Academy of Sciences and one Nobel laureate. An additional 60 people were postdoc, M.S., or undergraduate members of Pimentel's research group. His research students learned to strive for quality and perfection. His demanding standards; his critical, sharp physical insight; and his energetic enthusiasm in discussing the interpretation of new results inspired many. Pimentel's CHEM Study text (1960,3) introduced a generation of Americans to the excitement of work in science as well as to the basic facts of chemistry. Pimentel taught freshman chemistry to many thousands of students. His course was legendary; he taught with great enthusiasm even through the painful, terminal stages of his colon cancer.

Pimentel won one of Berkeley's distinguished teaching awards (1968) and several national teaching awards. The American Chemical Society's Award for Chemical Education was named the Pimentel Award in his honor; Berkeley's Physical Sciences Lecture Hall became Pimentel Hall in 1994. Pimentel served the nation and the scientific community as deputy director of the National Science Foundation from 1977 to 1980. Upon returning to Berkeley he became an associate director of the Lawrence Berkeley National Laboratory (LBNL) and head of the Laboratory for Chemical Biodynamics, an organized research unit of the College of Chemistry and a division of LBNL. As president of the American Chemical Society for 1986[2] he created National Chemistry Day and National Chemistry Week. His leadership served the profession and the science of chemistry. George Pimentel presented science with eloquence and distinction to our legislators and government executives. Pimentel's National Research Council report, *Opportunities in Chemistry* (1985,1), focused much attention on chemistry in Washington and around the world. Throughout his lively career he was an innovative leader on the Berkeley campus and one of Berkeley's most outstanding classroom teachers. Pimentel's papers are archived for scholars at Berkeley's Bancroft Library, University of California.

George Pimentel was born to French parents near Fresno, in central California. His family moved during the depression to a poor section of Los Angeles, where his parents separated. The children were thereafter supported by their mother. During an interview in the mid-1970s[3] George recounted:

> My father reached only the third grade and my mother was taken out of high school so that she could attend a business school. So their influence did not come through their own educations, but rather through the high value they placed on education. They were very enthusiastic about the academic successes of my brother and me. . . I also gained encouragement from my brother who was only a year and a half older than I, a very bright person. He offered intellectual companionship, guidance, and encouragement to me as the younger brother. We were very close. He was excellent in mathematics and I tried to emulate him in that, as in everything else. . . My father was in construction work, working as a foreman, working with his hands. That led me to contemplate going in that same direction, only in a professional way—trying to realize my father's ambitions that were out of his reach because he didn't have an education. And so my initial expecta-

tion when I got out of high school was that I'd become a civil engineer. . . I have one additional small experience that may have stimulated my interest in science. I attended junior high school in northern Los Angeles and this put me within bicycling distance of Cal Tech. During this time, I occasionally rode my bicycle over to Cal Tech at night to hear popularized lectures on science by Robert Millikan. I found these very exciting.

In 1939 Pimentel began to work his way through the University of California, Los Angeles; his interests shifted from civil to chemical engineering and then to physical chemistry and undergraduate research with J. B. Ramsey. He graduated in 1943 (and received the UCLA Distinguished Alumnus Award in 1979). For his first job he went north to join the Manhattan Project in Berkeley, where he worked on chemical processes for the separation of plutonium with Professor Wendell M. Latimer. In 1944 when he grasped the full implications of the project, however, he enlisted in the Navy and volunteered for submarine duty to do his part in hastening the war's end. At the end of the war he played an important role in establishing the U.S. Office of Naval Research, the beginning of today's government funding for science in universities.

In 1946 he returned to Berkeley for graduate work in infrared spectroscopy with Kenneth Pitzer. Upon earning his Ph.D. in 1949 he joined the Berkeley faculty as an instructor and became an assistant professor in 1951. He remained an active Berkeley faculty member until his death. Pitzer had also joined the Berkeley faculty immediately upon earning his Berkeley Ph.D. with Latimer, who had done likewise following graduate work at Berkeley with Gibson. Thus Pimentel and Pitzer stand as counter examples to the usual wisdom regarding faculty inbreeding. His transition from an impoverished working-class and service background to international fame makes the quintessential American dream a reality.

Pimentel's intense loyalty to the University of California and to chemistry were grounded in the opportunities they afforded him to transform his life and his mind. The Pimentel Memorial Lectureship endowed by IBM and a research award to a graduating senior recognize annually Pimentel's contributions at Berkeley.

INFRARED SPECTROSCOPY, HYDROGEN BONDING, FREE RADICALS, AND MATRIX ISOLATION

Pimentel's publications from his graduate work and his first years on the Berkeley faculty were primarily on the infrared spectroscopy of gases, solutions, and crystals of boranes and hydrocarbons, especially cyclic hydrocarbons. His lifelong interest in unusual chemical bonding is apparent in his first few years on the Berkeley faculty. In 1954 his first papers on the IR spectra of hydrogen-bonded molecules (1954) and on the matrix isolation technique appeared. In the following years he focused on the IR spectra of hydrogen-bonded species (1960,2) of free radicals produced by UV photolysis (see Table 1) and of highly reactive molecules usually isolated in solid rare gas or nitrogen matrices at between 4K and 20K. Pimentel developed the matrix isolation method to permit leisurely infrared spectroscopic study of such species. Fortunately, matrix shifts of infrared bands are quite small, facilitating identification relative to gas phase prototypes. Furthermore, the features are extremely sharp, enhancing sensitivity and resolution of closely spaced lines. Thus, vibrational spectra could be reliably assigned and conclusions drawn regarding the bonding. The first matrix studies were begun by Whittle and Pimentel before 1954, but prototype experiments were successful only after a sustained period of development of reliable low-temperature cells and systematic investigation of the effects of concentration, deposition conditions and temperature upon isolation efficiency, and deposition rate (Becker and Pimentel, 1956; Becker et al., 1957; Van Thiel et al., 1957; Pimentel, 1958a,b; Goldfarb and Pimentel, 1960). Finally, in 1958 this effort was rewarded by the first infrared detection of the molecule HNO (Brown and Pimentel, 1958), soon followed by the detection of HCO (1960,1). Since that time the method has come into full flower; in the 1961-1965 period some 30 diatomic and triatomic transient species were recorded with the matrix method while in the subsequent five-year period the number rose to about 70. Among the transient and unusual molecules first detected in the Berkeley laboratories are those shown in Table 1 (p.10).

Today the infrared spectra of hundreds of free radicals and transient molecules are known through the application of the matrix isolation technique and probably more than three-quarters of these were detected in the Berkeley laboratories or by former Pimentel students. Pimentel also studied many hydrogen-bonded systems in matrices and in 1960 published *The Hydrogen Bond* with McClellan (1960,2), a classic for decades. Many organic and inorganic chemists around the world now routinely study reactive molecules by matrix isolation spectroscopy throughout the UV, VIS, and IR. Most of Pimentel's studies were carried out at 15K to 20K using one to two liters of liquid hydrogen. The apparatus was placed under a large hood with heavy, friable asbestos curtains that covered the hair and clothing of the experimenter. Experiments often lasted several days and involved hot mercury lamps, tired students, many kilograms of mercury inside fragile glass vacuum systems, and other hazards. Thanks to Pimentel's emphasis on safety, hydrogen flames were seen only twice and there were no serious accidents. Free radicals trapped in inert matrices display chemiluminescence on warming to a diffusion temperature. Spectral analysis of this cryogenic chemiluminescence shows the role of excited electronic states in highly exothermic reactions.

TABLE 1 SPECIES OBSERVED BY MATRIX ISOLATION

MOLECULE	REFERENCE
HNO	Brown and Pimentel, 1958
HCO	1960,1
N (thermoluminescence)	Brocklehurst and Pimentel, 1962
O = C = N H + O	Milligan et al., 1962
KrF_2	1963
N_2H_2	Rosengren and Pimentel, 1965
NH	Rosengren and Pimentel, 1965
LiON	Andrews and Pimentel, 1966
FO_2	Spratley et al., 1966; Noble and Pimentel, 1966
CH_3	Andrews and Pimentel, 1967b
CH_3LiCl	Tan and Pimentel, 1968
Cl-ClO	Rochkind and Pimentel, 1967
$(ClO)_2$	Rochkind and Pimentel, 1967; Alcock and Pimentel, 1968
Li	Andrews and Pimentel, 1967a
$XeCl_2$	Nelson and Pimentel, 1967a
Cl_3 (or Cl_3^-)	Nelson and Pimentel, 1967b
HOF	Noble and Pimentel, 1968a
HCl_2 (or HCl_2^-)	Noble and Pimentel, 1968b
Cl_xBr_y	Nelson and Pimentel, 1968
HBr_2 (or HBr_2^-)	Bondybey et al., 1971
iso N_2O_3	Varetti and Pimentel, 1971
H	Bondybey and Pimentel, 1972
NH_3HCl complex	Ault and Pimentel, 1973
^{15}N-PAN	Varetti and Pimentel, 1974
NH_3-LiCl complexes	Ault and Pimentel, 1975

Such studies have revealed previously unobserved electronic states and recombination on electronic hypersurfaces for SO_2*, SO*, S_2*, CO_2*, HCOOH*, C_2H_4*, and BaO* (Long and Pimentel, 1977; Lee and Pimentel, 1978; Fournier et al., 1979; Lee and Pimentel, 1981a,b; Long et al., 1982)

In 1955 Pimentel was recognized by promotion to tenure and by award of a Guggenheim Fellowhip, and in 1959 by promotion to full professor and the Precision Scientific Award in Petroleum Chemistry of the American Chemical Society. Pimentel's lab was always exciting; my years as his student, 1960-1963, seemed particularly so. George had just finished the hydrogen bond book (1960,2), and matrix isolation was an established technique but still delivered new and inexplicable phenomena along with great results. Ken Herr was building the rapid scan IR instrument (1965,1). George was working intensely on the CHEM Study text for high schools (1960,3). There were visitors from around the world. George's administrative assistants, Teri Doizaki, who became the department's management services officer, and Suzy Arbuckle, were hard pressed to keep everything on an even keel. I will always remember a group of us sitting at a picnic table by the pool with David Buckingham at George's home in Lafayette talking quantum mechanics. Driving through the night fog to the Western Spectroscopy Association conference at Asilomar listening to famous professors discuss the latest sense and nonsense from various labs was equally memorable. Deference to rank and seniority was not part of a discussion with George. At the weekly group seminar we learned from George that the literature could contain serious mistakes and that Mother Nature was constantly attempting to lead scientists, and especially oneself, to false conclusions and ruined reputations. For nonscientific diversion we watched to see how long it would take the new postdoc from Europe or Asia to address the professor as George. Although George's first wife, Betty, daughter Chris, and twin daughters, Jan and Tess (early teens in 1960) did not often frequent the lab, the daughters were often in the office and frequently twirled Pop-O's old rotating oak bookshelf. George's family was very much a part of the research group family and vice versa. Parties at home around the pool in summer and during the Christmas break were a regular part of all of George's years in Berkeley.

THE CHEM STUDY PROJECT

In 1960 the CHEM Study project (1960,3) was born under the directorship of J. Arthur Campbell of Harvey Mudd College and guided by a steering committee headed by Nobel Laureate Glenn T. Seaborg. Campbell and Seaborg selected Pimentel to serve as editor of the written materials, with the intimidating challenge of producing an entire book in time for use in the fall of the same year with the help of 20 talented teacher coauthors. As written materials began to accumulate Pimentel organized them, revised them, and infused continuity of style and pedagogy. There were three editions: the first, produced during that first frantic summer and fall of 1960, and then two subsequent revisions in 1961 and 1962 based upon trials in high schools throughout the United States. In each one of these editions virtually every word was handwritten at least once by Pimentel. By the time the hardcover edition appeared in January 1963 it was a smooth, intelligible, and useful text that abruptly brought chemistry instruction in high schools up to date.

Accompanying this book was a set of 26 films. With David Ridgway as film director, Pimentel wrote the scripts for five of these films, appeared in two of them as the principal demonstrator, and narrated the other three. In addition, he appeared in two teacher preparation films. "His filmmaking experience in CHEM Study, combined with his demonstrated interest in science and society, led to his involvement in "Wondering About Things," a film about the controversial role of science and technology in modern life, intended for general audiences. He wrote the first script and appeared briefly in the film, which appeared in 1970 and has been viewed by an estimated 2 million people in public theaters and on television."[4]

The success of the CHEM Study project is indicated by the following statistics:

- Over 1 million copies of the textbook *Chemistry, An Experimental Science* were sold.
- Three authorized but independent revisions of the text have been produced.
- Many subsequent texts have used the CHEM Study inquiry model.
- The text has been translated into the following languages: Chinese (Taiwan), French, German, Gujarati, Hebrew, Hindi, Italian, Japanese, Korean, Portuguese (Portugal and Brazil), Russian (unauthorized), Spanish (Spain and Colombia), Thai, and Turkish.
- The films, always including some of the five films written by Pimentel, have been released as video tapes and translated into the following languages: Danish, French, German, Greek, Italian, Spanish (Spain and Latin America), and Swedish.
- All royalties were returned to the U.S. Treasury and the entire cost of the CHEM Study project was repaid one and a half times.

Pimentel understood the need for a high school chemistry course that would draw people into careers in science and engineering. He saw this as a national and global problem, understood that he could do the job on that scale with the help of good teachers and by building a strong national testing network, and accepted the challenge as presented by his colleague Glenn Seaborg, who was chancellor at the time. During the late 1980s he served as principal investigator for Science for Science Teachers (S_4ST), a summer National Science Foundation program for middle school science teachers, which he had encouraged a local high school physics teacher, Penny Moore, to create. They involved leading Berkeley chemistry, physics, geology, and biology faculty members to enhance the science backgrounds of middle school teachers from all parts of the United States.

INFRARED PHOTOCHEMISTRY

Pimentel (Pimentel, 1958a) and Pimentel and Baldeschwieler (1960,4) opened the field of infrared photochemistry by showing that *cis*↔*trans* isomerization could be caused by excitation of specific vibrational transitions of *cis*-HONO (nitrous acid). This was the first chemical transformation ever induced by infrared photolysis (1960,4; Hall and Pimentel, 1963). Later a similar study was conducted on the light-induced matrix isomerization of unstable forms of N_2O_3 (Varetti and Pimentel, 1971).

The much sought mode-selective excitation of bimolecular chemical reactions is elusive under normal reaction conditions. Pimentel recognized the possibility that a cryogenic matrix might provide environmental conditions in which this goal could be achieved. A number of bimolecular reactions have now been studied in solid inert gas matrices with tuned-laser, hence vibrationally selective, excitation of one of the reactants (Frei et al., 1981; Frei and Pimentel, 1981; Frei and Pimentel, 1983a; Knudsen and Pimentel, 1983; Cesaro et al., 1983; Frei and Pimentel, 1983b; 1985,2). Distinct evidence for mode-specific influence on the quantum yield has been found for the F_2+C_2H_4 reaction, (Frei and Pimentel, 1983a) F_2+*trans*-1,2-$C_2H_2D_2$, (Frei and Pimentel, 1983a) and for F_2+allene (Knudsen and Pimentel, 1983). This is the first clear demonstration of mode-selective excitation of bimolecular reactions. Pimentel's mode-selective bimolecular reactions and conformational changes work because the vibrational modes of small polyatomic molecules are relatively pure and weakly coupled at energies up to a few thousand wave numbers. Reactions with activation energies this low are rapid at room temperature, and thus this selectivity is limited to cryogenic systems.

RAPID SCAN INFRARED SPECTROSCOPY

In 1961 Pimentel set out to do rapid scanning infrared spectroscopy to study the spectra and kinetics of free radicals. Together with his student K. C. Herr he adapted the newly developed fast photoconductive Ge infrared detectors for this purpose (1965,1). They built large flash lamps and long cells for flash kinetic spectroscopy in the infrared. Consequently they improved time resolution in infrared spectroscopy by about six orders of magnitude. The first transient free radicals ever detected by IR in the gas-phase were CF_2 (1965,1) and CF_3 (Carlson and Pimentel, 1966). A series of papers traced the elimination of successive obstacles (1965,1; Pimentel and Herr, 1965; Pimentel, 1965; Herr et al., 1967). As sensitivity was improved, resolution became sufficiently good to reveal the rotational fine structure of CF_2 and consequently its molecular structure (Lefohn and Pimentel, 1971). The rate of recombination of CF_3 radicals to form C_2F_6 was carefully measured and found to have an activation energy near 800 cal rather than the presumed value of zero (Ogawa et al., 1970). The gaseous methyl radical was finally detected and its out-of-plane bending force constant measured (Tan et al., 1972).

Pimentel's laboratory maintained a unique position in rapid scan infrared spectroscopy since no other laboratory in the world was able to do more than duplicate some of the transient species spectra recorded earlier at Berkeley. It was not until the early 1980s that laser techniques began to compete in the recording of infrared spectra of short-lived species.

THE CHEMICAL LASER

With this rapid scan spectrometer Kasper and Pimentel discovered IR light pulses from the first chemical lasers, the iodine photodissociation laser (1964,1) and the HCl chemical laser (1965,2). The short, powerful flash of the rapid scan IR system permitted reactants to be prepared fully mixed on a time scale short compared with reaction times and especially compared with vibrational relaxation times; thus, a gain medium was produced with proper selection of the reaction. The long, multipass cells with mirrors provided the needed feedback and thus laser oscillation. Finally, the detectors permitted the observation of the resultant microsecond pulses. Thus, it was the unique capabilities of the Pimentel lab combined with an understanding of the time scales of mixing, reaction, relaxation, and radiation that permitted Pimentel to succeed where many others had failed.

At the time that they reported their discovery of the iodine photodissociation laser at the first Conference on Chemical Lasers (September 1964), there was speculation about more than 100 possible chemical reactions and 60 photodissociation reactions for producing laser radiation. There were even suggestions that reactions might produce distributions of excited states that were always nearly thermal and never inverted. Despite the multitude of suggestions, the only operating laser mentioned at the San Diego symposium was the I* photodissociation laser (1964,1) described by Kasper and Pimentel.

In 1961 Polanyi first pointed out the possibility of chemically pumped lasers based upon vibrational excitation.[5] In this early and prescient note he proposed four potential reactions, one of which was the $H+Cl_2$ reaction. Following that lead, Kasper and Pimentel published the detection of HCl laser emission from the H_2/Cl_2 explosion in 1965 (1965, 2). Polanyi continued his elegant development of the infrared chemiluminescent method for observing energy distributions in exothermic reactions.[6,7] These complemented the rapid expansion of chemical laser discoveries that emerged from Pimentel's laboratory following the pivotal discovery of the $F+H_2$ laser by Kompa and Pimentel (1967). Thus Pimentel pioneered the conversion of chemical energy released as product vibrational excitation into laser light.

Since that time Pimentel led the chemical laser field with the development of new lasers and with their application to research on reaction dynamics. The emphasis was on the discovery of chemical lasers based upon new types of reactions and on the development of methods by which kinetic information could be extracted from the laser performance. He was able to produce lasers operating on vibrational overtone transitions and on pure rotational transitions of hydrogen halides for states as high as $J = 31$ (1984; Suchard and Pimentel, 1971; Cuellar et al., 1974).

The importance of this field lies in its contributions to our understanding of chemical reaction dynamics and in its potential applications. The two most powerful chemical lasers were both discovered in Pimentel's laboratory, the iodine atom $^2P_{1/2} \rightarrow {}^2P_{3/2}$ photodissociation laser and the $F+H_2$ chemical laser. Chemical lasers offer information about the way energy is distributed among the degrees of freedom of the products. Such information reveals intimate details about the dynamics of the reactive collision and the nature of the transition state. It indicates, through microscopic reversibility, the relative importance of particular degrees of freedom in surmounting an activation energy barrier. It suggests the possibility of controlling chemical reactions through selective excitation of reagent energy states.

In 1966, while the chemical laser work was progressing rapidly, Pimentel was elected to the National Academy of Sciences and in 1968 to the American Academy of Arts and Sciences. In 1985, 1987, and 1989 he was elected to the American Philosophical Society, the Royal Society of Chemistry (Great Britain) as an honorary fellow, and the Royal Institution of Great Britain as an honorary member, respectively.

THE ASTRONAUT COMPETITION

In the middle of his term as Chemistry Department chair (1966-1968) Pimentel applied to become a member of the first cohort of scientist astronauts. Another applicant (personal communication, letter, November 1989, from Gerard K. O'Neill) wrote of that experience:

> When I first met him, it was April 1967, and we two were the oldest among seven finalists being given examinations that week at the Air Force School of Aerospace Medicine. . . Two months later we were together again for a day at the Manned Spaceflight Center at Houston. By then we had learned that in the National Academy of Sciences' evaluation of the thousand candidates who had applied, George had been ranked number one. He would in fact have been taken into the program had it not been for an obscure, very minor abnormality of one retina, something that wouldn't have mattered in any real task. . . In the final selection interview, when he was asked how he would react to being offered a two-year trip to Mars, with high risks involved, his instant answer was where do I sign up?
>
> I have never met anyone else who brought to his teaching, his writing, his research and his administrative and governmental tasks the youthful eager-

ness that George possessed always. His great experience, acquired over many decades, never diminished in the slightest his freshness of outlook, his willingness to welcome new ideas, and his delight in them. His youthful spirit must be one of the main reasons why he is, for all of us, a man to admire and love.

Since these first science astronauts saw very little action and since his contributions in Berkeley were so highly valued, George's friends and family were all quite content that he was not chosen.

INFRARED EXPLORATION OF MARS

The infrared spectroscopic technique furnishes the most definitive analytical technique available for remote determination of the composition of the Martian atmosphere. To conduct such measurements within the space, weight, and power constraints presented to spacecraft instrumentation and at the low light intensities available at Mars required, in 1969, an entirely new instrument design. The rapid scan spectroscopic experiment in Pimentel's laboratories and its builder, K. C. Herr, proved to be the key to this formidable challenge. Pimentel and his coworkers innovated a new type of spectrometer, and took full advantage of the latest developments in semiconductor detector and IR filter technology to reach the sensitivity levels needed. The flight instruments were constructed and assembled in the Berkeley laboratories, notwithstanding vigorous objections from the Jet Propulsion Lab, at which nearly all other instrumentation was built.

In retrospect the *Mariner* 6 and 7 infrared spectrometers can be seen to have been one of the most productive of new scientific information of all of the scientific instruments in those two missions. The primary goal was to determine the atmospheric composition. The data gave quantitatively the presence of three constituents: carbon dioxide, carbon monoxide, and water vapor. The absence of some 39 other possible gaseous constituents was ascertained, with laboratory-established sensitivity limits usually in the parts-per-million range. Notably absent were the molecules that might have been indicative of or relevant to the possible existence of life on Mars—oxides of nitrogen, ammonia, and carbon-hydrogen compounds—as well as those suggestive of active volcanism—hydrogen sulfide or oxides of sulfur (Horn et al., 1972).

The secondary missions of the infrared instrument were equally successful. The characteristic spectral signature of solid CO_2 demonstrated the polar cap composition (1969). The presence of hydrates in the Martian surface minerals was evident and distinguishable from ice near the edge of the receding polar cap (the polar collar) (1974). Upper atmosphere solid CO_2 clouds, analogous to terrestrial cirrus, ice clouds, were detected even near the Martian equator (1970,1). Perhaps most remarkable was the topographical information derived from the spectral data. With the best geographical resolution yet available, the spectrometer displayed surface altitudes varying by 8 kilometers and carbon dioxide surface pressures varying from 3.7 mbars to 8.1 mbars. The most spectacular terrain feature discovered was undoubtedly the region called Hellas, which was found to be a deep depression 1700 kilometers wide and dropping 5.5 kilometers from its rims (1970,2). These results were a truly remarkable achievement. Many doubted that such a spectrometer would be feasible at all, much less that it could be built away from the spacecraft facilities at the Jet Propulsion Laboratories. Most doubted that conclusions could be drawn even if spectra were recorded. Pimentel's truly exceptional creative genius, raw courage, and experimental finesse are clearly displayed in this work.

PUBLIC SERVICE

Pimentel served as deputy director of the National Science Foundation under Richard Atkinson from 1977 to 1980. Upon his return to Berkeley in 1980 Pimentel remarked[8] about his involvement in public service:

It all began right here in Berkeley, when I got involved in an informal evening discussion group organized by some friends in the Poli. Sci. department. Once a month, a handful of us—about half from departments like political science, history, and economics, and the other half from the sciences—would get together, have dinner, and talk about issues in science policy.

The experience of discussing and thinking about science policy led to my accepting a term of service on the National Academy of Sciences' committee on science and public policy. There, I acquired a reputation for talking loud and long about how scientists should get involved in government policy making. So when I was offered the job at NSF, it was really a case of put up or shut up. I accepted the job for an initial two-year term, and then it was extended to a third year because there was still a lot I wanted to accomplish.

Sure I'm glad to be back in academia—back to my research and my students—but I'm glad I did it. It was a valuable experience, and, if anything, I'm talking even more than I was before about the need for scientists to undertake this kind of duty. It has its frustrations; it's hard to move the system sometimes. But if working scientists don't pick up this burden . . .

Pimentel served on National Academy of Sciences' committees including the Panel on Atmospheric Chemistry (1975-1977), Committee on Science and Public Policy (1975-1977), Nominating Committee (chair, 1983), Board on Chemical Sciences and Technology (1982-1988), and the Committee to Survey Opportunities in the Chemical Sciences (chair, 1982-1986). He served NASA on the Lunar and Planetary Missions Board (1967-1970). He served the American Chemical Society on the Editorial Board of *Chemical and Engineering News* (1982-1984), the Committee on Chemistry and Public

Affairs (1982-1984), and as president-elect (1985), president (1986) and immediate past president (1987).

In 1985 the National Academy of Sciences and the National Research Council published the report *Opportunities in Chemistry*, better known as the Pimentel Report for its committee chair (1985,1). Committee member Alan Schriesheim, formerly vice president for research at Exxon and at the time director of Argonne National Laboratory, commented[9] that

> there was a real effort to involve the various segments of the chemistry community—the academic, industry, and government sectors—and then, within these sectors, the various disciplines of chemistry—physical chemistry, organic chemistry, biochemistry, and the rest. Early on the decision was made that the effort would be to identify areas that the community in a sense could coalesce around and say that these were deserving of additional funding. These were to be areas that were particularly exciting whether because of intellectual ferment or because they had high societal impact. The report itself was geared to have impact on policy makers who would be involved in funding decisions.

Pimentel was inspired to take on this enormous task by Presidential Science Adviser George Keyworth's 1982 advice to the President on areas of science that merited more funding. Chemistry was not one of the areas. The report is organized into three areas of service to societal need: (1) new processes, new products, and new materials; (2) food, health, and biotechnologies in relation to the understanding of complex molecular systems; and (3) national well-being through an understanding of the chemistry of the environment, continued economic competitiveness, and increased national security. Although the report did not coincide with federal budget surpluses, it did have an impact on funding decisions in chemistry. In succeeding years I often heard program officers in research agencies argue for support of programs and new initiatives by quoting recommendations from the Pimentel Report. Pimentel stated, "Most of the book is about what chemistry does for society."

In 1987 *Opportunities in Chemistry* appeared under the title *Opportunities in Chemistry: Today and Tomorrow*[10] rewritten to be suitable for advanced high school students and college nonscience majors. His daughter and coauthor, Janice Coonrod, dedicated some of the seven translations of the volume to her father; it reads in part:

> It comes as no surprise that the work to which he devoted his life continues to enlighten and enrich others even after his death. Although this publication is just one achievement in a career studded with outstanding accomplishments, it does in many ways uniquely symbolize the efforts of his lifetime. My father was a tireless advocate of the science of chemistry. It was his strong desire that chemistry might become accessible to all young people from all walks of life so that they might build a citizenry capable of making informed and responsible decisions about the use of chemistry on this planet. It was his wish that the general population might come to appreciate the integral part chemistry plays in solving human problems and responding to society's needs. And foremost, it was his desire to share his unbridled enthusiasm for the science of chemistry and to stimulate, excite, and encourage individuals who might be interested in the study of this amazing discipline.

Notwithstanding his extensive public service, Pimentel vigorously continued his exploration of chemical reactivity through matrix experiments, chemical laser studies, and with new ventures into organometallic chemistry (Weiller et al., 1989), and photochemistry on metal surfaces (1988).

Pimentel was selected for more than a dozen prestigious lectureships at universities throughout the world. He received an extraordinary number of awards and medals, including the Wolf Prize in Chemistry (1982), the U.S. National Medal of Science (1985), the Benjamin Franklin Medal of the Franklin Institute (1985), the Robert A. Welch Award in Chemistry (1986), and the Joseph Priestley Medal of the American Chemical Society (1989), its highest honor. He received honorary degrees from the University of Arizona, Rochester University, and the Colorado School of Mines.

He was a devoted father to Chris, Jan, and Tess, his daughters with his first wife, Betty; loving stepfather of Vincent and Tansy, children of his second wife, Jeanne; and proud grandfather of five grandchildren.

Pimentel prided himself on always keeping in good physical condition. His favorite participation sports were squash (for many years with Berkeley professor Robert E. Connick as a regular and very much taller opponent) and softball (with members of his research group and other chemistry colleagues as participants). He also played many younger colleagues. To judge by conversation at lunch or at Café Strada, to be able to match or better George on the squash court seemed as difficult as achieving promotion to tenure and often the source of comparable satisfaction. Many of us have fond memories of George dressed in sweats heading off to compete wearing glasses with frames that had been epoxy repaired more than once. He was active to the very end, and his energy and enthusiasm and enjoyment of sports characterized his approach to life. He chose his own epitaph: "He went to the ball park every day and he let them know he came to play."

I AM MOST GRATEFUL to George's daughters, Chris, Jan, and Tess; to his wife, Jeanne; to his research students, Lester Andrews, John Baldeschweiler, Ted Becker, Bill Klemperer, and Geri Richmond; and to Jane Scheiber in the University of California chemistry dean's office for valuable additions and corrections to this memoir.

NOTES

1. A complete bibliography of Pimentel's work and a list of his students have been published in *J. Phys. Chem.* 95(1991):2610-2615. His papers are archived at the University of California's Bancroft Library.

2. G. C. Pimentel. A full agenda for ACS in 1986. *Chem. Eng. News*, Jan. 6, 1986, p. 2.

3 G. C. Pimentel and D. Ridgway. Interview with George Pimentel. *J. Chem. Educ.* 51:224 1974.

4. Private communication Jeanne Pimentel.

5. J. C. Polanyi. Proposal for an infrared maser dependent on vibrational excitation. *J. Chem. Phys.* 34(1961):347.

6. J. C. Polanyi and J. L. Schreiber. The dynamics of bimolecular reactions. In *Physical Chemistry—An Advanced Treatise*, vol. VIA, Kinetics of Gas Reactions, eds., H. Eyring, W. Jost, and D. Henderson, p. 383. New York: Academic Press, New York, 1974.

7. J. C. Polyani. Concepts in reaction dynamics. *Accounts Chem. Res.* 5(1972):161-168.

8. J. Goldhaber. The other side of the fence. *LBL Newsmagazine*, winter 1980-1981, p. 12.

9. R. Rawls, J. Long, and J. Krieger. Opportunities in Chemistry: Long-awaited report issued. *Chem. Eng. News*, Oct. 14, 1985, p. 9.

10. G. C. Pimentel and J. A. Coonrod. *Opportunities in Chemistry: Today and Tomorrow.* Washington, D.C.: National Academy Press, 1987.

REFERENCES

Alcock, W. G., and G. C. Pimentel. 1968. Infrared spectrum of dichlorine dioxide, (ClO_2). *J. Chem. Phys.* 48:2373.

Andrews, L., and G. C. Pimentel. 1966. Infrared spectrum, structure and bonding of lithium nitroxide, LiON. *J. Chem. Phys.* 44:2361.

Andrews, L., and G. C. Pimentel. 1967a. Visible spectra of lithium in inert gas matrices. *J. Chem. Phys.* 47:2905.

Andrews, L., and G. C. Pimentel. 1967b. Infrared spectrum of methyl radical in solid argon. *J. Chem. Phys.* 47:3637.

Ault, B. S., and G. C. Pimentel. 1973. Infrared spectra of the ammonia-hydrochloric acid complex in solid nitrogen. *J. Phys. Chem.* 77:1649.

Ault, B. S., and G. C. Pimentel. 1975. Matrix isolation infrared studies of lithium bonding. *J. Phys. Chem.* 79:621.

Becker, E. D., and G. C. Pimentel. 1956. Spectroscopic studies of reactive molecules by the matrix isolation method. *J. Chem. Phys.* 25:224.

Becker, E. D., G. C. Pimentel and M. Van Thiel. 1957. Matrix isolation studies: Infrared spectra of intermediate species in the photolysis of hydrazoic acid. *J. Chem. Phys.* 26:145.

Bondybey, V., and G. C. Pimentel. 1972. Infrared absorption of interstitial hydrogen atoms in solid argon and krypton. *J. Chem. Phys.* 56:3832.

Bondybey, V., G. C. Pimentel, and P. N. Noble. 1971. Hydrogen dibromide radical: Infrared detection through the matrix isolation technique. *J. Chem. Phys.* 55:540.

Brocklehurst, B., and G. C. Pimentel. 1962. Thermoluminescence of solid nitrogen after electron bombardment at 4.2°K. *J. Chem. Phys.* 36:2040.

Brown, H. W., and G. C. Pimentel. 1958. The photolysis of nitromethane and of methyl nitrile in an argon matrix; infrared detection of nitroxyl, HNO. *J. Chem. Phys.* 29:883.

Carlson, G. A., and G. C. Pimentel. 1966. Infrared detection of gaseous trifluoromethyl radical. *J. Chem. Phys.* 44:4053.

Cesaro, S. N., H. Frei, and G. C. Pimentel. 1983. Vibrational excitation of the reaction between vinyl bromide and fluorine in solid argon. *J. Phys. Chem.*. 87:2142.

Cuellar, E., J. H. Parker, and G. C. Pimentel. 1974. Rotational chemical lasers from hydrogen fluoride elimination reactions. *J. Chem. Phys.* 61:422.

Fournier, J., J. Deson, C. Vermiel, and G. C. Pimentel. 1979. Fluorescence and thermoluminescence of N_20, CO, and $C0_2$ in an argon matrix at low temperature. *J. Chem. Phys.* 70:5726.

Frei, H., L. Fredin, and G. C. Pimentel. 1981. Vibrational excitation of ozone and molecular fluorine reactions in cryogenic matrices. *J. Chem. Phys.* 74:397.

Frei, H., and G. C. Pimentel. 1981. Reaction of nitric oxide and ozone in cryogenic matrices: quantum-mechanical tunnelling and vibrational enhancement. *J. Phys. Chem.* 85:3355.

Frei, H., and G. C. Pimentel. 1983a. Selective vibrational excitation of the ethylene-fluorine reaction in a nitrogen matrix. I. *J. Chem. Phys.* 78:3698.

Frei, H., and G. C. Pimentel. 1983b. Selective vibronic excitation of singlet oxygen-furan reactions in cryogenic matrices. *J. Chem. Phys.* 79:3307.

Goldfarb, T. D., and G. C. Pimentel. 1960. Spectroscopic study of the photolysis of diazomethane in solid nitrogen. *J. Am. Chem. Soc.* 82:1865.

Hall, R. T., and G. C. Pimentel. 1963. Isomerization of nitrous acid: An infrared photochemical reaction. *J. Chem. Phys.* 38:1889.

Herr, K. C., G. A. Carlson and G. C. Pimentel. 1967. Investigations of free radical reactions with rapid scan infrared spectroscopy: Flash noise as a limiting factor. *Kagaku no Ryoiki* 21:12.

Horn, D., J. McAfee, A. Winer, K. C. Herr, and G. C. Pimentel. 1972. The composition of the martian atmosphere: Minor constituents. *Icarus* 16:543.

Knudsen, A. K., and G. C. Pimentel. 1983. Vibrational excitation of the allene-fluorine reactions in cryogenic matrices: Possible mode selectivity. *J. Chem. Phys.* 78:6780.

Lee, Y. P., and G. C. Pimentel. 1978. Chemiluminescence of $SO(\tilde{c}^1\Sigma \rightarrow \tilde{a}^1\Delta)$ in solid argon. *J. Chem. Phys.* 69:3063.

Lee, Y. P., and G. C. Pimentel. 1981a. Formic acid chemiluminescence from cryogenic reaction between triplet methylene and oxygen. *J. Chem. Phys.* 74:4851

Lee, Y.-P., and G. C. Pimentel. 1981b. Chemiluminescence of ethylene in an inert matrix and the probable infrared spectrum of methylene. *J. Chem. Phys.* 75:4241.

Lefohn, A. S., and G. C. Pimentel. 1971. The infrared spectrum of gaseous CF_2 by rapid scan spectroscopy. *J. Chem. Phys.* 55:1213.

Long, S. R., Y.-P. Lee, O. D. Krogh, and G. C. Pimentel. 1982. The chemiluminescent reactions $Ba+N_20$ and $Ba+0_3$ in solid argon. *J. Chem. Phys.* 77:226.

Long, S. R., and G. C. Pimentel. 1977. Chemiluminecent reactions of sulfur ($^3P_2$) atoms in cryogenic matrices: $S+0_2 \rightarrow S0_2$ ($\tilde{a}\ ^3B_1$). *J. Chem. Phys.* 66:2219.

Milligan, D. E., M. E. Jacox, S. W. Charles, and G. C. Pimentel. 1962. Infrared spectroscopic study of the photolysis of HN_3 in solid CO_2. *J. Chem. Phys.* 37:2302.

Nelson, L. Y., and G. C. Pimentel. 1967a. Infrared detection of xenon dichloride. *Inorg. Chem.* 6:1758.

Nelson, L. Y., and G. C. Pimentel. 1967b. Infrared detection of the trichloride radical, Cl_3. *J. Chem. Phys.* 47:3671.

Nelson, L. Y., and G. C. Pimentel. 1968. Infrared spectra of chlorine-bromine polyhalogens by matrix isolation. *Inorg. Chem.* 7:1695.

Noble, P. N., and G. C. Pimentel. 1966. Confirmation of the identification of dioxygen monofluoride. *J. Chem. Phys.* 44:3641.

Noble, P. N., and G. C. Pimentel. 1968a. Hypofluorous acid: Infrared spectrum and vibrational potential function. *Spectrochim. Acta* 24A:797.

Noble, P. N., and G. C. Pimentel. 1968b. Hydrogen dichloride radical: Infrared detection through the matrix isolation technique. *J. Chem. Phys.* 49:3165.

Ogawa, T., G. A. Carlson, and G. C. Pimentel. 1970. Reaction rate of trifluoromethyl radicals by rapid scan infrared spectroscopy. *J. Phys. Chem.* 74:2090.

Pimentel, G. C. 1958a. Reaction kinetics by the matrix isolation method: diffusion in argon; *cis-trans* isomerization of nitrous acid. *J. Am. Chem. Soc.* 80:62.

Pimentel, G. C. 1958b. The promise and problems of the matrix isolation method for spectroscopic studies. *Spectrochim. Acta* 12:94.

Pimentel, G. C. 1965. Infrared detection of reactive species produced through flash photolysis. *Pure Appl. Chem.* 11:563.

Pimentel, G. C., and K. C. Herr. 1965. The infrared detection of free radicals using flash photolysis methods. *J. Chim. Phys.* 61:1509.

Rochkind, M. M., and G. C. Pimentel. 1967. Photolysis of matrix-isolated dichlorine monoxide: infrared spectra of ClClO and $(ClO)_2$. *J. Chem. Phys.* 46:4481.

Rosengren, K. J., and G. C. Pimentel. 1965. Infrared detection of diimide, N_2H_2, and imidogen, NH, by the matrix isolation method. *J. Chem. Phys.* 43:507.

Spratley, R. D., J. J. Turner, and G. C. Pimentel. 1966. Dioxygen monofluoride: infrared spectrum, vibrational potential function and bonding. *J. Chem. Phys.* 44:2063.

Suchard, S. N., and G. C. Pimentel. 1971. A deuterium fluoride vibrational overtone chemical laser. *Appl. Phys. Lett.* 18:530.

Tan, L. Y., and G. C. Pimentel. 1968. Methyl alkali halides: A new molecular type; infrared spectra by the matrix isolation technique. *J. Chem. Phys.* 48:5205.

Tan, L. Y., A. M. Winer, and G. C. Pimentel. 1972. Infrared spectrum of gaseous methyl radical by rapid scan spectroscopy. *J. Chem. Phys.* 57:4028.

Van Thiel, M., E. D. Becker, and G. C. Pimentel. 1957. Infrared studies of hydrogen bonding of methanol by the matrix isolation technique. *J. Chem. Phys.* 27:95.

Varetti, E. L., and G. C. Pimentel. 1971. Isomeric forms of dinitrogen trioxide in a nitrogen matrix. *J. Chem. Phys.* 55:3813.

Varetti, E. L., and G. C. Pimentel. 1974. The infrared spectrum of ^{15}N-labeled peroxyacetylnitrate {PAN} in an oxygen matrix. *Spectrochim. Acta* 30A:1069.

Weiller, B. H., E. P. Wasserman, and G. C. Pimentel. 1989. Time-resolved IR spectroscopy in the liquid rare gases: Direct rate measurement of an intermolecular alkane C-H oxidative addition reaction. *J. Am. Chem. Soc.* 111:8388.

SELECTED BIBLIOGRAPHY

1954

With W. Klemperer. Hydrogen bonding in sodium trifluoroacetate-trifluoroacetic acid compounds. *J. Chem. Phys.* 22:1399-1402.

1956

With D. E. Milligan and H. W. Brown. Infrared absorption by the N_3 radical. *J. Chem. Phys.* 25:1080.

1957

With M. Van Thiel and E. D. Becker. Infrared studies of hydrogen bonding of water by the matrix isolation technique. *J. Chem. Phys.* 27:486-490.

1960

[1] With G. E. Ewing and W. E. Thompson. The infrared detection of the formyl radical HCO. *J. Chem. Phys.* 32:927-932.

[2] With A. L. McClellan. *The Hydrogen Bond.* San Francisco: W. H. Freeman.

[3] *Chemistry—An Experimental Science.* San Francisco: W. H. Freeman.

[4] With J. D. Baldeschwieler. Light-induced *cis-trans* isomerization of nitrous acid formed by photolysis of hydrazoic acid and oxygen in solid nitrogen. *J. Chem. Phys.* 33:1008.

1963

With J. J. Turner. Krypton fluoride: Preparation by the matrix isolation technique. *Science* 140:974-975.

1964

[1] With J. V. V. Kasper. Atomic iodine photodissociation laser. *Appl. Phys. Lett.* 5:231-233.

[2] With C. B. Moore. Matrix reaction of methylene with nitrogen to form diazomethane. *J. Chem. Phys.* 41:3504-3509.

1965

[1] With K. C. Herr. A rapid-scan infrared spectrometer; flash photolytic detection of chloroformic acid and of CF_2. *Appl. Opt.* 4:25-30.

[2] With J. V. V. Kasper. HCl chemical laser. *Phys. Rev. Lett.* 14:352-354.

1967

With K. L. Kompa. Hydrofluoric acid chemical laser. *J. Chem. Phys.* 47:857-858.

1969

With K. C. Herr. Infrared absorptions near three microns recorded over the polar cap of Mars. *Science* 166:496-499.

1970

[1] With K. C. Herr. Evidence for solid carbon dioxide in the upper atmosphere of Mars. *Science* 167:46-49.

[2] With K. C. Herr, D. Horn, and J. M. McAfee. Martian topography from the Mariner 6 and 7 infrared spectra. *Astron. J.* 75:883-894.

[3] With M. J. Berry. Vibrational energy distribution in the dichloroethylene photoelimination chemical lasers. *J. Chem. Phys.* 53:3453-3460.

1972

With M. J. Molina. Tandem chemical laser measurements of vibrational energy distribution in the dichloroethylene photoelimination reactions. *J. Chem. Phys.* 56:3988-3993.

1973

With R. D. Coombe. The effect of rotation on the vibrational energy distributions in the reaction $F + H_2$. *J. Chem. Phys.* 59:1535-1536.

1974

With P. Forney and K. C. Herr. Evidence about hydrate and solid water in the Martian surface from the 1969 Mariner infrared spectrometer. *J. Geophys. Res.* 79:1623-1634.

1978

With J. P. Reilly, J. H. Clark, and C. B. Moore. HCO production, vibrational relaxation, chemical kinetics, and spectroscopy following laser photolysis of formaldehyde. *J. Chem. Phys.* 69:4381-4394.

1984

With G. L. Richmond. HF rotational laser emission from the ClF/H_2 reaction: Time evolution of the gain. *J. Chem. Phys.* 80:1162-1170.

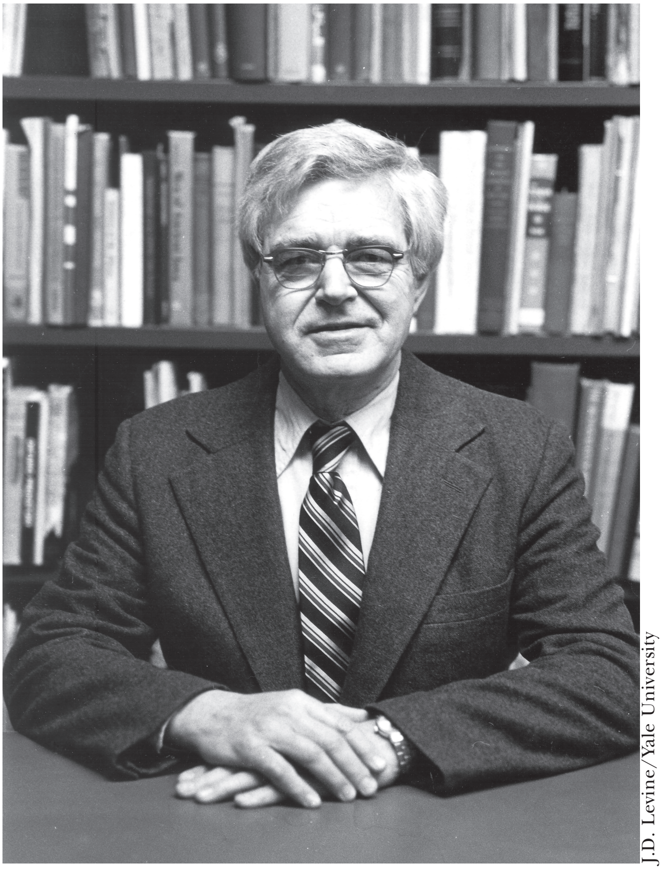
1985

[1] *Opportunities in Chemistry*. A report by the National Research Council's Committee to Survey Opportunities in the Chemical Sciences, George C. Pimentel, Chairman. Washington, D.C.: National Academy Press.

[2] With H. Frei. Infrared induced photochemical processes in matrices. *Ann. Rev. Phys. Chem.* 36:491-524.

1988

With V. M. Grassian. Photochemical reactions of *cis*- and *trans*-1, 2-dichloroethene adsorbed on Pd(111) and Pt(111). *J. Chem. Phys.* 88:4484-4491.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BENJAMIN IRVING ROUSE

August 29, 1913–February 4, 2006

BY WILLIAM F. KEEGAN

HIS FRIENDS AND COLLEAGUES KNEW HIM as "Ben." As he explained it, "My dad was Irving Rouse. I'm Ben" (in Drew, 2006), yet in all of his publications he used the name Irving Rouse. Like Christopher Columbus he "discovered" the native peoples of the Caribbean, and through his work our understanding of these peoples has been enhanced greatly. Moreover, the results of his research usually were published in a timely manner, and the notes and detailed drawings form an important corpus of data that is as useful today as it was 50 years ago. Ben's book *The Tainos: Rise and Decline of the People Who Greeted Columbus* (1992) has been hugely popular and widely read, and introduced the archaeology of this region to numerous people who otherwise might not be interested.

BEN'S FAMILY

Ben was born in Rochester, New York, on August 29, 1913. His father, who also graduated from Yale, owned a nursery, and Ben grew up with an interest in plants. He began his career at Yale in 1930; he was 17 years old. His undergraduate work was in plant science, and he intended to go into forestry. As he describes it, he took the $500 his family gave him for school and put it in the bank. But this was the year after the stock market collapse and the bank failed. Ben lost all his money. Faced with the need to fend for himself he took whatever job was available. At first this was mowing lawns and raking leaves on the Yale campus, but in time Cornelius Osgood developed a liking for Ben and put him to work cataloging anthropology collections in the Yale Peabody Museum. Osgood's confidence in Ben started him on his path in anthropology, a path that led to an extraordinary 70-year career.

On June 24, 1939, Ben married Mary Mikami. Mary was herself an extraordinary person. She came from an aristocratic family in Japan, where her father was an accomplished naval officer. Her family moved to the United States in the early 1900s, and she was born in San Francisco in 1912. After moving to Alaska, she was involved in anthropology projects and studied at the University of Alaska, where she met Froelich Rainey, who convinced her to pursue a Ph.D. at Yale. There she met and married Ben. They had two sons, David and Peter. David became an urban landscape architect in Philadelphia, following in the family tradition. Peter was the chief of staff to Tom Daschle and currently is the chief of staff for Barack Obama in the U.S. Senate. Mary Mikami was Ben's lifelong companion, and at times collaborator, until her death at the age of 87 on August 7, 1999. Her passing was memorialized in the U.S. Senate (Congressional Record, 1999).

BEN AND YALE

Working 20 hours a week and studying full time through the lean years of the Great Depression, Ben completed his B.S. in plant science at Yale's Sheffield Scientific School in 1934. This was a very prestigious accomplishment. However, by his junior year Ben had decided that he did not want to be a forester. Plant science, in his opinion, was a "mature field of study," and he became fascinated with the fledgling field of anthropology through his work on anthropological collections in the Yale Peabody Museum. At Osgood's urging he began to take graduate classes in anthropology.

Ben completed his Ph.D. in 1938. It was later published by Yale University Publications in Anthropology in two parts. The first dealt with methods of analysis entitled *Prehistory in Haiti: A Study in Method* (1939); the second focused on the application of these methods and was called *Culture of the Ft. Liberté Region, Haiti* (1941). After completing his Ph.D., Ben was hired by the Peabody Museum of Natural History as an assistant curator; he was promoted to associate curator in 1947 and research associate in 1954. He was instructor in anthropology from 1939 to1943, became an assistant professor in 1943, associate professor in 1948, professor in 1954, and MacCurdy professor in 1970. During his years at Yale, he served in a number of administrative capacities, including director of graduate studies (1953-1957, 1969-1972), department chair (1957-1963), director of undergraduate studies (1967-1970), and chair of the interdisciplinary archaeology program beginning in 1970.

Ben was the Charles J. MacCurdy Professor of Anthropology and curator of anthropology when he officially retired in 1984, but he never stopped working. A most admirable quality was his dedication to getting his archaeological research into press. His investigations at the Hacienda Grande site were published in 1990, and despite the fact he completed the fieldwork in Antigua in 1973, he pushed on to publish the results of this work (1999).

MAJOR CONTRIBUTION: TIME-SPACE SYSTEMATICS

My main recollection of graduate school was that the professors had diverse conflicting points of view. As an undergraduate I had been led to believe that there was a right way of doing things and all I had to do was learn what it was. It bothered me at the time, but when I look back I think it was very good for me. It forced me to develop my own viewpoint and to be open to other points of view. One of the major influences on my thinking was the linguistic method of analysis (Siegel, 1996, p. 672).

This quote is the perfect summation of Ben's perspective. He really believed that there was one right way to do things. Starting from a strong background in taxonomy and influenced by linguistics, he sought to develop a method of classification that could be applied universally. He believed that classification was knowledge: If you could identify cultures and place them in the appropriate boxes of time and space, you would produce a complete culture history. Ben never liked the messiness of anthropology.

His contributions to classification are legion. He was a major player in the debates concerning archaeological taxonomy. He developed a unique scheme for classifying archaeological materials based on modal analysis. His approach was first published in 1939 in a publication that remains a classic work that is as relevant today as it was 70 years ago (*Prehistory in Haiti: A Study in Method*). Even though his scheme was never widely adopted, Willey and Sabloff (1974) in their book *A History of American Archaeology* recognized his contributions by placing him at the base of the tree from which modern American archaeology developed. Through the years Ben revised his time-space diagram for Caribbean cultures with the belief that every refinement moved us that much closer to understanding the past. It has formed the foundation for Caribbean culture history for over 50 years.

Ben took a sabbatical in England in 1963-1964. He received a Guggenheim Foundation Fellowship and was hosted by the Institute of Archaeology at the University of London. He expressed gratitude to the Guggenheim Foundation for suggesting that he go to Europe to expand his horizons: "I am particularly grateful to the Guggenheim selection committee for recognizing my parochialism and insisting that I go to Europe to correct it" (1972, p. xvii). At the time, he was working on three books. The first, *Introduction to Prehistory: A Systematic Approach* (1972), dealt with the methods of prehistory. The other two were to deal with aspects of world prehistory. This book clearly illustrates Ben's interest in a "linguistic approach." It is essentially a lexicon and grammar for describing archaeological materials and their relationships with regard to the identification of "peoples and cultures" from an admittedly "normative" perspective.

Yet Ben never moved beyond his undergraduate belief that there was only one correct way to study the past. He commented, "As I look back, I'm impressed by the fact that archaeology in the 1960s had reached the same state of maturity in classification that biology had reached when I was an undergraduate" (Siegel, 1996, p. 672). He goes on to say, "Just before the revolution in archaeology took place, archaeologists had very high prestige in the discipline of anthropology because we knew what we wanted to do. Then Binford and his generation destroyed all that" (Siegel, 1996, p. 677).

Ben may have claimed to be "open to other points of view," but he really was not. He ruled the Caribbean with an iron fist for many years and if your grant proposal or peer-reviewed article did not fit with his approach (and he seems to have reviewed them all), they were not funded or published. We had a particularly nasty exchange in the late 1980s. *American Anthropologist* asked him to submit a paper on the origins of the Taínos. No one would review it. As a naïve young assistant curator, I accepted the challenge and wrote a scathing review. The other reviewers must have done the same because the article was rejected. Ben was furious! When he learned that I had been a reviewer, he told me that the only reason I had a job doing Caribbean archaeology was because I had "snuck in through the Bahamas." I know of a similar exchange during fieldwork in Antigua in 1974, during which he told his graduate student to just do the best job he could. When he found out that this student was applying the theories and methods of the "New Archaeology," Rouse was outraged. I relate these stories not to chastise Ben. He was always a gracious host and gentleman when I visited him at Yale. But it is important to recognize that he had a very particular mindset, and that he spent his career trying to develop the one correct way of doing archaeology. He set himself an impossible task.

ROUSE IN THE CARIBBEAN

Professor Cornelius Osgood arrived at Yale in 1930. Following the lead of Franz Boas, Osgood was interested in developing a comprehensive and systematic program of regional study, and with private backing he initiated the Caribbean Anthropological Program (CAP) in 1933 with the assistance of Froelich Rainey. The program included social anthropology as well as archaeology, and Professor Sidney Mintz was an early participant. The program never had substantial funding, but it encouraged interest in an area that previously had been neglected.

It is not clear why the Caribbean was chosen. Rouse (Siegel, 1996, p. 682) suggests that Charlotte Gower, a fellow graduate student at the University of Chicago, influenced Osgood's choice of area. Gower wrote her dissertation on the West Indies and considered possible connections between the Caribbean and Florida (Gower, 1927). It is perhaps for this reason that Florida originally was included in the CAP. In fact, Florida became something of a refuge for the Caribbean program. As Ben recounts, "After World War II it was difficult to get back into the West Indies. Transportation patterns hadn't been reestablished. I wanted to go into the field, so Osgood suggested that I work in Florida. At the same time John Goggin came to Yale as a graduate student" (Siegel, 1996, p. 682). But the interest in Florida soon waned; "after realizing that there was really little relationship between Florida and the West Indies, we dropped Florida from the program" (Siegel, 1996, p. 682).

Private sponsorship provided Yale graduate student Froelich Rainey the opportunity to sail through the Bahamas in search of archaeological sites in 1933. Rainey failed to find anything of significance in the Bahamas (but see Keegan, 1992), and so he and his patron turned to Haiti. One of CAP's first projects was an archaeological investigation of Haiti. As mentioned, Osgood took an immediate liking to the young and industrious Ben Rouse and put him to work cataloging anthropological collections in the Peabody Museum, and encouraged him to pursue graduate studies in anthropology. Ben was sent to Haiti with Rainey in 1934, and these investigations formed the basis for his dissertation (1938).

Reading between the lines, there seems to have been some tension between Rouse and Rainey. In his memoir, *Reflections of a Digger: Fifty years of Archaeology*, Rainey (1992) devotes a chapter to his experiences in the Caribbean. He comments that all of the ideas he proposed for the islands had been overturned, but goes on to say that some people still think he was right. Of course it was Rouse who rejected Rainey's ideas. Rainey (1992, p. 43) reminisces that years later when introducing Ben at a lecture: "My clearest memory of him was sitting in a small Haitian jail with me, while the local police chief and his men dug out a large mound at Meillac in north Haiti, where they thought we were digging for pirate treasure. Perhaps that was a good beginning for a very serious and very academic sort of youngster." Rainey left the Caribbean in 1935 to conduct research in Alaska, and Ben took over.

Ben next worked in Puerto Rico in 1936, 1937, and 1938 as part of the Scientific Survey of Porto Rico and the Virgin Islands sponsored by the New York Academy of Sciences. This was a landmark program combining investigations by numerous scientists investigating all aspects of the environment and archaeology on the island. Rainey had conducted surveys and excavations in Puerto Rico in 1934 under the direction of the Peabody Museum. Ben followed up on Rainey's (1940) work, and their differences in interpretation are clear and evident. Whereas Rainey believed that the different cultural assemblages that he identified reflected different migrations, Ben believed that they were part of a single line of development. These differences contributed to Ben's lifelong focus on the meaning and identification of human migrations (1986). Ben objected to the general application of the European Conquest Model to explain all migrations, and he demonstrated that one model cannot possibly fit all cases (Drew, 2006).

Between May and September 1941, Ben and Osgood conducted research in Cuba that was sponsored by the Institute of Andean Research. Osgood (1942) focused on the Archaic cultures (Ciboney), while Rouse investigated what would become known as the Ceramic age. He recalls,

> "Sr. Orencio Miguel Alonso…took me to most of the sites I visited in the municipalities of Banes and Antilla. His automobile, 'Drácula,' proved indispensable in this work, for it could go all places where mine could not. My excavations were carried out by the Boy Scouts of the Banes troop under his direction" (1942, p. 6).

Ben studied Caribbean collections in European museums in 1939. The most significant of these, in his opinion, was J. A. Bullbrook's collection from Trinidad, which was housed in the British Museum. Bullbrook had written a detailed accounting of his work and finds in 1919, but these had never been published. Rouse (1953) recognized the significance of this work because it provided a detailed accounting of stratigraphic relationships "sixteen years before Rainey's pioneer stratigraphic research in Puerto Rico." Ben spent 10 weeks in Trinidad in the summer of 1946, collaborating with Bullbrook, and editing his manuscript (Bullbrook, 1953). He did additional work on the island with John Goggin in 1953. Excavations at the sites of Cedros and Ortoire would become the type-sites for the Cedrosan Saladoid and Ortoiroid series of Caribbean peoples and cultures. These established an eastern South American origin for the Ceramic and Archaic ages, respectively. He returned to Trinidad with Fred Olsen in 1969 and collected samples for radiocarbon dating.

Osgood and George Howard conducted preliminary surveys of Venezuela and Trinidad in 1941. They were followed a year later by José María Cruxent, who recently had moved to Venezuela from Barcelona, Spain, during the Spanish Civil War. Cruxent would spend the next 16 years investigating Venezuelan prehistory. Ben worked with Cruxent in 1946, 1950, 1955, and 1956-1957. Here, on the banks of the Orinoco River, Ben found the evidence he needed to challenge Julian Steward's circum-Caribbean theory (see below).

Beginning in the 1950s, a variety of the new investigations were conducted by his students: Robert Howard in Jamaica (1950), Marshall McKusick in St. Lucia (1960), Paul Gene Hahn in Cuba (1961), and Louis Allaire in Martinique (1977). What is surprising is that only Allaire, of all of his students, continued to conduct research in the Caribbean after completing their dissertations.

Ben used the results of his students and other investigators to fill in the gaps in his chart. All of these excavations contributed to Ben's increasingly detailed diagrams of the "peoples and cultures" of the Caribbean. Beginning with the classification techniques first developed in his dissertation, he plotted the distributions of like materials in time and space (with space on the x axis and time on the y axis). A major breakthrough came in the 1950s with the development of radiocarbon dating. Ben received a National Sciences Foundation grant in 1963 to obtain the first radiocarbon dates for the region. Minze Stuiver analyzed a total of 31 samples from Venezuela, Guadeloupe, and Puerto Rico at the Yale Geochronology Lab. More dates were obtained in succeeding years, and Ben was then able to refine the chronology for the region (1978).

Ben's final major field project was conducted in the summer of 1973 on Antigua. Fred Olsen, who developed a method for safely packing high explosives and was head of explosives and ammunition research for the U.S. Army until 1929, had built a winter home on Antigua in 1954. Olsen was an avid amateur archaeologist and began excavating a site at Mill Reef. He invited Ben to Antigua to help them learn to excavate properly. Ben accepted the invitation and spent 10 days helping the "Mill Reef Diggers" in 1956. As Olsen (1974a, p. 26) recounts, Ben "immediately captivated Mill Reef with his modesty, consideration, and patience."

Disappointed that the Mill Reef site was small and the artifacts relatively unspectacular, Olsen continued his explorations of archaeological sites on the island. With the discovery of the Indian Creek site he believed that he finally had a spectacular site, and Ben was again invited to the island to conduct systematic excavations. It is my understanding that Ben did not want to do the project, and after an initial visit to the site in 1969, he held out for several more years. However, by this time Olsen had amassed a fabulous collection of pre-Columbian artifacts and Yale was cultivating him for a major donation (George Kubler wrote the foreword to Olsen's book [1974a]).

Ben eventually relented and in May 1973 he and graduate student Dave D. Davis went to Antigua. Davis did the initial survey of Indian Creek and then directed the excavation of Archaic sites (Davis, 2000), while Rouse with local help excavated Indian Creek (1999). During the excavation of the final trench (trench 7) in June, Ben suffered a heart attack and spent the next five weeks in the hospital (Olsen, 1974b). Despite his reluctance to participate in this project, the results from Indian Creek and Jolly Beach served to define the culture history of the northern Lesser Antilles. A second important outcome of this work was the encouragement he gave to Desmond Nicholson. Nicholson, a long-time resident of Antigua, had a more archaeological focus than did Olsen (who seems to have been more interested in artifacts). Desmond went on to found the Museum of Antigua and Barbuda, and was the driving force behind archaeological investigations on the island for decades.

Although he never directed another research project, over the years Ben visited numerous ongoing excavations. Young researchers especially sought Ben's sage advice and wisdom. For example, Shaun Sullivan brought Ben to Middle Caicos (Turks & Caicos Islands) in 1977. Sullivan (1981) had discovered the first "ball court" in the Bahama archipelago at site MC-6. Given this unique discovery, Sullivan sought Ben's opinion. Getting to MC-6 required following a treacherous 3.5-km-long trail. When Ben fell on the trail, Sullivan feared that he had killed him!

We need also to consider something of an enigma. The International Association for Caribbean Archaeology, albeit known by different names at different times, has been the primary forum for Caribbeanists for the past 45 years. Typically, the Congress meets every other year on a different Caribbean island. Ben attended what was then called the First International Convention for the Study of pre-Columbian Culture in the Lesser Antilles held at Fort-de-France, Martinique, in July 1961. Father Robert Pinchon organized this convention, and the debates were so contentious I have been told that distribution of the publication was suppressed. Proceedings of the business meeting for the second "congress" indicate that Pinchon had asked Ben, Ripley Bullen, and William Haag to serve as an advisory committee. The second congress was supposed to be organized by Thomas J. Maxwell in Puerto Rico in 1963, but Maxwell left Puerto Rico before the meeting came to fruition. Ripley Bullen picked up the slack and with Neville Connell, director of the Barbados Museum, the second congress was held in Barbados in 1967. Bullen was named the permanent chair, and he organized biennial meetings and published congress proceedings until his death in 1977.

What is surprising is that Rouse, despite his status as a founder of Caribbean archaeology, did not support this organization. He did not attend the second meeting in Barbados (in 1967), the third in Grenada (in 1969), the fourth in St. Lucia (in 1971), and although he published a paper in the proceedings of the fifth congress (in Antigua in 1973) he was not in attendance, probably due to his heart attack just prior to the congress. He did attend the sixth congress in Guadeloupe in 1975 but not the seventh or eighth. Despite this apparent lack of interest in the organization, he was recognized for his contributions to Caribbean archaeology, along with Jacques Petitjean Roget, at the 16th congress in Guadeloupe (in 1995). Why did the "father" of Caribbean archaeology not participate on a more regular basis in meetings of Caribbean archaeologists?

I do not know the answer, but I can offer speculations. At the first convention Father Pinchon, an amateur archaeologist, was brutal in his questioning of Ben's interpretations. The permanent chair of subsequent congresses was Ripley Bullen, who did not have a Ph.D. and who developed a different concept of ceramic "series." The congress also had a more amateur feel to it, especially in the early years. Moreover, archaeologists from Hispanic countries, notably Mario Sanoja and Iraida Vargas in Venezuela and Marcio Veloz Maggiolo in the Dominican Republic were pursuing Marxist explanations for cultural developments in the Caribbean (*modo de vida*). French archaeologists were pursuing their own agenda. Clifford Evans and Betty Meggers continued to promote Julian Steward's notion of a Formative that derived from migrations out of Andean South America (circum-Caribbean chiefdoms). As mentioned earlier, Rouse believed that there was one correct way to do archaeology. At Caribbean congresses he would have had to confront a diversity of approaches and a chaotic view of archaeology. I suspect he believed that the effort was not worth his time.

We also need to recognize Ben's contributions to Connecticut archaeology. "I did local archaeology. Quite a bit of it. I was 16 years old when I became involved with the ASC" (Drew, 2006). The Archaeological Society of Connecticut (ASC) was officially founded in 1934 with the goal of training archaeologists to complete the archaeological survey of the state. Osgood was the first president. Ben was the first secretary-treasurer, then secretary, and he was editor of the *ASC Bulletin*. In the late 1950s it was decided that the University of Connecticut at Storrs would handle local archaeology while Yale would focus on national and international projects. By this time Ben had already moved on, and was more focused on his interests in world archaeology. However, in 1984 he collaborated with Lucianne Lavin to rehabilitate the Peabody Museum's aging exhibits on Native Americans with a special focus on the archaeology of Connecticut.

DEFINING MOMENT: HANDBOOK OF SOUTH AMERICAN INDIANS

A defining moment in Ben's career was his participation in the *Handbook of South American Indians* in the mid 1940s.

He contributed chapters on the (Island) Arawak (now Taínos) and (Island) Carib. These chapters drew heavily on the accounts of European chroniclers, and served as the main source of information about these cultures for years (1948).

Julian Steward edited this seven-volume compendium and introduced the concept of sociocultural levels of integration to organize the volumes. Steward classified South American Indians into Marginal Tribes, Tropical Forest Cultures, Circum-Caribbean Chiefdoms, and Andean States; a slight variation on the more general classification of cultures into bands, tribes, chiefdoms, and states. The Island Arawaks were grouped with the Circum-Caribbean Chiefdoms, which Steward proposed were derived from the expansion of complex societies from the Andes along the Caribbean littoral and out into the islands. Rouse disagreed. He proposed instead that the native peoples of the Caribbean had originated in lowland South America along the banks of the Amazon and Orinoco rivers. After migrating downriver to the northeastern coast of Venezuela and the Guianas (Orinoco Delta) they then migrated into the Caribbean islands (1953). Ben believed that the Caribbean was colonized by four discrete migrations. These occurred during the Lithic, Archaic, Ceramic, and Historic Ages. After every migration the borders were hermetically sealed such that new migrations were not accepted.

Other archaeologists in the region viewed every new pottery series as reflecting a separate migration of peoples from South America. Rouse has remained adamant that there was a single Ceramic Age migration called Saladoid that was followed by the local development (in Puerto Rico) of a new series called Ostionoid. To emphasize this point he adopted the concept of subseries that was first proposed by Gary Vescelius, the territorial archaeologist for the U.S.

Virgin Islands. Ben regrouped his earlier series into subseries leaving only the initial Saladoid and subsequent Ostionoid as full series. This modification eliminated the possibility of multiple migrations, and cut off discussions of outside, circum-Caribbean influences in the region. As he noted, "My efforts have been largely devoted to trying to counteract the assumption that everything had to come in from the outside" (Siegel, 1996, p. 682). He did accept that there were outside influences, but he maintained a belief in the uniqueness of Caribbean cultures.

FINAL THOUGHTS

I first met Ben at the Second Bahamas Conference on Archaeology in 1978, but recall an interesting exchange during the Third Bahamas Conference held on San Salvador, Bahamas, in 1982. John Winter presented a paper on a study using neutron activation to characterize pottery from Cuba and the Bahamas. He concluded that similarities in their signatures indicated that Bahamian pottery (Palmetto ware) must have developed from a Cuban tradition. In the discussion that followed I argued that without any dates it was impossible to identify a Cuban source for Bahamian pottery (i.e., when did the spread of pottery from Cuba occur?). During the break, Ben came up to me and said, "You don't think much of pottery analysis, do you?" How could I, a new M.A., respond to this great figure in Caribbean archaeology who at that time had spent almost 50 years studying pottery in the Caribbean? I said, "It is not the study of pottery I object to, it is the use of incomplete evidence to justify this particular conclusion."

Over the years I and many others visited Ben's lab at Yale on numerous occasions. He was always a gracious host, and incredibly generous with his time and resources. He had the most incredible collection of articles and papers on Caribbean archaeology, and an encyclopedic knowledge of who wrote what and when. When I last saw him I was studying Ostionan pottery, and he offered me his cards describing modes for this subseries in Puerto Rico (in the days before computers Ben would list different modes for a style or series on separate 3×5 cards). Foolishly I did not accept his offer, but hopefully the cards are still on file at Yale.

Ben Rouse is rightfully recognized as the *doyen* of Caribbean archaeology. But it would be wrong to view him simply as a Caribbeanist. His contributions and influence extend far beyond this region. He was a pioneer in what later would be called the Classificatory-Historical Period in American archaeology. His contributions to classification as a tool in archaeology are recognized widely. Furthermore, he had a great interest in world archaeology, a subject he taught at Yale beginning in the 1960s. Ben distinguished between archaeologists (methodological technicians) and prehistorians (those who wrote the past). He was always a prehistorian, and took a broad and synthetic view of the peoples and cultures that lived in the past. He has left a lasting imprint on the Caribbean region in particular and American archaeology in general.

CHRONOLOGY

1913	Born August 29 in Rochester, New York
1930-1934	Attended Sheffield Scientific School, Yale University as an undergraduate
1934-38	Attended graduate school, Yale University
1934-1938	Secretary-treasurer, Archaeological Society of Connecticut
1935	Fieldwork in Haiti
1936-1938	Fieldwork in Puerto Rico
1938-1950	Editor, Bulletin of the Archaeological Society of Connecticut
1939	June 24, married Mary Mikami
1939	Carnegie Foundation grant to study in European museums

1941	Fieldwork in Cuba
1944	Fieldwork in Florida
1944-1947	Coeditor, Yale University Publications in Anthropology
1946-1950	President, Eastern States Archaeological Federation
1946-1950	Editor, American Antiquity
1946-1957	Fieldwork in Venezuela
1946, 1953	Fieldwork in Trinidad
1948-1960	Member, Executive Board, Florida Anthropological Society
1950-1953	Member, Executive Board, American Anthropological Association
1952-1953	President, Society for American Archaeology
1950-1963	Editor, Yale University Publications in Anthropology
1957-1958	Vice president, American Ethnological Society
1958-1961	Delegate of the American Anthropological Association, National Research Council
1960-1962	Associate editor, American Anthropologist
1963-1964	Guggenheim fellow, Institute of Archaeology,University of London
1967-1968	President, American Anthropological Association
1968-1969	Acting editor, Yale University Publications in Anthropology
1973	Fieldwork in Antigua
1973-1985	Assistant editor for Caribbean archaeology, Handbook of Latin American Studies, U.S. Library of Congress
1977	Oppenheimer visiting associate, University of CapeTown
1977-1979	President, Association for Field Archaeology
1984	Retired from Yale University
2006	Died February 4 in New Haven, Connecticut

AWARDS AND HONORS

1948	A. Cressy Morrison Prize for the monograph *Porto Rican Prehistory*
1951	Elected to the New York Academy of Sciences
1954	Medalla Commemorativa del Vuelo Panamericano pro Faro a Colón, awarded by the Cuban government
1960	Viking Fund Medal and Award in Anthropology
1962	Elected to the National Academy of Sciences
1963-1964	Guggenheim fellow
1984	Distinguished Service Award, American Anthropological Association
1985	Fiftieth Anniversary Award, Society for American Archaeology
1995	Distinguished Service Award, International Association for Caribbean Archaeology

PROFESSIONAL RECORD

1934	B.S., Yale University
1938	Ph.D., Yale University
1938	Named assistant curator of anthropology, Peabody Museum of Natural History, Yale University
1939	Named instructor in the Department of Anthropology, Yale University
1943	Promoted to assistant professor, Department of Anthropology, Yale University
1947	Promoted to associate curator, Peabody Museum of Natural History, Yale University
1948	Promoted to associate professor, Department of Anthropology, Yale University
1954	Promoted to full professor, Department of Anthropology, Yale University
1954	Named research associate, Peabody Museum of Natural History, Yale University
1957-63	Chair, Department of Anthropology, Yale University
1970	Named Charles J. MacCurdy Professor of Anthropology, Department of Anthropology, Yale University
1975	Named research affiliate, Peabody Museum of Natural History, Yale University
1977	Named curator of anthropology, Peabody Museum of Natural History, Yale University

1984 Named Professor Emeritus, Department of Anthropology, Yale University
1984 Named Curator Emeritus, Peabody Museum of Natural History, Yale University

MEMBERSHIPS

American Academy of Arts and Sciences
American Anthropological Association
American Council of Learned Societies
American Ethnological Society
Archaeological Society of Connecticut
Association for Field Archaeology
Connecticut Academy of Science and Engineering
Eastern States Archaeological Federation
Florida Anthropological Society
Gesell Institute of Child Development
Society for American Archaeology
Society of Professional Archaeologists

REFERENCES

Allaire, L. 1977. *Later Prehistory in Martinique and the Island Carib: Problems in Ethnic Identity*. Ph.D. dissertation, Yale University. University Microfilms, Ann Arbor, Mich.

Bullbrook, J. A. 1953. *On the Excavation of a Shell Mound at Palo Seco, Trinidad, B.W.I.* New Haven: Yale University Publications in Anthropology No. 50.

Congressional Record. Senate–October 1, 1999. In memorium–Mary Mikami Rouse, pp. S11791-S11792. Washington, D.C.: U.S. Government Printing Office.

Davis, D. D. 2000. *Jolly Beach and the Preceramic Occupation of Antigua, West Indies*. New Haven: Yale University Publications in Anthropology No. 84.

Drew, R. 2006. The influence of Irving Benjamin Rouse [sic]: A conversation. *Kacike* Feb. 20. Available at www.kacike.org/Rouse.html.

Gower, C. D. 1927. *The Northern and Southern Affiliations of Antillean Culture*. Memoirs of the American Anthorpological Association 35. Menasha, WI: American Anthropological Association.

Hahn, P. G. 1961. *A Relative Chronology of the Cuban Nonceramic Tradition.* Ph.D. dissertation, Yale University. University Microfilms, Ann Arbor, Mich.

Howard, R. R. 1950. *The Archaeology of Jamaica and Its Position in Relation to Circum-Caribbean Culture.* Ph.D. dissertation, Yale University. University Microfilms, Ann Arbor, Mich.

Keegan, W. F. 1992. *The People Who Discovered Columbus.* Gainesville: University Press of Florida.

McKusick, M. B. 1960. *The Distribution of Ceramic Styles in the Lesser Antilles, West Indies.* Ph.D. dissertation, Yale University. University Microfilms, Ann Arbor, Mich.

Olsen, F. 1974a. *On the Trail of the Arawaks.* Norman: University of Oklahoma Press.

Olsen, F. 1974b. *Indian Creek: Arawak Site on Antigua, West Indies.* Norman: University of Oklahoma Press.

Osgood, Cornelius. 1942. *The Ciboney Culture of Cayo Redondo, Cuba.* Yale University Publications in Anthropology No. 25. New Haven: Yale University Press.

Rainey, F. G. 1940. Porto Rican archaeology. In *Scientific Survey of Porto Rico and the Virgin Islands.* New York Academy of Sciences, vol. 18, part 1.

Rainey, F. G. 1992. *Reflections of a Digger: Fifty Years of Archaeology.* Philadelphia: University of Pennsylvania Press.

Rouse, B. I. 1938. *Contributions to the Prehistory of the Ft. Liberté Region of Haiti.* Ph.D. dissertation. Yale University.

Siegel, P. E. 1996. An interview with Irving Rouse. *Curr. Anthropol.* 37:671-689. [Note: Peter Siegel, who is now an associate professor at Montclair State University, conducted a remarkable and very comprehensive interview with Ben. It provides a detailed accounting of Ben's career in his own words.]

Sullivan, S. D. 1981. *Prehistoric Patterns of Exploitation and Colonization in the Turks and Caicos Islands.* Ph.D. dissertation. University of Illinois at Urbana-Champaign.

Willey, G., and J. Sabloff. 1974. *A History of American Archaeology.* San Francisco: Freeman and Sons.

SELECTED BIBLIOGRAPHY

1939

Prehistory in Haiti: A Study in Method. Publications in Anthropology No. 21. New Haven: Yale University.

1941

Culture of the Ft. Liberté Region, Haiti. Publications in Anthropology Nos. 23 and 24. New Haven: Yale University.

1942

Archaeology of the Mariabon Hills, Cuba. Yale University Publications in Anthropology No. 26. New Haven: Yale University Press.

1948

The West Indies: An introduction to the Ciboney. In *Handbook of South American Indians. The Circum-Caribbean Tribes*, vol. 4, ed. J. H. Steward, pp. 497-503. Washington, D.C.: Bureau of American Ethnology Bulletin 143.

The Arawak. In *Handbook of South American Indians. The Circum-Caribbean Tribes*, vol. 4, ed. J. H. Steward, pp. 507-546. Washington, D.C.: Bureau of American Ethnology Bulletin 143.

The Carib. In *Handbook of South American Indians. The Circum-Caribbean Tribes*, vol. 4, ed. J. H. Steward, pp. 547-565. Washington, D.C.: Bureau of American Ethnology Bulletin 143.

1949

Petroglyphs. In *Handbook of South American Indians. The Circum-Caribbean Tribes*, vol. 5, ed. J. H. Steward, pp. 493-502. Washington, D.C.: Bureau of American Ethnology Bulletin 143.

1951

Areas and periods of culture in the Greater Antilles. *Southwest. J. Anthropol.* 7(3):248-265.

Prehistoric Caribbean culture contact as seen from Venezuela. *Trans. N. Y. Acad. Sci.* 13(8):342-347.

1952

Porto Rican prehistory. Introduction: Excavations in the west and north. In *Scientific Survey of Porto Rico and the Virgin Islands.* New York Academy of Sciences 18(4):307-460.

Porto Rican Prehistory: Excavations in the interior, south, and east: Chronological implications. *Scientific Survey of Porto Rico and the Virgin Islands.* New York Academy of Sciences 18(4):463-578.

1953

The circum-Caribbean theory: An archaeological test. *Am. Anthropol.* 55(2):188-200.

1956

Settlement patterns in the Caribbean area. In *Prehistoric Settlement Patterns in the New World*, Viking Fund Publications in Anthropology, vol. 23, ed. G. Willey, pp. 165-172. New York: Wenner-Gren Foundation for Anthropological Research.

1958

Archaeological similarities between the southeast and the West Indies. In *Florida Anthropology*, Publication No. 2, ed. C. Fairbanks, pp. 3-14. Tallahassee: Florida Anthropological Society.

1960

The Entry of Man into the West Indies. Yale University Publications in Anthropology No. 61. New Haven: Yale University Press.

1961

Archaeology in lowland South America and the Caribbean, 1935-1960. *Am. Antiquity* 27(1):56-62.

The Bailey collection of stone artifacts from Puerto Rico. In *Essays in Pre-Columbian Art and Archaeology*, ed. S. K. Lothrop, pp. 342-355. Cambridge: Harvard University Press.

1962

The intermediate area, Amazonia, and the Caribbean area. In *Courses Toward Urban Life: Archaeological Considerations of Some Cultural Alternatives*, eds. R. J. Braidwood and G. R. Willey, pp. 34-59. Publications in Anthropology No. 32. New York: Viking Fund.

1964

Prehistory of the West Indies. *Science* 144:499-513.

The Caribbean area. In *Prehistoric Man in the New World*, eds. J. D. Jennings and E. Norbeck, pp. 389-417. Chicago: University of Chicago Press.

1966

Mesoamerica and the eastern Caribbean area. In *Handbook of Middle American Indians*, vol. 4, eds. G. E. Ekholm and G. R. Willey, pp. 234-242. Austin: University of Texas Press.

Caribbean ceramics: A study in method and theory. In *Ceramics and Man*, Viking Fund Publications in Anthropology, vol. 41, ed. F. R. Matson, pp. 88-103. New York: Wenner-Gren Foundation for Anthropological Research.

1969

With J. Cruxent. Early man in the West Indies. *Sci. Am.* 221(5):42-52.

1972

Introduction to Prehistory: A Systematic Approach. New York: McGraw-Hill.

1977

Patterns and process in West Indian archaeology. *World Archaeol.* 9(1):1-11.

1978

With L. Allaire. Caribbean. In *Chronologies in New World Archaeology*, eds. R. E. Taylor and C. Meighan, pp. 431-481. New York: Academic Press.

1980

The concept of series in Bahamian archaeology. *Fla. Anthropol.* 33(3):94-98.

1982

Ceramic and religious development in the Greater Antilles. *J. New World Archaeol.* 5(2):45-55.

1985

With C. Moore. Cultural sequence in southwestern Haiti. *Proceedings of the Tenth International Congress for Caribbean Archaeology*, eds.: Louis Allare and Francine-M. Meyer, pp. 3-21. Montréal: Université de Montréal.

1986

Migrations in Prehistory: Inferring Population Movements from Cultural Remains. New Haven: Yale University Press.

1989

Peopling and repeopling of the West Indies. In *Biogeography of the West Indies, Past, Present and Future*, ed. C. Woods, pp. 119-135. Gainesville, Fla.: Sandhill Crane Press.

Peoples and cultures of the Saladoid frontier in the Greater Antilles. In *Early Ceramic Population Lifeways and Adaptive Strategies in the Caribbean*. BAR International Series No. 506, ed. P. E. Siegel, pp. 383-404. Oxford: BAR.

1990

With R. E. Alegría. *1990 Excavations at Maria de la Cruz Cave and Hacienda Grande Village Site, Loiza, Puerto Rico*. Yale University Publications in Anthropology No. 80. New Haven: Yale University Press.

1992

The Tainos: Rise and Decline of the People Who Greeted Columbus. New Haven: Yale University Press.

1996

History of archaeology in the Caribbean area. In *The History of Archaeology: An Encyclopedia*, ed. T. Murray. New York: Garland Publishing.

1999

With B. Faber Morse. *Excavations at the Indian Creek Site, Antigua, West Indies.* Yale University Publications in Anthropology No. 82. New Haven: Yale University Press.

Photograph courtesy of Clarice Schwinger.

Julian Schwinger

JULIAN SCHWINGER

February 12, 1918–July 16, 1994

BY PAUL C. MARTIN AND SHELDON L. GLASHOW

JULIAN SCHWINGER, WHO DIED ON July 16, 1994, at the age of 76, was a phenomenal theoretical physicist. Gentle but steadfastly independent, quiet but dramatically eloquent, self-taught and self-propelled, brilliant and prolific, Schwinger remained active and productive until his death. His ideas, discoveries, and techniques pervade all areas of physics.

Schwinger burst upon the scene meteorically in the late 1930s, and by the mid-20th century his reputation among physicists matched those of earlier giants. To a public vaguely conscious of relativity and quantum uncertainty but keenly aware of nuclear energy, the New York Times reported in 1948 that theorists regarded him as the heir apparent to Einstein's mantle and his work on the interaction of energy and matter as the most important development in the last 20 years. With the development of powerful new theoretical methods for describing physical problems, his influence grew. In the early 1950s the *Journal of Jocular Physics*, a publication of the Bohr Institute for Theoretical Physics in Copenhagen, included a template for articles by aspiring theorists. It began "According to Julian Schwinger" and invoked "the Green's function expression for …". References to unpublished Schwinger lecture notes and some classic Schwinger papers followed. The recipe elicited smiles, but it accurately portrayed his preeminence at that time. With this preeminence came stratospheric expectations, which he continually strove to fulfill.

Schwinger was born in upper Manhattan on February 12, 1918. He went to P.S. 186, to Townsend Harris High School (then New York City's leading public high school), and to the College of the City of New York, following brother Harold by six years. Harold was the outstanding student, the valedictorian, their mother would explain. Julian took the establishment of teachers, textbooks, and assignments less seriously. From some, most notably physics teacher Irving Lowen, he benefited greatly. But there were better things to do with the 11th edition of the *Encyclopaedia Britannica* and the books and journals in nearby libraries.

In 1926 when Werner Heisenberg and Paul Dirac were developing quantum mechanics, Schwinger was in the third grade. Eight years later, before completing high school, he had assimilated these ideas and in an unpublished paper extended Dirac's ideas to many-electron systems. By then, word of the wunderkind had spread among graduate students at City College, where he enrolled in the fall of 1934 and at Columbia University, to which—thanks to that institution's support and the subsequent intervention of I. I. Rabi—he was able to transfer in 1936.

In a remarkable letter dated July 10, 1935, from Hans Bethe to I. I. Rabi, Bethe describes his meeting with Schwinger:

> I entirely forgot that he [Schwinger] was a sophomore 17 years of age. . . His knowledge of quantum electrodynamics is certainly equal to my own, and I can hardly understand how he could acquire that knowledge in less than two years and almost all by himself." Bethe concludes that "Schwinger will develop into one of the world's foremost theoretical physicists if properly guided, i.e., if his curriculum is largely left to his own free choice.

Less than four years after he entered college Schwinger had completed both the requirements for his undergraduate and graduate degrees and the research for his doctoral thesis. During his sophomore year, with Otto Halpern, he predicted the polarization of electrons by double scattering and with Lloyd Metz he computed the lifetime of the neutron. On his own as a junior he computed how neutrons were polarized by double scattering from atomic electrons. That the electron current must be treated relativistically by the Dirac equation (that is, that the classical approximations made by Felix Bloch were inadequate) was noted *sotto voce.* Next, he calculated the influence of a rotating magnetic field on a spin of any magnitude j. His analysis for $j = 1/2$ remains the prototype for all discussions of transitions in two-level systems by "Rabi flipping."

During the spring of 1937, he and Edward Teller studied coherent neutron scattering by hydrogen molecules, showing how the spin-dependent, zero-energy, neutron-proton-scattering amplitudes could be determined from the experimental data. This topic was the theme of his doctoral thesis.

In the fall of 1937, with his undergraduate degree in hand, eight significant papers published, and his doctoral thesis virtually complete, Schwinger left New York, planning to spend the fall term at the University of Wisconsin with Gregory Breit and Eugene Wigner, and the spring term at the University of California, Berkeley, with J. Robert Oppenheimer. In Madison he took such great pleasure in working at night on problems of his own choosing that he stayed for the entire year. He would maintain this nocturnal regimen for most of his career.

Schwinger returned to Columbia for 1938-1939. As house theorist he worked with Hyman Henry Goldsmith, John Manley, Victor Cohen, and Morton Hammermesh on nuclear-energy-level widths and on the neutron-proton interaction and with Rabi and his associates on molecular beams. His doctoral degree under Rabi's supervision was awarded in 1939.

Schwinger spent the next two years at Berkeley working with Oppenheimer, students, and visitors (Herbert Corbett, Edward Gerjuoy, Herbert Nye, and William Rarita). With Rarita he determined definitively the effects of the tensor force on the deuteron's magnetic and quadrupole moments. He also examined the consequences of tensor and exchange forces between pairs of nucleons on the magnetic and quadrupole moments of light nuclei, nuclear pair emission, deuteron photodisintegration, and other phenomena.

The Rarita-Schwinger equation—one of the few of his many contributions that bear his name—was all but forgotten for many years. But this generalization of the Dirac equation to particles with spin 3/2, and the study of its invariances when the particles are massless, has been recalled by theorists who postulate a gravitino, a spin-3/2 fermion supersymmetric partner of the graviton.

Notwithstanding a ticker tape parade for Albert Einstein, theoretical physics held little fascination for the American public or major American universities prior to the Second World War. Even so, in 1941 the nation's great universities might have been expected to compete fiercely for an acknowledged young genius who lectured along with Wolfgang Pauli, Frederick Seitz, and Victor Weisskopf at the world-famous Michigan summer school for physics. They did not. In some cases, a long tradition of anti-Semitism may have been a factor. Schwinger was offered and accepted a lowly instructorship at Purdue University with just one concession to his preferred work schedule: His introductory physics section would start at noon.

Led by first-rank physicist Karl Lark-Horovitz, Purdue attracted able graduate students and postdoctoral fellows. Among them was Robert Sachs, who (as related by Sylvan Schweber in his book on QED) recalled that in February 1942, "We had to spend the whole time trying to cheer Julian up" at his 24th birthday party "because he had not yet made the great discovery expected of him."

Along with physicists at Cornell University and the University of Rochester and with colleagues at Purdue, Schwinger spent the first year and a half of World War II working on the properties of microwave cavities. The work was coordinated with and supported by MIT Radiation Laboratory research projects.

Invited by Oppenheimer to join the Manhattan Project, Schwinger spent the summer of 1943 at the University of Chicago's Metallurgical Laboratory, where John Wheeler, Eugene Wigner, and other scientists were designing the first Hanford reactor. As in Madison, Schwinger worked nights, and so Bernard Feld (who had worked with him at Columbia) decided to work an intermediate afternoon-evening shift so that he might help link Schwinger with those working normal hours.

After "a brief sojourn to see if I wanted to help develop the Bomb—I didn't," recalled Schwinger, "I spent the war years helping to develop microwave radar." Reluctance to follow others' agendas once again helped determine his course. Thus, in the fall of 1943 after most luminaries with nuclear expertise had left the MIT Rad Lab for Los Alamos, Schwinger arrived in Cambridge with little notion that he would remain in the area for more than a quarter century.

Many of Schwinger's colleagues during his three-year stint at the Rad Lab became his lifelong friends. Among them were Harold Levine from Cornell; Nathan Marcuvitz, an electrical engineer from Brooklyn College; and David

Saxon, an MIT graduate student. Schwinger's collaboration with Levine led to a series of papers that creatively used variational methods and Green's functions—two approaches central to so much of Schwinger's work—to obtain important new results on radiation and diffraction.

Schwinger and Marcuvitz appreciated the value of integral equation formulations of waveguide theory that incorporate the boundary conditions accompanying partial differential equation formulations and can be cast in the engineering language of transmission lines and networks. The isolation of complex internal properties of components and the characterization of these components through a small set of parameters provided valuable insights—insights that would later prove valuable in characterizing nuclear phenomena via effective range theory, scattering matrices, and new formal approaches to complex scattering processes.

At the Rad Lab Schwinger gave a series of lectures on microwave propagation for which David Saxon served as his Boswell. Many of the ideas and techniques in them recur in his later theoretical work on quantum mechanics, electrodynamics, nuclear physics, and statistical mechanics. A small volume, titled *Discontinuities in Waveguides*, containing some of these lectures, was published decades later. In the volume's introduction and 138 pages of text, Schwinger himself observed that the name "Green" or simply "G" (for Green's function) appeared more than 200 times. Some powerful relations imposed on scattering amplitudes by time reversibility and energy conservation can also be traced back to Schwinger's work at the time.

When the War ended, Schwinger's attention turned to the physics of high-energy accelerators and to the obstacles to producing them. It struck him that the energy loss of a highly relativistic electron accelerating in a circular orbit could be simply and straightforwardly deduced from the covariant expression for radiation damping, making the fourth power law for the radiated energy transparent. "Manifest covariance" would play an important role in Schwinger's work on quantum electrodynamics. During this period, Schwinger also designed a novel accelerator, later named the *minotron*.

In addition to work on other aspects of synchrotron radiations, notepads in his desk drawers at that time included studies of neutron scattering in a Coulomb field, and a group-theory-free approach to the properties of angular momentum that expresses angular momentum operators in terms of oscillator creation and annihilation operators. *On Angular Momentum*, a set of his notes that makes exhaustive use of this approach, circulated widely for 15 years prior to its publication in 1965.

Schwinger's long and diverse bibliography, with more than 200 publications, contains no publications over the period 1942 through 1946. However, the war produced sweeping changes in the social and intellectual values and mores of the public and the nation's premier universities. Thus, in February 1946, the month Schwinger turned 28, he was offered and accepted a tenured position at Harvard. Professorship offers from Columbia and Berkeley soon followed, but he turned them down.

Students attending topflight universities were also different before and after the war. Postwar students included mature veterans whose studies had been interrupted by the war and bright youth from a broader cross-section of the nation's preparatory schools. Doors were open, for example, to outstanding students from New York's select high schools (for example, Bronx Science, Brooklyn Tech, and Stuyvesant, the successors to Schwinger's alma mater, Townsend Harris).

Schwinger's first year at Harvard, 1946-1947, was a busy one. He offered courses on waveguides and theoretical nuclear physics, and accepted a number of graduate students whom he set to work on a wide range of problems. Among these early students were Bernard Lippmann who investigated integral equation formulations of scattering theory (Lippmann-Schwinger equations); Walter Kohn, who studied variational principles for scattering; Ben Mottelson, who worked on the properties of light nuclei; Bryce DeWitt, who explored gravitation and the interaction of gravitation with light; and Roy Glauber, who examined meson-nucleon interactions and mesonic decay. He and longtime friend Herman Feshbach pursued their studies of the internucleon potential.

When the academic year ended, Schwinger and 22 other physicists headed off to the Shelter Island conference on the foundations of quantum physics, where the electrodynamic origin of the spectral lineshift measured by Willis Lamb and Robert Retherford was discussed. Legend has it that Weisskopf and Schwinger proposed that in the Dirac theory compensating effects of electrons and positrons could lead to a cancellation of divergences, and that Hans Bethe—on his way home from the conference—recognized that the bulk of the effect could be estimated nonrelativistically.

Four days after the conference ended, Schwinger married Clarice Carroll, whom he had been courting for several years and with whom he would share the next 47 years.

Schwinger's lectures, from his early days at Harvard on, have been likened to concerts at which a virtuoso performs pieces brilliantly. Each lecture was an event. Speaking eloquently, without notes, and writing deftly with both hands, Schwinger would weave original examples and profound insights into beautiful patterns. Audiences would listen reverently seeking to discern the unheralded difficult cadenzas. As at a concert, interruptions to the flow were out of place.

Schwinger's masterly performances were not limited to the Harvard community. His audiences quickly grew to include faculty and students from throughout the Boston area. Notes taken by John Blatt, an MIT instructor, were shipped to a team of Princeton graduate students, who in swift relays copied them onto duplicator masters for reproduction. Underground notes in multiple handwritings, with some pages containing picturesque mistranscriptions (such as "military matrices" for "unitary matrices") spread quickly throughout the country and overseas.

Schwinger was never satisfied with his expositions. Each time he offered a course he carefully reworked and honed his ideas, methods, and examples, presenting them in a new way, a way that differed from his earlier versions circulating in others' articles and lecture notes, often without attribution. Significant portions of many classic texts on nuclear physics, atomic physics, optics, electromagnetism, statistical physics, quantum mechanics, and quantum field theory can be traced to one or another version of his lectures.

As noted, a few isolated gems—his work on microwaves and his notes on angular momentum—were eventually published. He was also stimulated in 1964 "to rescue from the quiet death of lecture notes" a beautiful discussion of Coulomb Green's functions "worked out to present to a quantum mechanics course given in the late 1940s." The bound-state momentum space wave functions are deftly and concisely constructed as four-dimensional spherical harmonics.

Notes for his early quantum mechanics courses also include elegant and revealing unpublished treatments of Coulomb scattering and of the unusual way that the Stark effect lifts hydrogenic degeneracies. These and other jewels may be found in the archives assembled by UCLA of lecture notes, chapters, and preliminary editions of books on quantum mechanics, field theory, and electromagnetism that failed to meet his exacting standards. A few appear in *Classical Electrodynamics*, published in 1998.

Not until September 1947 did Schwinger begin to work on the electrodynamic effects responsible for deviations of experimental observations from values predicted by the Dirac equation. Hyperfine structure measurements of hydrogen, deuterium, and tritium by John Nafe, Edward Nelson, and Rabi indicated a 0.12 percent error in the electron's magnetic moment, and measurements by Lamb and Retherford displayed a splitting of about 1050 megacycles between states of the hydrogen atom with degenerate Dirac energies. "By the end of November I had the results," Schwinger later recalled. He described them to a capacity audience at an American Physical Society meeting at Columbia University on a Saturday morning in January 1948, giving a command repeat performance to an overflow audience that afternoon. He discussed his calculations in fuller detail at the Pocono conference in the spring and in lectures at the University of Michigan summer school. Demonstrating his computational virtuosity, he published his reformulation of quantum electrodynamics in three long papers in *Physical Review,* Quantum Electrodynamics I (1948), II (1949), and III (1949). They include several of the results for which he, Richard Feynman, and Sin-Itiro Tomanaga were eventually awarded the 1965 Nobel Prize in Physics. To those who admire the eloquence of Schwinger's expositions, it seems ironic that these three uncharacteristically opaque papers should have helped secure his place in Nobel history.

In light of his many spectacular achievements, including his fundamental contributions to quantum electrodynamics, Schwinger was elected to the National Academy of Sciences at the exceptionally young age of 31.

By 1950 Schwinger recognized the need for a more systematic approach to quantum field theory utilizing a covariant quantum version of Hamilton's principle. In 1951 in a pair of brief papers in the Proceedings of the National Academy of Sciences, the techniques and concepts on which field theorists all rely made their appearance. Using "sources" as fundamental variables, Schwinger provided the functional differential equation version of what in integral form is now called functional integration. Of lasting importance, much of this material has been rediscovered by others. For theoretical students at Harvard at the time, Schwinger's techniques provided an Aladdin's lamp for parsing, analyzing, and solving problems. As a matter of principle, these papers noted,

> The temporal development of quantized fields is described by propagation functions, or Green's functions. The construction of these functions for coupled fields is usually considered from the viewpoint of perturbation theory. Although the latter may be resorted to for detailed calculations, the formal theory of Green's functions should not be based on the assumption of expandability in powers of the coupling constant.

After relating the outgoing wave boundary condition to the vacuum, the second paper defined functions (such as self-energies and effective interactions) that characterize exactly (that is, not as power series in the coupling constant) the propagation and interaction of quantum fields. This approach opened the way for major conceptual and computational advances in quantum electrodynamics. A series of papers called "Theory of Quantized Fields" followed.

Word appears to have circulated that the stress Schwinger placed on the properties of fields that transcended perturbation theory, and his personal dislike of diagrams disadvantaged those working for and with him in the 1950s. Hardly! His students and postdoctoral fellows were fully conversant and facile with the diagrammatic approaches of Feynman and Freeman Dyson and analytic approaches. With Schwinger's tools, they generated directly and succinctly the connected diagrams involving dressed propagators that describe various processes. With them they evaluated a large share of the quantum electrodynamic corrections to hydrogen and positronium bound states and a large share of the higher order corrections (for example, to the electron's magnetic moment) computed at that time.

Other aspects of Schwinger's routine can also mistakenly be cast in an unkindly light. It is true, for example, that students might wait a long time to see him during his lengthy office hours. He could have spent less time with each and he could have accepted fewer. In his first year at Harvard he accepted 10 graduate students, and in subsequent years no one recalls his ever turning down a prospective student whom the department certified as qualified. When requested, Schwinger posed problems to students, sometimes offering them and colleagues his notes. At the same time, he welcomed students who preferred to formulate their own thesis topics. If students told him they were stuck, he would offer suggestions and proposals on the spot and at subsequent meetings. Rare are the students who did not cherish their interactions with Schwinger in sessions that were often lengthy.

His late arrival for classes was not because he left gathering materials for his lecture to the last minute. Not only in the early years but also throughout his long career he insisted on remaining home the night before each lecture, staying up late to prepare exactly what he would say and how best to say it.

Among the giant figures in theoretical physics, his level of commitment to course lectures and to the supervision of large numbers of research students may be unmatched.

Schwinger's investigations of quantum field theory continued through the 1950s. Relativistic invariance and gauge invariance constrain the formally divergent expressions appearing in quantum electrodynamics calculations. Colleagues of Pauli, ignoring the consequences of gauge invariance, had recast and manipulated these expressions to predict a finite photon mass. Schwinger's 1951 paper on vacuum polarization and gauge invariance addressed some of these issues with a novel and elegant proper-time formalism. The nonperturbative properties of a Dirac field coupled to a prescribed external electromagnetic field, first derived in this paper, are still widely used and admired. Schwinger saw that many ambiguities associated with interacting quantum fields lay in the treatment of formal expressions for composite operators such as currents. Indeed, the "triangle anomalies" that play a major role in modern (post-1969) field theory were first identified here and studied further by Schwinger and Ken Johnson during the 1950s. Further studies of quantized fields led in 1958 to Schwinger's important series of papers on "Spin, Statistics, and the TCP Theorem."

During the 1950s, puzzles posed by elementary particle physics preoccupied Schwinger. What role could strange particles, whose properties were just being elucidated, play in the grand scheme of things? He was convinced that the answer had to do with their transformation properties under a generalization of isotopic-spin symmetry, which he took to be the four-dimensional rotation group. The group generators, under commutation, defined what would later become known as the "algebra of charges."

Schwinger gathered particle species together, both strange and nonstrange, into representations of his proposed group. In this manner the otherwise mysterious Gell-Mann-Nishijima formula—which relates charge, hypercharge, and isospin—had a natural explanation. It later turned out that Schwinger's intuition was correct, although his choice for the relevant transformation was not.

The approximate symmetries of mesons and baryons were not shared by the leptons. For these particles, Schwinger proposed a direct analog to isospin. Just such a group was later to become an integral part of today's successful electroweak theory. The known leptons—in Schwinger's perversely original interpretation—were to form a weak isospin triplet: $\{\mu^+, \nu, e^-\}$. An immediate consequence of this notion was the selection rule forbidding $\mu \rightarrow e + \gamma$ and the obligatory distinction between neutrinos associated with electrons and muons. "Is there a family of bosons that realizes the T=1 symmetry of [the lepton symmetry group]?" Schwinger asked. If so, the charged counterparts of the photon could mediate the weak interactions. Both the vectorial nature of the weak force and its apparent universality would arise as simple consequences of the underlying symmetry structure. He also suggested that vacuum expectation values of scalar fields could provide a way of breaking symmetries and giving fermions their masses.

Schwinger's 1957 paper on particle symmetries appeared at a time of rapid progress and great confusion, between the discoveries of parity violation and the V-A nature of the weak interactions. His ambitious paper concluded with the modest suggestion that "it can be of value if it provides a convenient frame of reference in seeking a more coherent account of natural phenomena." For some of the theorists who developed that coherent theory over the next 15 years, it did just that. Schwinger himself, however, turned to other problems.

A 1959 paper with Martin extended Schwinger's nonperturbative field theoretic concepts and methods for the vacuum state to material systems in equilibrium at nonvanishing densities and temperatures, and a 1961 paper, camouflaged by the title "Brownian Motion of a Quantum Oscillator" paved the way for the study of systems far from thermal equilibrium. Extended by K. T. Mahantappa, Pradip Bakshi, and Victor Korenman at Harvard, and rediscovered (independently) by Leonid Keldysh, Schwinger's "two-time" approach is now widely used in studies of cosmology, quark-gluon plasmas, and microelectronic devices.

As indicated above, Schwinger recognized in the early 1950s that the composite operators for observables must be treated with care. Naive manipulations with canonical commutation relations suggest that the space and time components of a current commute with each other. In 1959 Schwinger published an argument, dazzling in its simplicity, that moved this problem to the fore and identified a class of anomalies, now called "Schwinger terms." He followed it in papers directed toward the gravitational field with a study of the conditions imposed by consistency on stress tensor commutation relations. Today we recognize the key roles such terms play in particle physics and statistical mechanics.

In the late 1960s Schwinger directed much of his attention to his source theory. The motivation was clear. In spite of field theory's many triumphs, the prospects then seemed dim for predicting the results of experiments involving strongly interacting particles from a unified field theory. Prospects for a renormalizable theory of the electroweak interactions also seemed dim. Why not try to develop a theory that would progress in the same way as experiment—from lower to higher energies? Source theory provided a framework for pursuing this modest goal.

Soon thereafter these prospects brightened. Gauge field theories were shown to be renormalizable and consonant with an increasing number of phenomena. Quantum field theory, to which Schwinger had contributed so much, might describe all strong and electroweak phenomena. Schwinger demurred, remaining steadfastly committed to the source theory approach that he and his students were pursuing. The philosophical basis of divergence-free "anabatic" (going up) phenomenological source theory was, he maintained, immensely different from "the speculative approach of trickle-down" field theory. So too were its predictive powers. He espoused this contrarian position steadfastly.

During the 1960s, Schwinger's lifestyle expanded in other ways. He began playing tennis regularly, and he and Clarice spent time in distant places, including Paris and Tokyo. In 1971 the Schwingers left Harvard and their Belmont home for UCLA and the Bel Aire hills. In sunny southern California, with students, new collaborators, and longtime friends, Schwinger continued working on source theory ("source" appears in the title of more than 15 publications) and contributing significantly to a host of interesting physical problems not in vogue. With Lester DeRaad Jr. and Berthold-Georg Englert, he explored statistical models of the atom that extend the Fermi-Thomas approximation and, with Kimball Milton and DeRaad, various aspects of the Casimir effect. In his new surroundings he published more than 70 papers.

Reports of cold fusion whetted his contrarian appetite. The publicized experiments might be flawed, he would observe, but fundamental physical principles do not rigorously exclude the possibility that without tokamaks and high-temperature plasmas, somehow, in some way, in some material, the energy required for fusion might be coherently concentrated and transferred from atoms to nuclei.

One of Schwinger's last papers is a 1993 talk titled "The Greening of Quantum Field Theory: George and I, Lecture at Nottingham, July 14, 1993." It contains the count of references to Green in Discontinuities in Waveguides mentioned earlier and a recital of a multitude of the linkages with George Green of Schwinger's research on field and particle theory, statistical mechanics, through to work on the Casimir effect and sonoluminescence. Although Schwinger's genius was widely recognized immediately, and Green's very slowly. Schwinger concludes his talk by answering the question, "What then shall we say about George Green?" with "He is, in a manner of speaking, alive, well, and living among us." That, too, can be said for Schwinger.

Schwinger's legacy has also been greatly amplified by the 70 doctoral students and 20 postdoctoral fellows who worked with him. For their research they have innumerable major awards, including four Nobel prizes; nine of his students have been elected to the National Academy of Sciences.

Two features shared by Schwinger's professional offspring are striking: the diversity of their specialties and the consistently high regard and great debt they express for his mentorship. The group includes leaders in particle theory, nuclear physics, astrophysics, gravity, space physics, optics, atomic physics, condensed matter physics, electromagnetic phenomena, applied physics, mathematics, and biology. It also includes many who, like Schwinger, have worked in a variety of fields, mirroring Schwinger's own broad interests and his passion for seeking patterns and paradigms that put new facts in proper perspective.

Their recollections are remarkably uniform. While few former students considered him a close friend, almost all speak fondly of his kindness and generosity. He was considerate and willing to do his best to provide scientific advice when he thought help was needed. His insight and suggestions were often decisive.

By example he conveyed lofty aspirations: to approach every problem in a broad context, with as few assumptions as possible; to seek new and verifiable results and to present them as elegantly as possible; to avoid energy- and time-consuming political maneuvering; to understand, extend, unify, and generalize; and to reveal the hidden beauty of nature. Walter Kohn spoke for all of Schwinger's students in saying, We carried away the self-admonition to try and measure up to his high standards; to dig for the essential; to pay attention to the experimental facts; to try to say something precise and operationally meaningful, even if—as is usual—one cannot calculate everything a priori; not to be satisfied until ideas have been embedded in a coherent, logical and aesthetically satisfying structure.

Schwinger also had a remarkable knowledge of matters nonscientific and a gentle humor. While too reserved to savor media stardom, he enjoyed presenting relativity to a wide audience in a popular book and on BBC television. He was always willing to lend his name and support to worthy causes. Fond recollections of the hospitality, warmth, and interest displayed by both Julian and Clarice Schwinger abound.

An article about Julian Schwinger was published by the authors of this memoir in Physics Today, Oct. 1995, pp. 40-46, under the copyright of the American Institute of Physics. With AIP permission the authors have presented here a slightly modified version of that article.

SELECTED BIBLIOGRAPHY

1935

With O. Halpern. On the polarization of electrons by double scattering. *Phys. Rev.* 48:109.

1937

On the magnetic scattering of neutrons. *Phys. Rev.* 51:544-552.

On the non-adiabatic processes in inhomogeneous fields. *Phys. Rev.* 51:648-651.

With E. Teller. The scattering of neutrons by ortho and para hydrogen. *Phys. Rev.* 51:775.

On the spin of the neutron. *Phys. Rev.* 52:1250.

1941

With W. Rarita. On the neutron-proton interaction. *Phys. Rev.* 59:436-452.

With R. Rarita. On a theory of particles with half-integral spin. *Phys. Rev.* 60:61.

1946

Electron radiation in high energy accelerators. *Phys. Rev.* 70:798.

1947

A variational principle for scattering problems. *Phys. Rev.* 72:742.

1948

On quantum electrodynamics and the magnetic moment of the electron. *Phys. Rev.* 73:416-441.

Quantum electrodynamics. I. A covariant formulation. *Phys. Rev.* 74:1439-1461.

1949

Quantum electrodynamics. II. Vacuum polarization and self-energy. *Phys. Rev.* 75:651-679.

Quantum electrodynamics. III. The electrodynamic properties of the electron *Phys. Rev.* 76:790.

1950

With B. Lippman. Variational principles for scattering processes. I. *Phys. Rev.* 79:469-480.

1951

On gauge invariance and vacuum polarization. *Phys. Rev.* 82:664-679.

On the Green's functions of quantized fields. I, II. *Proc. Natl. Acad. Sci. U. S. A.* 37:452-459.

1958

Spin, statistics and the TCP theorem. *Proc. Natl. Acad. Sci. U. S. A.* 44:223-228, 617-619.

1959

With P. C. Martin. Theory of many-particle systems. I. *Phys. Rev.* 115:1342-1373.

Field theory commutators. *Phys. Rev. Lett.* 3:269.

1961

Brownian motion of a quantum oscillator. *J. Math. Phys.* 2:407.

1963

Commutation relations and conservation laws. *Phys. Rev.* 130:406-409.

1964

Coulomb Green's function. *J. Math. Phys.* 5:1606.

1965

Quantum Theory of Angular Momentum (eds. L. Biedenharn and H. van Dam). New York: Academic Press.

1966

Magnetic charge and quantum field theory. *Phys. Rev.* 144:1087-1093.

1968

With D. Saxon. *Discontinuities in Wave Guides.* New York: Gordon and Breach.

1970

Particles, Sources, and Fields. I. Reading, Mass.: Addison-Wesley.

1973

Particles, Sources, and Fields. II. Reading, Mass.: Addison-Wesley.

1978

With L. L. DeRaad Jr. and K. A. Milton. Casimir effect in dielectrics. *Ann. Phys.* 115(1):1-23.

1985

With B. G. Englert. Semiclassical atom. *Phys. Rev. A* 32:26-35.

1986

Einstein's Legacy: The Unity of Space and Time. New York: W. H. Freeman and Co.

1996

The Greening of Quantum Field Theory: George and I, Lecture at Nottingham, July 14, 1993. Printed in Julian Schwinger: *The Physicist, the Teacher, the Man,* ed. Y. J. Ng. Singapore: World Scientific.


Photograph by Orren Jack Turner.

LYMAN SPITZER JR.

June 26, 1914–March 31, 1997

BY JEREMIAH P. OSTRIKER

One of the leading theoretical astrophysicists of the 20th century, Lyman Spitzer showed a renaissance or even a classical figure in both his character and personal style. I once speculated that a biographer would someday remark on the importance of Spitzer's early exposure to ancient literature, and his family assured me that he had been, in fact, throughout his life strongly influenced by classical, especially Latin, models. If ever I have known an individual who fit the renaissance ideal of the gentleman scholar (based, of course, on earlier Latin archetypes), it was Lyman. The upright bearing, courteous speech, clarity, and total independence of mind were the dress of a person seemingly dropped into our midst from another age. Born in 1914 into a prosperous Toledo, Ohio, commercial family, he later married into the local, still wealthier clan of the Canadays. After Scott High School in Toledo and then Phillips Academy, Andover, Massachusetts, he received his B.A. at Yale in 1935, went to Cambridge University for a year (1935-1936), and there he was influenced by Arthur Eddington and Subramanian Chandrasekhar (an almost contemporary). Returning to the United States, he received his Ph.D. at Princeton under the legendary Henry Norris Russell (in 1938). Spitzer then went briefly to Harvard as a postdoctoral fellow, followed by a move to Yale, where he was appointed as instructor in 1939. It was shortly after moving to Yale that he married Doreen D. Canaday, herself a Bryn Mawr graduate, a totally charming and strong-willed woman with whom he raised a family of four children born between 1942 and 1954: Nicholas C., Dionis C., Sarah L., and Lydia S.

With the outbreak of World War II, Spitzer took leave from Yale to conduct scientific work in support of the war effort, initially as a member of the Special Studies Group at Columbia, then as director of the Sonar Analysis Group (at age 30). Radar was the major British technical contribution to the Allied war effort. While Lyman was always modest about the development, sonar along with the much more recognized A-bomb effort was one of the decisive technical contributions to the U.S. war machine. After the war, he returned briefly to Yale as associate professor (1946-1947). Spitzer then returned to Princeton University as professor in the spring of 1947, at the age of 33, succeeding Russell as chair of the Department of Astronomy and director of Princeton University Observatory. The scientific program of Princeton University Observatory, initiated in 1947 by Spitzer along with his contemporaneous colleague Martin Schwarzschild, was maintained as a leading center of astrophysics—especially theoretical astrophysics—for the last half of the 20th century, until Spitzer and Schwarzschild both died within a few weeks of each other in the spring of 1997.

A few words on how Spitzer planned and carried out his return to Princeton give insight into both his character and the times in which he lived. The year is 1946. Lyman is aged 32 and an associate professor at Yale. With great aplomb, he writes to the then leading light of astrophysics, Professor Harlow Shapley of Harvard College Observatory, who it appears had been commissioned by President Howard W. Dodds of Princeton to find a successor to the retiring

Princeton professor Henry Norris Russell. He begins his covering letter on a positive note.

For many reasons, I believe that the chairmanship at Princeton offers very great opportunities of the sort which interest me, and I would definitely accept an offer from Princeton University if it were along the lines which I visualize, and which I describe below...

The most important aspect of the Princeton opening, from my point of view, is the general policy of the University administration toward the Astronomy Department.

He includes in the letter rather precise details of the form of funding required from Princeton, the nature and title of positions required, and ends with characteristic formal but firm courtesy.

My own respect for the astronomy at Princeton in general and for Professor Russell in particular is so profound that it would be a great personal pleasure for me to come to Princeton under almost any conditions. The very strong support which astrophysics enjoys at Yale, however, would make it very difficult for me to leave New Haven, with it[s] opportunities for effective research and growth, unless the corresponding opportunities at Princeton are at least as great.

If the authorities at Princeton would like to discuss these proposals with me, I shall be very glad to visit Princeton in the near future. Naturally I should appreciate receiving your reaction and that of the Princeton administration to these ideas.

His plans for Princeton, described in an attachment to his letter to Shapley, asked for the resources to build a theoretical astronomy program in Princeton worthy of the opportunities of the age and the traditions of that institution. Excerpts from this document follow:

Princeton University is justly known as one of the world's leading centers of theoretical astronomy. This reputation has been built up over a considerable period of years, and should be preserved. The plan presented here is devised to continue this historical tradition in the field of theoretical astrophysics, and at the same time to preserve a balanced department by maintaining research in an observational field that is an integral part of the Princeton tradition—precise photometry of variable stars. In the first section below the scientific aspects of the plan are discussed, while the cost estimates are presented in the second section. It should be emphasized that in detail this plan is to be regarded as somewhat flexible, since its execution would naturally depend on the availability of qualified personnel as well as on the facilities at Princeton.

1. Scientific Program

It is proposed that the primary effort in astronomy at Princeton continue in the field of theoretical astrophysics, with three men of professorial rank in this field—the Director, Professor Stewart, and an additional man. Dr. Martin Schwarzschild would be an excellent choice for this third position, and there is reason to believe he might accept an offer of this type. If he were not available, and if no one of similar caliber could be found, a temporary Visiting Professor could be brought in, possibly a new man every year. On Professor Stewart's retirement, in some 15 years, it is assumed that his place would be taken by another theoretical astrophysicist with wide abilities and broad training.

To keep theory in touch with current observational problems, it is planned that in the near future the two new members of the permanent staff would each spend one academic term out of every four in a major observational center such as the Mount Wilson Observatory. It is understood that staff members would continue to receive their usual stipend from Princeton while carrying out research at other observatories in this manner. Such an arrangement would provide, at very moderate expense to Princeton, the observational facilities afforded by the world's largest telescopes. It is believed that the material obtained in these trips could also be used by Princeton graduate students, in keeping with the Princeton tradition.

Such a staff as outlined would serve as a center or focus of an active research group. A number of graduate students should be attracted each year by such a stimulating department. If a governmental Science Foundation is set up, and if such a Foundation decides to support theoretical astrophysics on a substantial scale, the astronomy Department at Princeton would make an ideal focus for such support. To cross-fertilize the different fields of astronomy, and to keep theorists and observationalists in touch with each other, it would be desirable to bring scientists from other institutions to Princeton from time to time for joint consideration of the major problems under study by astrophysicists. Thus the establishment of a considerable number of Visiting Professorships, financed by some governmental research unit, seems a definite possibility.

Well, as they say, the rest is history. All happened, as so often in Spitzer's life, exactly as he had planned. Princeton University Observatory became the world's leading institution in theoretical astrophysics almost instantly, with the addition of Spitzer and Schwarzschild, their students and associates. When formally offered the directorship at Princeton, Spitzer made his acceptance conditional upon the appointment of Schwarzschild as professor. At this time in history, when the Jewish faculty at Princeton was rare to nonexistent, this was taking a rather forceful stand. Building upon the foundation established by Henry Norris Russell, Spitzer and Schwarzschild together created a department with an enduring cordial atmosphere of mutual support and encouragement for astrophysical research at the highest level. The tradition of rigorous and creative scientific scholarship made Princeton a preeminent center of astrophysical research in the world. Many of his students went on to distinguished careers in astronomy.

Before turning to Spitzer's scientific work, let me say a word about his character and personality. While a paragon of personal integrity, he was also able to ascertain where his own advantage lay in every circumstance. So, in explaining why he had not early on accepted a junior position at Princeton, he later noted dryly, "It appeared that my chances of being offered Russell's position, if it became available, might well be greater if I were back at Yale than if I were already at Princeton." He loved pranks and sumptuous desserts. His pranks occasionally landed him in trouble, and he was essentially arrested by the Princeton University security police when he was found climbing up the side of the tower of the graduate college with a rope and possibly even pitons. It was with some difficulty that the limbs of the law were persuaded that he was a distinguished scientist, a chair of an academic department, and had in fact violated no written law! Other spoofs were more abstruse. He contributed a brief paper filled with plausible but insane mathematics under the pseudonym H. Pétard (actually the paper was written with J. Tukey) to the *American Mathematical Monthly* in 1938 entitled "A Contribution to the Mathematical Theory of Big Game Hunting."

Lyman also had rather strong and very highly principled but quite private political views. During the 1972 presidential campaign between Richard Nixon and George McGovern, he made a formal date with me to discuss "some nonastronomical matters." When I appeared in his office, he asked me directly, "What is your opinion of the character of Richard Milhous Nixon?" Delirious with the opportunity to vent my own rather intemperate, negative views, I carried on with vigor at length. Then, as I paused for a breath at one point, he stood up (signifying that the meeting was at an end), offered me his hand, and said, "Jerry, thank you so much. I greatly value learning your views on the many topics that you follow more closely than do I." Years later I was told by an individual I thought to be reliable that Lyman was on one of Nixon's extended "enemies lists," presumably due to his large financial contribution to Nixon's opponent. I was never able to confirm this, but when I asked Lyman about it directly many years after the event, his equivocal reply was, "I did not think much of McGovern, but I firmly concurred with your view that Nixon did not have a character suitable to be the President of the United States of America."

What kind of a scientist was he? Lyman Spitzer chose to tackle big, challenging problems. He wrote classic theoretical papers that helped shape at least three different fields of science: interstellar matter, the dynamics of star clusters, and the physics of plasmas. In addition, he proposed one of the leading methods for magnetically confining thermonuclear fusion and led a pioneering effort to do so at Princeton. And, finally, by both example and inspired leadership, he was a prophet and among the most influential proponents of the U.S. effort in space astronomy. Selected writings of Lyman Spitzer Jr., *Dreams, Stars, and Electrons,* published by Princeton University Press in 1997, provides a useful introduction to Spitzer's work in all of these areas.

The sheer volume of work is staggering, with four monographs and more than 100 articles in refereed scientific journals (and double that number if one were to include other widely cited and influential contributions) in over half a century of active research. Spitzer's trademark was the incisive physical insight, coupled with the ability to formulate and accurately solve appropriate model problems. The impact of his work is strengthened by a crisp and lucid style of exposition. He invariably discovered at the outset of an investigation which were the important physical effects to be modeled carefully and which processes could be ignored in the initial assay. This is a skill that cannot easily be taught, but the readers of Spitzer's papers will come away with a vision of how a remarkable scientific mind works.

In the late 1930s Spitzer was struck by the fact that elliptical galaxies contained old stars but no large amounts of interstellar gas, whereas spiral galaxies that contained substantial amounts of gas also had young stars. He concluded that stars must be forming even today from clouds of gas and dust. Today this is obvious, but at that time the realization that star formation is an ongoing process was quite new to astrophysics. It took decades for the implications to sink in. Spitzer began a theoretical study of the physics of interstellar matter that lasted almost six decades. He worked on the theory of the heating and cooling of interstellar gases, stressing the presence and importance of interstellar magnetic fields, the likelihood of pressure equilibrium among various components, and the significant role played by interstellar dust grains. His investigations, which established the field of interstellar matter as a rich discipline, culminated in the publication of his classic book *Diffuse Matter in Space* in 1968, followed by *Physical Processes in the Interstellar Medium* in 1978. When I arrived as a wet-behind-the-ears, newly minted research associate and lecturer in 1965 after a year of "finishing school"—my postdoctoral year in Cambridge, England, paralleled that of Spitzer 29 years earlier—my first task was to provide a close reading of the manuscript for *Diffuse Matter in Space*. It was dense going for someone who, though well trained (by S. Chandrasekhar), had neither formal nor informal exposure to the subject matter, and I checked and rechecked every equation, reading with attention every line. I seriously doubt I added much, if anything, to the work, but the effort, the contact, and mentoring by Lyman as I taught the interstellar medium graduate course at Princeton certainly contributed crucially to my own most cited publication "A Theory of the Interstellar Medium—Three Components Regulated by Supernova Explosions in an Inhomogeneous Substrate," written jointly with C. F. McKee in 1977 (*Astrophys. J.* 218:148-169).

Spitzer, following H. Alfvén, helped to establish the physical and mathematical foundations of plasma physics in the 1950s. Spitzer recognized early the importance of determining the thermal, electrical (the "Spitzer conductivity"), and mechanical transport coefficients in a fully ionized gas, and he made the initial calculations of thermal and electrical conductivities and diffusion coefficients for plasmas. His pioneering studies in basic plasma physics culminated in the volume *Physics of Fully Ionized Gases* (1956), which became a classic, oft cited text, central to the education of successive generations of plasma physicists. He also carried out the first computations of the toroidal confinement of a plasma.

Following up on his theoretical work in plasma physics, Spitzer proposed to the U.S. Atomic Energy Commission (in 1951) a project to try to contain and harness the nuclear burning of hydrogen at temperatures exceeding those found on the sun, terming the machine a "Stellarator," which would be "designed to obtain power from the thermonuclear reactions between deuterium and either deuterium or tritium." First approved as Project Matterhorn in 1953, the Princeton Plasma Physics Laboratory at the James Forrestal Campus became the leading laboratory in this field. After shepherding its creation, Spitzer led the laboratory until 1967. Now, in 2006, over half a century after the founding of Project Matterhorn, the laboratory is returning to the Stellarator concept, hoping to demonstrate the power of hydrogen fusion. This was the design first proposed by Spitzer in the paper entitled "The Stellarator Concept," published by *Physics of Fluids* in 1958. Big science was still a hands-on activity in this era and Spitzer notes:

> The Atomic Energy Commission (AEC) supported the idea, after we had persuaded Jim Van Allen at Iowa to head this work for a few years. Van wisely suggested that we start with a simple, modest device. The resultant "Table-top stellarator," our Model A, was indeed primitive. Martin Schwarzschild and I spent several weekends sitting on the floor of our rabbit hutch, winding flat copper wire around 2-inch diameter glass tubes.

It is an extraordinarily apt measure of Spitzer's prescience that Princeton Plasma Physics Laboratory is now, one-half century after Lyman proposed it, building, after an international technical review, what may be the most promising design yet for taming the physical process that makes the stars shine; the National Compact Stellarator Experiment (NCSX) is scheduled to begin operation in 2009.

In stellar dynamics Spitzer clarified the process of "relaxation" introduced by S. Chandrasekhar and showed how this leads a stellar system to approach a singular state, as the effective conduction of heat outward in the star cluster (caused by gravitational interactions between pairs of stars) forces the inner parts to contract more and more rapidly. He discussed how the relaxation process in real star clusters is accelerated by the existence of a spectrum of stellar masses, but retarded by the presence of binary stars. His many contributions to the field were summarized in 1987 in the book *Dynamical Evolution of Globular Clusters.*

Spitzer's seminal contributions to space astronomy are legendary and were recognized in 2003 when the large infrared space observatory launched earlier that year was named the Lyman Spitzer Telescope. One important reason for this recognition was that this telescope was optimized to see the infrared radiation emitted by dust from the dense gaseous regions within which all stars seem to form, and Spitzer had carried out pioneering studies of the physics of interstellar dust. In 1941 he discussed the important dynamical effects of radiation pressure acting on interstellar grains. In 1948 he investigated the effects of dust grains on the temperature of interstellar gas, recognizing the important heating effect of photoelectrons ejected from interstellar grains. To estimate this heating rate Spitzer carried out pioneering work on the charging of interstellar grains, a problem he returned to in 1950, noting the different levels of grain charging to be expected in different interstellar regions. In 1949 the phenomenon of starlight polarization was discovered and immediately identified as being due to the polarizing effect of aligned interstellar dust grains. It was not clear what physical process could produce the observed alignment, and to this date this question has not been fully answered. Spitzer was attracted to this problem, and over the years made a number of important contributions.

In 1946 he proposed, in a report under Project RAND titled "Astronomical Advantages of an Extra-Terrestrial Observatory," the development of large space telescopes. In the abstract he points out, quite amazingly for what appears to be the first time, "the results that might be expected from astronomical measurements made with a satellite vehicle... While a more exhaustive analysis would alter some of the details of the present study, it would probably not change the chief conclusion—that such a scientific tool, if practically feasible, could revolutionize astronomical techniques and open up completely new vistas of astronomical research." He then goes on to outline the advantages to be gained due to greater angular resolution (overcoming astronomical "seeing" problems), to the increased wavelength coverage available, and to the stability of a low-gravity environment. He continued to lobby for an astronomical space program, using after 1966 the Space Science Board of the National Research Council as a platform for his efforts.

All of the benefits foretold have been realized by present satellite experiments, with Spitzer having been a major contributor to their realization. Under his direction a group of Princeton scientists developed the extremely successful *Copernicus* (32-inch) ultraviolet satellite. Launched in 1972, it made a number of significant astronomical discoveries, including among them an accurate value for the cosmologically important ratio of deuterium to hydrogen in interstellar space. But this satellite barely escaped being a total failure. In Florida just before launch when much of the team was partying, Lyman, studying technical specifications, discovered a potential defect in the engineering that might have caused the instrument to lock in place and be undeployable. He computed where to set the focus and telephoned his results to the launch site. As it eventuated, his diligence was amply rewarded, because the drive motor did fail and his timely action saved the mission.

Lyman really enjoyed getting into the nitty-gritty engineering details on which success or failure of such missions can rest. Discoveries made by the *Copernicus* satellite led to fundamental changes in our understanding of the interstellar medium. The current, very productive Hubble Space Telescope, which was approved in 1977 and launched in 1990, is now returning incomparable pictures of the cosmos, and was in a quite literal sense Spitzer's brainchild. He played major roles in shepherding it through many difficult stages of its existence from the earliest planning to its recent refurbishment. At certain critical points this required heavy old-fashioned lobbying of Congress with John Bahcall (from the Institute for Advanced Study). His evident enjoyment and success in these ventures surprised those who (erroneously) considered him to be shy. Spitzer continued to sit as an elder statesman on the Space Telescope Institute Council, providing wise guidance for this extraordinarily important scientific venture until his death in 1997.

In addition to his purely scientific skills, Spitzer's vigorous personality, sound judgment, and basic human decency propelled him to positions of leadership at a variety of levels. At Princeton, where he was chair of the Department of Astrophysical Sciences and director of the observatory for a third of a century (1947-1979), he built one of the world's leading institutions for astronomical education and research, with an almost unique atmosphere for research. The congenial and supportive (and rather formal) environment Spitzer created in collaboration with his brilliant colleague Martin Schwarzschild, where the generous interest of each scientist in the other's research led to increased productivity and originality, as well as the cross-fertilization exemplified best by Spitzer's own work, was widely admired but not easily imitated.

Spitzer took his teaching very seriously. For decades he gave a course on the physics of the interstellar medium, and the accompanying monograph, *Diffuse Matter in Space*, (1968) established a new scientific field and educated a generation of students who, as they settled into other institutions, propagated these teachings. Not all worked in this area, and a brief list of the Ph.D. students supervised in whole or in part by Spitzer and went on to distinguished careers in astrophysics would certainly include B. Elmegreen, G. Field, J. Gaustad, J. R. Gott, C. Heiles, D. Morton, B. Oke, R. Sanders, T. X. Thuan, L. Searle, and R. Weyman.

As a national scientific administrator, he served as director of the wartime Sonar Analysis Group (1944-1946), president of the American Astronomical Society (1958-1960), and chair of the Space Telescope Institute Council (1981-1990), and held other major national leadership positions on numerous committees, commissions, and the like that guided the scientific life of the nation. Spitzer's service to his country was recognized with medals for scientific achievement and national service from NASA in 1972, 1976, and 1991 and the U.S. National Medal of Science in 1980. His scientific work received worldwide recognition, and he was the recipient of many honors, including membership in the National Academy of Sciences in 1952, the Henry Norris Russell Prize of the American Astronomical Society in 1953, the Henry Draper medal of the National Academy of Sciences in 1974, the James Clerk Maxwell Prize of the American Physical Society in 1975, the Crafoord Prize of the Royal Swedish Academy of Sciences in 1985, and the James Madison Medal of Princeton University in 1989.

He was an enthusiastic music lover, and an active mountain climber (making the first ascent of the spectacular Mt.

Thor on Canada's Baffin Island). And, even in retirement, he barely slowed his active outdoors life, continuing to indulge his passion for climbing and endowing the Lyman Spitzer Climbing Grants of the American Alpine Club to support cutting-edge climbing expeditions. Spitzer formally retired in 1982 but did not slow his active involvement in forefront research, involving both theoretical work and, following the launch of Hubble Space Telescope, observational studies using the high-resolution spectrograph to study interstellar absorption lines. On March 31, 1997, Spitzer spent the day in Peyton Hall, working on a scientific manuscript, and happily discussing recent developments with his colleagues. At home that evening after a full day of work, he suddenly collapsed and died, concluding an extraordinary and exemplary life.

THE AUTHOR IS INDEBTED to Professor B. T. Draine for providing a summary of Spitzer's work on dust grain physics.

SELECTED BIBLIOGRAPHY

1940

The stability of isolated clusters. *Mon. Notes R. Astron. Soc.* 100:396-413.

1941

The dynamics of the interstellar medium. I. Local equilibrium. *Astrophys. J.* 93:369-379.

1942

The dynamics of the interstellar medium. III. Galactic distribution. *Astrophys. J.* 95:329-344.

1946

Astronomical advantages of an extra-terrestrial observatory, Project RAND. *Astron. Q.* 7:131-142.

1948

The formation of stars. *Phys. Today* 1:6-11.

The temperature of interstellar matter. I. *Astrophys. J.* 107:6-33.

1950

With R. S. Cohen and P. M. Routly. The electrical conductivity of an ionized gas. *Phys. Rev.* 80:230-238.

1951

With W. Baade. Stellar populations and collisions of galaxies. *Astrophys. J.* 113:413-418.

With M. Schwarzschild. The possible influence of interstellar clouds on stellar velocities. *Astrophys. J.* 114:385-397.

1952

Interplanetary travel between satellite orbits. *J. Am. Rocket Soc.* 22:92-96.

1953

With R. Härm. Transport phenomena in a completely ionized gas. *Phys. Rev.* 89:977-981.

1956

On a possible interstellar galactic corona. *Astrophys. J.* 124:20-34.

Physics of Fully Ionized Gases. New York: Interscience.

1958

Disruption of galactic clusters. *Astrophys. J.* 127:12-27.

The Stellarator concept. *Phys. Fluids* 1:253-264.

1968

With P. G. Bergmann. Physics of sound in the sea. I. Transmission. Summary. In *Physics of Sound in the Sea, Part I Transmission*, chap. 10, eds. P. G. Bergmann and A. Yaspan, p. 236. New York: Gordon and Breach.

Diffuse Matter in Space. New York: Interscience.

1969

With J. N. Bahcall. Absorption lines produced by galactic halos. *Astrophys. J. Lett.* 156:L63-L65.

Equipartition and the formation of compact nuclei in spherical stellar systems. *Astrophys. J. Lett.* 158:L139-L143.

1971

Dynamical evolution of dense spherical star systems. *Pontif. Acad. Sci. Scripta Varia* 35:443-475.

With M. H. Hart. Random gravitational encounters and the evolution of spherical systems. I. Method. *Astrophys. J.* 164:399-409.

1973

With J. B. Rogerson, J. F. Drake, K. Dressler, E. B. Jenkins, D. C. Morton, and D. G. York. Spectrophotometric results from the Copernicus satellite. I. Instrumentation and performance. *Astrophys. J. Lett.* 181:L97-L102.

With J. F. Drake, E. B. Jenkins, D. C. Morton, J. B. Rogerson, and D. G. York. Spectrophotometric results from the Copernicus satellite. IV. Molecular hydrogen in interstellar space. *Astrophys. J. Lett.* 181:L116-L121.

1974

History of the large space telescope. In *American Institute of Aeronautics and Astronautics 12th Aerospace Sciences Meeting, Washington, D.C., Jan. 30-Feb. 1*, pp. 3-6. New York: AIAA.

1978

Physical Processes in the Interstellar Medium. New York: Wiley.

1984

Dynamics of globular clusters. *Science* 225:465-472.

1985

Average density along interstellar lines of sight. *Astrophys. J. Lett.* 290:L21-L24.

Clouds between the stars. Crafoord lecture, Royal Swedish Academy of Sciences. *Phys. Scripta* T11:5-13.

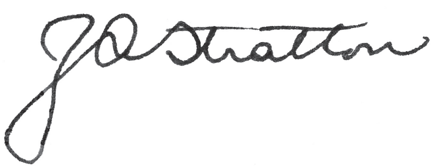
1987

Dynamical Evolution of Globular Clusters. Princeton: Princeton University Press.

1997

With J. P. Ostriker, eds. *Dreams, Stars, and Electrons.* Princeton: Princeton University Press.

MIT Museum



JULIUS ADAMS STRATTON

May 18, 1901–June 22, 1994

BY PAUL E. GRAY

JAY, AS HE WAS KNOWN by nearly all who worked with him, served the Massachusetts Institute of Technology, the Radiation Laboratory at MIT, the federal government, the National Academies, and the Ford Foundation during his long and productive life. His work at MIT, as a member of the faculty and subsequently as provost, chancellor, and president, was vital to the development of both research and education during periods of rapid growth and change at MIT.

EARLY YEARS

Stratton was born on May 18, 1901, in Seattle, Washington. His father, Julius A. Stratton, was an attorney who founded a law firm well known and respected throughout the northwest; later he became a judge. His mother, Laura Adams Stratton, was an accomplished pianist. Following his father's retirement in 1906, the family moved to Germany, where young Julius attended school through age nine and became fluent in German. In 1910 the family returned to Seattle, where he completed his public school education.

Stratton came to MIT, with which he was associated for 74 years, as the result of an accident at sea and on the advice of a fellow student. From an early age he was interested in how things worked and in building things, particularly devices that involved electricity. In high school he grew fascinated by radio in the early days of spark-gap transmitters and galena crystal detectors. These interests, combined with the shutdown of amateur radio operations during World War I and the desire to serve the nation, led him to qualify as a commercial radio operator (second grade) and to sign on during summer vacations as a shipboard radio operator.

Stratton had been admitted to Stanford for matriculation in September 1919 when he signed on for the summer right before as radio operator aboard the SS *Western Glen* out of Seattle headed for Japan and Manchuria. The ship encountered a typhoon near Kobe, Japan, and went to the rescue of another vessel in distress nearby. The *Western Glen* also experienced engine failure at the start of the return voyage, requiring a return to port in Japan. These misfortunes resulted in a late arrival back in the United States, too late for Stratton to enroll at Stanford that year. In search of an alternative, he succeeded in gaining late admission to the University of Washington in Seattle. That year he pursued his primary interests, electricity and mathematics. A classmate persuaded him, however, to apply for transfer to MIT, where he was admitted in 1920. He found his way out to the east coast by plying his favorite trade as a radio operator—this time first grade—aboard the SS *Eastern Pilot* bound for New York City via the Panama Canal, finally arriving in Boston in August 1920, a week before the start of classes.

At MIT (known at that time colloquially as "Boston Tech") Stratton enrolled in the Electrical Communications: Telegraph, Telephone, and Radio option of the Department of Electrical Engineering, which he later described as "far more interesting than that of ordinary dynamo-electric machinery. Line telegraphy and telephony involve some of the most complex mathematics known."[1] He was awarded the S.B. degree in June 1923, with a thesis titled "The Absolute

Calibration of Wavemeters." The equipment he developed for this project generated harmonics up to 30 megahertz from a one-kilohertz tuning fork.

During his senior year, Stratton considered continuing his studies in Europe. Professor Arthur Kennelly of the Electrical Engineering Department urged him to enroll at the Université de Nancy, and gave him a reference. He traveled to Paris via Cherbourg (this time as a passenger) with the dual goals of continuing his education and becoming fluent in the French language. During the 1923-1924 academic year, he traveled to Nancy, Grenoble, Toulouse, and Italy, returning to the United States in August 1924. At this time he was not focused solely on science and mathematics. While at Grenoble and Toulouse, he seriously considered a doctoral thesis on the influence of science on literature—a reflection of the influence of his MIT English teacher, "Tubby" Rogers.

From September 1924 through June 1926 Stratton worked as a research assistant in communications at MIT and studied for his master's degree in electrical engineering, graduating with a thesis titled "A High Frequency Bridge." During this period he wrote to his father with remarkable prescience about the trajectory of his career: "I will admit that an ultimate goal which would cause me complete satisfaction would be the administration of such an institution as Tech or the Bureau of Standards at Washington."[2]

Upon completion of his master's degree, Stratton was awarded a traveling fellowship that enabled him to return to Europe, where many universities were seething with excitement over the latest developments in quantum theory and atomic structure. He enrolled for a doctor of science degree in mathematical physics at the *Eidgenössische Technische Hochschule* (ETH) in Zurich, Switzerland, where he studied under Peter Debye and graduated in March 1928 with a thesis titled "Streuungskoeffizient von Wasserstoft nach der

Wellenmechanic" (The Scattering Coefficient of Hydrogen According to Wave Mechanics). He then returned to MIT as an assistant professor in electrical engineering, a modern physicist embedded in an engineering department.

Stratton's desire to see the world continued to grow. During the summers he traveled to Africa, the Yukon, and Ecuador. His interest in other cultures and nations ran deep, a quality very much in line with MIT's increasing international reach and stature.

On June 14, 1935, in Saint Paul's Chapel at Ivy Depot, Virginia, Julius Adams Stratton married Catherine Nelson Coffman. From this fortunate union came three daughters: Catherine, Cary, and Laura. Their mother, known to all as Kay, is active in the MIT community as a member of the Council for the Arts and as the guiding force behind two annual panel discussions: one each fall on a selected critical issue, and one each spring on some aspect of aging gracefully.

THE YOUNG PHYSICIST

Stratton's experiences at the ETH helped shape his professional focus and further stimulated his passion for mathematics and physics. As he captured the shift: "In the years 1923-1924 I was thinking of a doctorate in literature or philosophy. This was to be the subject: The Influence of Science on 19th Century French Literature. I decided to go into pure physics. The years 1925 through 1928 changed my mind."[3] In 1930 his MIT appointment was moved to the Department of Physics.

Karl Taylor Compton, the newly appointed president just arrived from Princeton, was setting out to strengthen the sciences in general at MIT, and especially to give greater emphasis to modern physics. Stratton became an integral part of this transformation. He was promoted to professor in 1941, the same year his book *Electromagnetic Theory* was published. This work, now long out of print, is still widely consulted and referenced more than a half century later. In 2006 the Institute of Electrical and Electronic Engineers selected it for reprinting as part of their series of electrical engineering classics.

Much of Stratton's research in the 1930s was carried out at the Round Hill Experiment Station in South Dartmouth, Massachusetts. Colonel Edward Green owned this estate. He had inherited a large fortune from his mother—Hetty Green, known in her day as the witch of Wall Street—and was insatiably curious about science, particularly radio communication. He invited MIT to use the property for research in meteorology and the propagation of electromagnetic waves. Stratton's work there involved the propagation of very short radio waves and light through rain and fog. He also studied the possibility of using intense electromagnetic radiation to disperse fog, and made measurements of the field of an antenna over the open sea, employing the *Mayflower*, a dirigible loaned to him by the Goodyear Zeppelin Company. He prepared and published, through the National Academy of Sciences, tables of spheroidal functions—solutions of differential equations arising in his study of antennas. Between 1927 and 1942, 11 of his technical papers were published in refereed journals.

THE WAR YEARS

As for many other scientists, the German invasion of Poland in 1939 marked a watershed in Stratton's career. Well before the start of the war, British scientists had employed high-frequency radio waves (ca. 100 megahertz) as an early form of radar by exploiting reflections from aircraft. This system, known as "Chain Home" was very important during the Battle of Britain in the summer of 1940, when it gave the Royal Air Force warning of the approach of the German bombers.

While the scientists realized that higher frequencies would permit much smaller antennas and would yield greater precision in target location, vacuum tube transmitters were unable to generate radiation of sufficient intensity at frequencies in the gigahertz range. But the invention of the microwave cavity magnetron a few months after war broke out made possible the creation of radar systems that would prove remarkably effective as a tool of war. Unsure that they could develop the invention at home under wartime conditions, British scientists in August 1940 sent the Tizard Mission, (more formally "The British Technical and Scientific Mission to the United States") along with the magnetron and its developers, to the United States, where American engineers and scientists were in a position to develop—without hindrance or distraction—militarily useful microwave radar systems.[4]

When the federal government established the Radiation Laboratory at MIT in October 1940, Stratton was one of many who joined in the task of making microwave radar useful to the military on land and sea as well as in the air. He was a natural for this work, given his understanding of electromagnetic radiation and the applications of Maxwell's equations. He was appointed in November 1940 as a volunteer consultant to the Microwave Section of Division D of the National Defense Research Council, and seconded by MIT to the Rad Lab, as it came to be known.

His initial work at the Rad Lab was on the development of loran (long-range navigation), a system that employed synchronized transmission of pulsed signals from a variety of locations.[5] Comparison of signals from two or more transmitters enabled determination of the precise location of the receiving aircraft or ship. Loran foreshadowed the development a half century later of the Global Positioning System

(GPS)—a ubiquitous satellite-based system that employs similar principles. Loran was the first system developed by the Rad Lab to be applied by the military. It was instrumental both in winning the war against German submarines in the Atlantic and in directing Allied aircraft flights sent into Europe on bombing missions.

The lab developed multiple radar systems, some for use on the ground for targeting weapons and for detection of aircraft; some installed in aircraft, both for nighttime combat and for location of enemy submarines and surface ships; and some for use on ships in surveillance and directing the fire of heavy weapons. The lab's influence on the conduct of the war was evident in both the eastern and western theaters.

The rapid pace of development and of improvements to radar equipment at the Rad Lab was not matched, unfortunately, by the American military bureaucracy, which proved so lethargic that the troops usually had "the third best [radar] set." Stratton outlined the problem in a letter to his Rad Lab colleague Edward Bowles:

> Despite sincere good will on the part of individual officers I am nevertheless impelled to the belief that the planning and implementation of a rational program and the procurement of radar equipment is proceeding with intolerable slowness. . . The most glaring defect in the system is the number of hands through which each problem and paper must pass for discussion, revision, and endorsement. . . Unfortunately the time consumed by conference after conference is bought, under the circumstances of war, by soldiers lives.[6]

This problem resisted solution throughout the war years and beyond. Procurement of rapidly evolving technical equipment by the government is an ongoing problem to this day.

In August 1942 Stratton was appointed expert consultant to the secretary of war, Henry G. Stimson, and served in that capacity until December 1945. In this role he made frequent visits to the theaters of war. In October 1942, as a member of a committee investigating communication problems in the North Atlantic, he traveled by air to England with extended stopovers in Presque Isle, Labrador, Greenland, and Iceland. In 1943, soon after the Allied invasion of North Africa, he spent time in Algiers, Tunis, Italy, and London assessing radar utility and communications effectiveness.

Decades later Stratton told me of a trip he had made in the spring of 1944 by British destroyer from Britain to Iceland. It was a dangerous passage because of German submarine activity in the North Atlantic. On board also was the archbishop of Canterbury, headed to greet British troops in Iceland. When the vessel pulled into port, the commander of the British garrison was surprised to see the archbishop because nearly all British soldiers training in Iceland had been quietly returned to Britain to prepare for imminent invasion. The commander, assuming that the cleric would be upset about having made the dangerous trip for no reason, summoned the courage to explain over dinner. “Do not be concerned, son,” the Archbishop assured him. “I have come to Iceland often in the spring to fish for salmon and I was not going to miss the opportunity this year.”

During the Rad Lab’s five years of operation, more than 4,000 people were employed either there or in related efforts, such as the Radar School, that trained soldiers—more than 8,000 in all—to operate and maintain the new equipment. The taxpayer dollars expended through the laboratory approached $100 million, a sum in excess of MIT’s total expenditures during the first 75 years of its existence![7]

Robert Buderi wrote, “The Atomic Bomb only ended the war. Radar won it.”[8] Stratton, it must be noted, contributed to this outcome. He was awarded a Presidential Medal of Merit in 1946 upon recommendation of the secretary of war.

SENIOR LEADERSHIP

In August 1945 the Office of Scientific Research and Development, the agency that had overseen all laboratories created to aid the war effort, was shut down and the Radiation Laboratory began to wind down its affairs. But there had already been talk about keeping its work alive in one form or another. "As early as 1943 there was speculation about a peacetime sequel to the Rad Lab. There had grown a remarkable spirit of cooperation between the physicists and the electrical engineers."[9] On January 1, 1946, Stratton took over administration of the disappearing Rad Lab's Division of Basic Research, which had been created in August 1945 following the surrender of Japan. On the suggestion of John Slater, head of MIT's physics department, Stratton named it the Research Laboratory of Electronics (RLE).

Research support in the early days of RLE came from the Department of Defense through a multiservices contract administered by the Office of Naval Research, much of it as a block grant of $600,000 per year. RLE was "responsible for extending the useful range of the electromagnetic spectrum . . . to shorter wavelengths, approaching ultimately that of infra-red."[10] Title to the temporary MIT buildings that had housed the Rad Lab and to all its equipment, was transferred to RLE in July 1946. The largest of the temporary buildings became Building 20. Until its demise in 1998 at age 55 it was much cherished research space for some of MIT's most creative and productive minds.

While interdepartmental laboratories are now common in universities, the novel idea, in 1946, of a laboratory with its own director, in which faculty would have dual loyalties both to the lab and to their home department, and in which technical and research staff would be appointed without review by a department, received mixed reviews from the MIT faculty. Further, there was concern about the appropriateness of military support in peacetime, even though all the research to be undertaken was unclassified. Stratton, as founding director, together with his colleagues from the electrical engineering and physics departments who formed the RLE Steering Committee, resolved these and other critical issues during the formative years of the nation's first university-based interdepartmental research laboratory. Its history, now spanning more than six decades of scientific and engineering accomplishments, owes a great deal to his leadership of the laboratory in the beginning.

Soon, however, he entered on another phase of his career. In 1949 James Rhyne Killian Jr. succeeded Karl Taylor Compton as MIT president and appointed Stratton as MIT's first provost. This was the start of a period of extraordinary growth at MIT. Vannevar Bush's landmark 1945 report, *Science, the Endless Frontier*, led to the creation of the National Science Foundation and the National Institutes of Health. Cold-war tensions, much raised in 1957 by the Soviet's launch of *Sputnik*, caused enrollments in engineering and science to soar. The federal government vastly increased financial support for research and for students in science and engineering fields. Charles Stark Draper's Instrumentation Laboratory (originally affiliated with MIT's Aeronautics Department, later to become the independent Draper Laboratory) expanded to add the Apollo mission to its development of inertial navigation systems for the military services. The compound annual growth rate of sponsored research was in double digits until the late 1960s.

Stratton was responsible for overseeing the physical and intellectual growth of MIT in these decades and for the thoughtful development and implementation of necessary structures, policies, and procedures that became the foundation of and model for MIT in the modern age. During his tenure as provost, two new schools were created: the School of Humanities and Social Studies in 1950 (now the School of Humanities, Arts, and Social Sciences) and the School of Industrial Management in 1952 (now the Sloan School of Management).

In 1957 Killian was called to Washington to serve as President Eisenhower's science adviser, and Stratton, who had been appointed chancellor in 1956 (again a new position at MIT), became acting president and an ex officio member of the governing board. In 1958 he was elected the 11th president. His presidency saw much physical expansion at MIT. New buildings for Chemistry, Earth Sciences, Biology, and the Center for Materials Science and Engineering filled former parking lots. McCormick Hall, the first dormitory for undergraduate women, opened in 1965. This was the crucial first step in increasing the number of women at MIT, who now make up 45 percent of the undergraduate student body, a dramatic change from fewer than 3 percent in the postwar period. Stratton was so liked and respected by students that they suggested the new student center, completed in 1966, be named for him.

As founding director of RLE, as provost, and as chancellor and president, Stratton deserves—along with Compton and Killian—a large share of the credit for transforming MIT from the premier school of engineering to a modern research university.

THE NATIONAL ACADEMY OF ENGINEERING

The National Academy of Sciences (NAS) was created in 1863 under a charter signed by President Lincoln. NAS membership was, and continues to be, a widely respected honor, treasured by those who are elected. Although from early on the NAS maintained a section for accomplished engineers, it was small and relatively few engineers were elected to membership. In 1961 the idea of a comparable national academy to recognize outstanding engineers was proposed in an article in the *Journal of Engineering Education*.[11] The national engineering societies associated with the Engineering Joint Council strongly backed the idea. A specific proposal was made to the NAS Council in April of 1963.

The precise way that the engineers' academy would be set up and structured provoked strong feelings and frequently contentious debate. Some wished the new academy to be independent of the NAS; others wanted it as an integral component of the NAS.

The president of the NAS, Frederick Seitz, was deeply skeptical at first about the idea of a sister academy. He voiced his reservations to the NAS Council, referencing a letter written by Thomas K. Sherwood in support of an affiliated academy of engineers.[12] Such an affiliated academy, he argued

> could result in a large and powerful organization competing with the NAS. It will be able to enlist strong support from industry as well as government. It is likely that congressional committees and government agencies will find more reason to ask for advice and help from the NAE [National Academy of Engineering] if and when it becomes respectable than from the scientific community. . . The first major difference of opinion as to who should do what could lead the engineers to terminate the proposed cooperative arrangement and go off entirely on their own.

Stratton, who served then on the NAS Council, chaired a committee that was formed in 1963 to study the matter. He expressed his views on the question of independence versus affiliation in a letter to committee member Augustus B. Kinzel:[13]

> I am prepared to take the engineers at their word and to believe that they would prefer to develop a new academy in affiliation with the National Academy of Sciences but that if this proves unfeasible they will proceed on their own. It seems to me that it would be most unwise and indeed unfortunate

for the National Academy of Sciences to reject the overtures made by the engineers without a sincere try to work something out. Such an unfortunate action on our part could only deepen a feeling of distrust that has lately been developing between scientists and engineers.

In the fall of 1964 Seitz—finally persuaded by arguments on the other side—suggested to the Council that the NAE become a part of the NAS, rather than having the engineers seek a separate charter for themselves. The NAS Council approved the proposal on December 5, 1964. The NAE, consisting then of the 25 founding members (Stratton among them), held its first meeting on December 10. A public announcement appeared the next day.[14]

The wisdom of creating an affiliated academy for the engineering community, and of subsequently engaging it as a partner with the NAS in the management of the National Research Council, has been evident in the four-plus decades since the decision was reached. The two academies, joined more recently by the Institute of Medicine, represent the organization of first response to requests from the federal government for expert analysis of issues in science, technology, and health. Stratton's persuasiveness and clarity of vision were important in shaping the evolution of the enterprise, now commonly referred to simply as "The National Academies."

THE FORD FOUNDATION

Following his retirement as president of MIT in 1966, Stratton was elected chairman of the board of the Ford Foundation, at that time the largest grant-making charitable agency in the nation. He had served as a trustee of the foundation since 1955. At the MIT retirement party given in Stratton's honor, his lifelong friend—physicist William P. Allis—described the transition as "taking off academic robes to put on foundation garments."

During his term as chairman, Stratton changed a number of Ford Foundation policies that related to board size, composition, and function. Some of these changes reflected practices that had concerned him during his years as a trustee. For example, board meetings had been conducted in a manner that left little opportunity or time for discussion. Officers of the foundation had been instructed not to talk with a trustee unless the president of the foundation was present. And he believed that the specific interests of some trustees influenced grants to too great a degree. He described the changes he made in an oral history conducted after his term ended:

> My procedure has been to involve every member [in discussions], to turn to him for what he thinks on each issue.

> [We] introduced the counterpart of the collegiate visiting committee. [Such trustee committees had long been in use at MIT.] The purpose was clearly to get the trustees to know the staff members and what was going on. [There was] one for each division and one for the vice-presidents. My impression is that these have taken on in a most extraordinary fashion. And it was the VPs themselves who said "This is great. Let's have meetings. Let's keep this up." This was all part of the strategy to break down the walls that lay between the trustees and the staff. I think it has been a very very useful contribution.[15]

Stratton also introduced a mandatory retirement age and fixed terms for trustees. He worked with the membership committee to appoint trustees with attention to varieties of experience.

When Stratton retired, the board concluded its resolution with the following sentences:

> "He has demonstrated in every word and action the meaning of the standards to which he has held us all: that we are here to serve not our own ends but those for which the Ford Foundation is chartered. He leaves the foundation stronger than he found it, and all who care for its work are deeply in his debt."[16]

During his years as chairman of the Ford Foundation, Stratton also accepted presidential appointment as chairman of the Committee on Marine Science, Engineering, and Resources (COMSER). The committee of 15 announced by President Lyndon B. Johnson on January 9, 1967, commenced work six weeks later. COMSER emerged from a growing sense, both here and abroad, that the world could no longer afford to go without an authoritative assessment of troublesome, potentially disastrous environmental problems. "The need to develop an adequate national ocean program arises from a combination of rapidly converging and interacting forces: world population growth; the need for sources of protein; ocean industries as components of inviting opportunities for economic growth." Under Stratton COMSER was charged with recommending "National Policy to develop, encourage and maintain a coordinated, comprehensive, and long range program in marine science for the benefit of mankind . . . expanding scientific knowledge of the marine environment and of developing an ocean engineering capability to accelerate exploration and development of marine resources."

COMSER's final report, *Our Nation and the Sea, A Plan for National Action,* was presented to a different president following its publication in January 1969.[17] With remarkable clarity and foresight it laid out key problems facing the marine environment.

- The environment [of the sea] is being affected by man himself, in many ways adversely. It is critical to protect man from the vicissitudes of the environment and the environment, in turn, from the works of man.
- The oceans and marine-related activities must be viewed in the context of the total land-sea-air environment. Mankind is fast approaching a stage where the total planetary

environment can be influenced, modified and perhaps be controlled by human activities.

•Means for reaching reasonable accommodation of competing national interests must be found to achieve efficient and harmonious development of the sea's resources. The atmosphere, which is so influenced by the oceans, knows no national boundaries; the nations of the world share a common interest in its monitoring and prediction and in its modifications.

One outcome of the COMSER's work was the creation of NOAA, the National Oceanic and Atmospheric Administration. Other recommendations relating to international cooperation and to the conservation of marine resources were strongly opposed by fishing and other commercial marine interests, and did not lead to hoped-for changes in policy.

Stratton's close relationship with MIT continued during his time at the Ford Foundation. He remained a member of the MIT's governing board and served on several committees, including a presidential search committee formed in the fall of 1970.

FINAL CHAPTERS

Stratton returned to MIT full-time in 1971, at the conclusion of his term as Ford Foundation chairman. His affection and concern for the MIT, central as they were to his professional life for more than half a century, were undiminished by his years in New York, and he immersed himself once more in the life of MIT with the same quiet energy and vigor for which he was so well known.

Stratton's lifelong interest in the history and cultures of institutions was reflected in his determination to prepare a comprehensive history of the origins of MIT, and he began this work in earnest when he returned to Cambridge in 1972. His administrative assistant Loretta B. Mannix, who had gone to the Ford Foundation with him, did most of the archival research for this project. Eventually she progressed from editing his work to doing portions of the writing herself, and Stratton made it clear that they were to be listed as co-authors when the book was published. Following Stratton's death, Ms. Mannix carried on with the writing as long as she was able.

When it became clear in late 2002 that help was needed to complete the book, MIT commissioned Philip N. Alexander, a research associate in the MIT Program in Writing, to complete the research, to bring together the separately written chapters, to write the last six chapters, and to make it whole. As the page that often holds a dedication puts it:

A Work
Initiated by Julius A. Stratton
Continued by Loretta H. Mannix
Completed by Philip N. Alexander

The book, titled *Mind and Hand—The Birth of MIT*, was published by MIT Press in 2005. It begins with the European antecedents of technical education in the United States, introduces the founders, describes the origins of MIT, and concludes with descriptions of the first curriculum, the modes of instruction, the faculty, and the first group of students who entered in 1865.

In January 1971 the MIT governing board elected Jerome B. Wiesner as president, and also named the writer of this memoir as deputy to the president, with the title of chancellor. This was only the second use of this title at MIT—Stratton had been the first to hold it more than 15 years earlier.

Our common titular bond led to a friendship that lasted until Stratton's death on June 22, 1994. While I had had two occasions as a very junior faculty member to talk with him in the 1960s, we did not become well acquainted until his return to MIT. Then we regularly sought a quiet place to talk, usually over lunch, about MIT affairs. As a new, somewhat green senior administrator, I benefited beyond measure from both the questions he raised and insights that he imparted during these informal chats.

During the last 20 years of his life, Jay Stratton had the opportunity to observe up close the continuing transition from premier engineering school to world-class research university that he—together with Compton and Killian—had set in motion at MIT 60 years earlier.

His life spanned most of the twentieth century, a period of wars hot and cold, intervals of peace, the Great Depression, scientific discoveries, and technological innovations unimaginable at the end of the prior century. The faculty, staff, and students are only the closest of those who benefit from his work, his character, and his values—demonstrated in a life well spent.

I AM INDEBTED TO two members of the staff of the MIT Archives—Elizabeth Andrews, associate director, and Nora Murphy, research archivist—who provided essential guidance and to Philip Alexander, whose careful reading of an earlier draft of this paper was of great help.

NOTES

1. Stratton's letter to parents, October 14, 1922, MIT Archives Manuscript Collection MC-341, Series I, Box 2.
2. Stratton's letter to father, April 4, 1924, ibid.
3. Ibid.
4. R. Buderi. *The Invention That Changed the World,* pp. 108-109, 265. New York: Touchstone, 1996.
5. Ibid.
6. Stratton's letter to Edward L. Bowles, May 4, 1943, MIT Archives Manuscript Collection, MC-341, Series III, Box 29.

7. 1945-1946 Report of the President, MIT, pp. 8, 10, 16.
8. R. Buderi. *The Invention That Changed the World*, p. 247. New York: Touchstone, 1996.
9. R.L.E.: 1946+20 (An RLE publication, May 1946), p. 1.
10. Ibid, p. 5.
11. Work, Harold K. The question of establishing a national academy of engineering. *Journal of Engineering Education* 51, no.9 (1961): 698-700.
12. Seitz letter to the NAS Council, May 13, 1964, MIT Archives Manuscript Collection MC-341, Series III, Box 29.
13. Stratton's letter to Kinzel, March 26, 1963, MIT Archives Manuscript Collection MC-341, Series III, Box 29.
14. NAS/NAE announcement December 11, 1964, MIT Archives Manuscript Collection MC-341, Series III, Box 29.
15. Stratton Oral History, Ford Foundation, MIT Archives Manuscript Collection MC-341, Series III, Box 12, pp. 8, 99-105.
16. MIT Archives Manuscript Collection MC-341, Series III, Box 13.
17. January 7 White House Announcement, 1969, MIT Archives Manuscript Collection MC-341, Series III, Box 13.

SELECTED BIBLIOGRAPHY

1926

Complete suppression of a single frequency by means of resonant circuits and regeneration. *Rev. Sci. Instrum.* 13(1):95-105.

1927

Zerstreuungskoeffizient für kurze Wellen nach der Schrödingerschen Theorie. *Phys. Z.* 28:316-323.

1928

Streuungskoeffizient von Wasserstoff nach de Wellenmechanik. (Doctoral thesis.) *Helv. Phys. Acta* 1:47-74.

1930

The effect of rain and fog on the propagation of very short radio waves. *Proc. IRE* 18(6):1064-1074.

1931

With H. G. Houghton. A theoretical investigation of the transmission of light through fog. *Phys. Rev.* 38:159-165.

1932

With H. A. Chinn. The radiation characteristics of a vertical half-wave antenna. *Proc. IRE* 20(12):1892-1913.

1935

Spheroidal functions. *Proc. Natl. Acad. Sci. U. S. A.* 21(1):51-56.

Spheroidal functions of the second kind. *Proc. Natl. Acad. Sci. U. S. A.* 21(6):316-321.

1939

With L. J. Chu. Diffraction theory of electromagnetic waves. *Phys. Rev.* 56:99-107.

1941

Electromagnetic Theory. New York: McGraw-Hill.

With L. J. Chu. I. Forced oscillations of a cylindrical conductor. II. Forced Oscillations of a conducting sphere. III. Forced oscillations of a prolate spheroid. *J. Appl. Phys.* 12(3):230-248.

With P. M. Morse, L. J. Chu, and R. A. Hutner. *Elliptic Cylinder and Spheroidal Wave Functions.* New York: John Wiley.

1946

Research laboratory of electronics at the Massachusetts Institute of Technology. *Rev. Sci. Instrum.* 17(2):81-83.

1948

Foreword. In *Tables of Scattering Functions for Spherical Particles.* Applied Mathematics Series 4:v-vii. Washington D. C.: U.S. National Bureau of Standards.

1953

Research and the university. *Chem. Eng. News* 31(25):2581.

1956

Science and the educated man. *Phys. Today* 9(4):17-20.

With P. M. Morse, L. J. Chu, J. D. C. Little, and F. J. Corbató. *Spheroidal Wave Functions.* New York: John Wiley.

1958

Science and economic progress. In *Private Investment: The Key to International Industrial Development*, pp. 173-182. New York: McGraw-Hill.

Universities—A key to America's leadership. *Technol. Rev.* 60(4):201.

1960

The interdependence of science and art. *Technol. Rev.* 62(9):38.

Abstract and concrete. *Museum News* 39(1):34-37.

1961

Physics and engineering in a free society. 1. Education. *Phys. Today* 14(3):20-23.

Statement opposing H.R. 1, a bill to establish a national science academy. *High. Educ. Natl. Affairs Bull.* 10(23).

1963

The progress of education in science and engineering. In *The Educational Register*, 23rd ed., 1963-1964, pp. 9-14. Boston: Vincent-Curtis.

Science and the process of management. In *Proceedings*, CIOS X113 International Management Congress, Sept. 16-20, 1963, New York City, pp. 46-49. New York: Council for International Progress in Management (USA).

1964

Science and the process of management. *Res. Manage.* 7(2):79-90.

Learning and action. *Proc. Am. Philos. Soc.* 108(5):387-394.

1965

The new academy of engineering. *Technol. Rev.* 67(9):41-44.

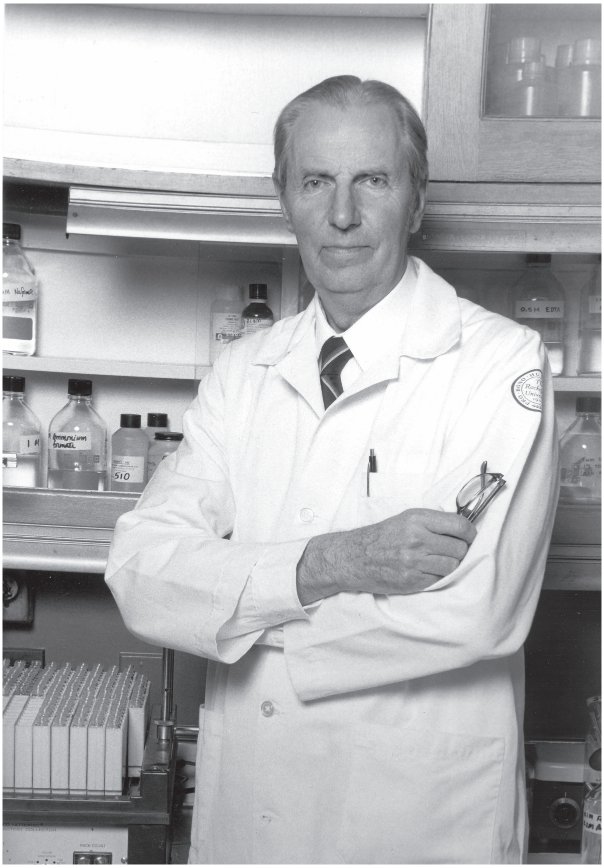
The humanities in professional education. *Carn. Rev.* Spec. Issue, Fall(6):40-43.

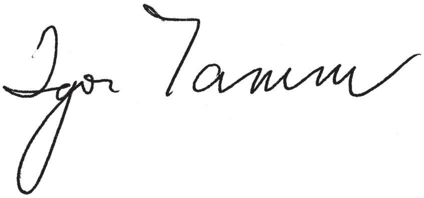
1967

The future of progress. *Yearbk. Am. Iron Steel Inst. 1967*, pp. 17-30.

1969

Our nation and the sea. *Congr. Rec.* 115(97):E4876-E4878.





IGOR TAMM

April 27, 1922–February 6, 1995

BY PURNELL W. CHOPPIN

IGOR TAMM, VIROLOGIST, CELL BIOLOGIST and pioneer in studies of virus replication and the chemical inhibition of such replication, died at the age of 72 on February 6, 1995, at his home in Watch Hill, Rhode Island, of a chronic lung disease that he had battled with characteristic quiet courage and without complaint for over 50 years. He was born on April 27, 1922, in Tapa, Estonia. He attended the State English College in Tallinn, Estonia, from 1939 to 1944 and the Tartu University Medical Faculty in Tartu, Estonia, from 1942 to 1943. His father, an architect, died when Tamm was young. Igor's mother, who came to the United States after him, spent a number of years associated with the American Geographical Society.

As a young boy Tamm had lived under both the German and Soviet occupations of Estonia. In 1943, after the word went out that Estonian teenagers were to be taken into the German army and sent to the Russian front, he and a fellow medical student escaped in a small boat, which they sailed late at night through the German blockade to Finland. Finland was at that time occupied, so they stowed away in the coal storage area of a freighter for several days until it arrived in Sweden. Soon after emerging from the ship, he developed the first symptoms of the disease from which he would suffer for the rest of his life.

In Stockholm, Tamm entered medical school at the Karolinska Institutet. He had long been interested in going to the United States, and while at the Karolinska, he met an American diplomat, a Yale alumnus, who encouraged him to apply to the Yale Medical School. In 1945 Tamm transferred to Yale, receiving his M.D. with honors in 1947. After two years of house-staff training in internal medicine at the Yale-New Haven Hospital, he moved in 1949 to the Rockefeller Institute for Medical Research, later to become Rockefeller University, which remained his beloved scientific home until his death, and which he graced by his presence for four and a half decades. There he rose to the rank of professor and senior physician in 1964, Abby Rockefeller Mauzé Professor in 1986, and professor emeritus in 1992. In 1959 he became head of the Laboratory of Virology, succeeding his mentor Frank L. Horsfall Jr., who left the Rockefeller Institute to become the director of the Sloan-Kettering Institute for Cancer Research. This transition was very smooth because Horsfall, as vice-president and physician in chief of the hospital, had for several years been increasingly involved in administrative matters, and Tamm was in effect acting as head of the laboratory and primary mentor of the younger people there.

While at Yale Medical School, Tamm met a classmate, Olive E. Pitkin of Bennington, Vermont. They were married in 1953. Olive, a highly talented pediatrician, spent many years with the New York City Department of Health. They had three children—Carol, Eric, and Ellen—who have pursued rewarding careers. Throughout their 42 years of marriage Olive was not only a loving wife but also a friend, professional colleague, and partner in every sense of the word. Her support throughout his long illness, particularly in the last few years, was truly monumental. It enabled Igor to do what he wanted most, to be with family and friends, and continue to pursue his love of science and research to the very end.

Igor Tamm's contributions to virology and cell biology were both enormous and varied. They began with his isolation, purification, and extensive biochemical characterization of what became known as the Tamm-Horsfall glycoprotein, named for him and his mentor. Tamm isolated the mucoprotein from human urine. He was searching for a natural inhibitor of virus replication and he chose urine as a possible source, influenced by his studies at Yale under J. P. Peters, a prominent renal expert of the day. In addition to its property as a receptor for influenza virus, the Tamm-Horsfall mucoprotein was later studied by those interested in renal function and disease. Years after Tamm's work, it was shown to have a protective effect on kidney stone formation. This mucoprotein was the first virus receptor to be isolated and purified, and thus was a landmark in virology. Many years later Tamm said he still regarded this early work as one of his most important contributions.

Because of its virus receptor activity, the mucoprotein was a competitive inhibitor of virus adsorption, the first step in virus infection, as well as a substrate for the influenza virus receptor-destroying enzyme, neuraminidase. The work with the mucoprotein was the start of a long career in the study of the inhibition of viral multiplication by both natural products and chemicals, particularly benzimidazole derivatives and guanidine.

For four decades, beginning with his paper in 1952 with Karl Folkers and Horsfall on the effect of benzimidazoles, he employed inhibitors to elucidate the biochemical and cell biological mechanisms of virus replication and its inhibition, as well as the mechanisms and prevention of virus-induced cell injury. Many viruses were involved, including influenza, mumps, vaccinia, and adenoviruses, but the most intensively studied were the enteroviruses, particularly poliovirus. His contributions were many, highly original, and major. In these studies he enjoyed the collaboration of many students, postdoctoral fellows, and junior faculty members, including Hans J. Eggers, Lawrence A. Caliguiri, and Rostom Bablanian. With graduate student David Baltimore and Richard M. Franklin, Baltimore's primary mentor in the virology laboratory, Tamm participated in the elucidation of the synthesis of poliovirus RNA-dependent RNA polymerase. His work with the benzimidazoles as an inhibitor of virus and cellular RNA synthesis led to the use of these compounds by others in studies of cellular RNA synthesis, including James E. Darnell at Rockefeller University. Most of Tamm's work in this area was very widely recognized, however one important footnote is worthy of mention here. In a 1960 paper in the Journal of Experimental Medicine titled "On the Role of Ribonucleic Acid in Animal Virus Synthesis," he showed that 5,6-dichloro-lβ-D-ribofuranosylbenzimidazole (DRB), which he had previously found to inhibit RNA virus synthesis, inhibited adenovirus replication. However, adenovirus was a DNA virus. Tamm drew the prescient conclusion from this finding that RNA synthesis was required for DNA virus replication. Significantly, this occurred before the important description of messenger RNA (mRNA) which is copied from cellular DNA and is translated into protein. DRB was inhibiting that step in adenovirus replication.

There were many other important contributions of the Virology Laboratory at Rockefeller University under Tamm's leadership. Prominent among these was the discovery in 1963 by one of his graduate students, Peter J. Gomatos, that reovirus and wound tumor virus contained double-stranded RNA as their genetic material. This was the first description of double-stranded RNA in any biological system. Subsequently, such RNA was found to have important roles outside of virus replication, such as in the induction of interferon and in small interfering RNAs.

Other work in different areas of virology included studies with Purnell W. Choppin, then a postdoctoral fellow, on the identification and characterization of genetic heterogeneity of influenza virus particles with respect to their interactions with cellular receptors, antibodies, and mucoprotein inhibitors of virus adsorption. These studies were done on H2N2 influenza virus strains, isolated by Choppin during the 1957 Asian influenza pandemic. One of these strains, RI/5, became one of the most widely used for studies of H2N2 influenza virus.

With graduate student Frederick Wheelock, studies were done on mitosis and cell division in cells infected with Newcastle disease virus, a paramyxovirus, and there were many other significant studies done with students, postdoctoral fellows, and collaborators, including Nicholas H. Acheson, Lawrence Alstiel, William D. Ensminger, Roger Hand, Barbara Jasny, Robert M. Krug, Frank R. Landsberger, Douglas S. Lyles, Anne G. Mosser, Suydam Osterhout, Lawrence S. Sturman, and many others.

Tamm's interest in inhibition of virus replication eventually led him to research on interferon, interleukins, and other cytokines, particularly the control of their synthesis and their effects on both normal and malignant cells. This work became his principal interest in later years. These studies were carried out over a period of almost two decades with graduate students, postdoctoral fellows, and members of the virology laboratory at Rockefeller, including Pravin B. Sehgal, Lawrence M. Pfeffer, Eugenia Wang, Toyoko Kikuchi, James S. Murphy, and James Krueger, as well as with colleagues in other laboratories at Rockefeller, such as James E. Darnell, and at other institutions, such as Jan Vilcek at New York University. The work included elucidation of the induction and the enhancement of interferon production by benzimidazoles and other agents, and studies of interferon mRNA synthesis and stability. Extensive work was also done on the effects of interferon on the growth, volume, division, and motility of human cells. The effects of interferon on the organization of microfilaments in cells were explored, as well as the reduction by interferon of pinocytosis by cells and of cellular insulin receptors. This work on the effects of interferon on cells helped lay the groundwork for the therapeutic use of interferon not only for viral diseases such as hepatitis but also diseases such as multiple sclerosis. His last work was focused on the role of interleukin-6 on normal and cancer cells and showed that this cytokine decreases the cell-to-cell adhesion of human ductal breast carcinoma cells. Knowledge of this kind has significance for an understanding of the metastasis of cancer cells.

In addition to those who worked directly with Tamm, there were many highly productive scientists who benefited greatly as a member of the laboratory that he headed and the excellent academic and research environment that it provided. These included Richard W. Compans, Walter H. Doerfler, Polly R. Etkind, Allan R. Goldberg, William W. Hall, Donald H. Harter, Kathryn V. Holmes, Ming-chu Hsu, Hans-Dieter Klenk, Robert A. Lamb, Sondra Lazarowitz, James J. McSharry, C. Lennart Philipson, Richard W. Peluso, Christopher Richardson, David S. Roos, Andreas S. Scheid, Samuel M. Silver, and many more.

Complementing his original research, Tamm with Frank Horsfall edited *Viral and Rickettsial Infections of Man*, which for many years was the definitive text in the field. He was an editor or member of the editorial board of several journals, including Journal of Immunology, Proceedings of the

Society for Experimental Biology and Medicine, Journal of Experimental Medicine, Biochemical Pharmacology, and Journal of Interferon Research. He served on many advisory boards and study sections for government agencies and private organizations concerned with research, including the National Institutes of Health, the Armed Forces Epidemiological Board, the American Cancer Society, and the Sloan-Kettering Institute for Cancer Research. In 1976 the National Institutes of Health turned to him to be the general chairman of the timely, comprehensive, and important Task Force on Virology.

In addition to election to the National Academy of Sciences in 1975, Tamm's honors included the Alfred Benzon Prize from Denmark (the first American to receive this award) for "outstanding research on the replication of viruses," and the Sarah L. Poiley Memorial Award from the New York Academy of Sciences.

Igor Tamm's great accomplishments in research were matched by his skills as a mentor and adviser to a very large number of young scientists who spent time in the laboratory that he headed. His students, postdoctoral fellows, and junior faculty members have gone on to highly productive research careers: professorships in universities and senior positions in research institutes around the world, working in a wide variety of departments (e.g., microbiology, virology, cell biology, genetics, biochemistry, medicine, and neurology) in the United States, Canada, Germany, France, United Kingdom, Switzerland, Japan, and other countries. Several have held responsible research positions in industry. In addition, others have gone on to senior administrative positions, such as dean or president of a large research institute or major university. Several have been elected to the National Academy of Sciences and/or received many other honors, including one Nobel Prize (David Baltimore).

On a personal note, I remember very well the first time I saw Igor. It was late in 1956 when I came to the Rockefeller Institute to discuss the possibility of my joining the lab as a postdoctoral fellow in the summer of 1957. When I met him, he was 34, tall, and rail slim; his blue eyes were sparkling, and he treated me with great courtesy. He was clearly interested in me as a person as well as a prospective fellow, and he thoughtfully took the time to show me not only around the lab but also the institute as a whole. Among those he introduced me to in the lab that day was James S. Murphy, who had joined the lab a year earlier and had the privilege of spending much of his career there with Igor until the time of his death.

I saw Igor for the last time in Watch Hill, Rhode Island, a few months before his death. He was still alert, rail slim, and his blue eyes still sparkled. I had left Rockefeller University in 1985 to join the Howard Hughes Medical Institute, but we had kept in close touch. As always he was deeply interested in what I, my wife, Joan, and our daughter were doing both professionally and personally. He was of course very enthusiastic about the work that he was still carrying out with Toyoko Kikuchi, his long-time and very talented research assistant and colleague, and James Murphy. His great spirit, courtesy, and interest in his friends and his science were undiminished by the relentless course of his disease and his dependence on the oxygen source to which he was tethered by long plastic tubes that enabled him to move around and work.

In between those two occasions there were 38 years of treasured interactions. Igor was my scientific mentor, role model, collaborator, and above all, a cherished friend. He was generous to a fault. We shared much of our scientific careers, and for 15 years he graciously allowed me to share in the leadership of the virology laboratory at Rockefeller

University, through which passed some of the most talented, intelligent, and wonderful people I have ever known. At Igor's funeral service Alexander ("Alick") G. Bearn, his close friend since 1950 and colleague at Rockefeller for many years, spoke eloquently of Igor. Near the end of his talk Alick said, "If there is one quality that captures the essence of Igor it is ..." and before he pronounced the word I knew he was going to say, "integrity," because that was so obviously the case. And it was integrity not only in his every thought and action but also in his words both spoken and written. He always spoke clearly, calmly, and precisely, in a baritone voice, each sentence meticulously crafted and grammatically correct. He was fluent in five languages, Estonian, English, German, Russian, and Swedish, and could, in his own words, "Get by in French." To know the care and precision with which Igor fashioned the written word, one had only to submit a draft of a paper to him for his comments and see, with both dismay and appreciation of its correctness, what he had done to it with his editorial pen. In the precomputer days in the lab, there were many drafts of a manuscript on yellow paper before it was ready to go on white.

Igor was a fine tennis player, and as a young scientist at Rockefeller in the late 1940s and early 1950s, he was occasionally summoned to play doubles with the indomitable director of the Rockefeller Institute Hospital, Thomas M. Rivers. Rivers was also the dean of American virology. Other young scientists there who shared in that privilege included Alick Bearn, Henry Kunkel, and Harold Ginsberg, all of whom were later elected to the National Academy of Sciences. I have been told that they wished to play on the side with Rivers as he somehow usually won. Igor's skill on the tennis court was matched by his ability as a graceful ice skater and dancer of the European School. At parties and dances at Rockefeller when the musicians struck up a waltz, accomplished women dancers, my wife among them, would gravitate toward Igor.

Nothing is more illustrative of the great spirit and love of science, family, and friends that Igor had than his actions in the last few years of his life. After achieving professor emeritus status at Rockefeller University, and with the relentless progress of disease, shortness of breath, and dependence on oxygen, Igor and Olive moved to their home in Watch Hill, where they not only spent precious time with their family but also continued to warmly welcome friends. Through it all Igor continued his scientific work, no longer with his own hands but through daily contact by phone and mail with Toyoko Kikuchi and James Murphy. Always uncomplaining and eager to discuss his research with colleagues, his work flourished. In the last year of his life he published three major papers and others were in preparation. On the last day of his life Igor was making notes for his next conversation with Toyoko about the work in the lab. He died peacefully in his sleep.

No one fits better than Igor Tamm the words of Shakespeare in Henry VIII: "He was a scholar and a ripe and good one; exceeding wise, fair-spoken, and persuading."

A great many of us had the privilege to know Igor as a student, fellow, or colleague, and a true friend. Whether with him for only a few or for many years, we remember this gentle, wise, extraordinarily capable, loyal man with great respect and affection. We all owe him very much.

SELECTED BIBLIOGRAPHY

1950

With F. L. Horsfall Jr. Characterization and separation of an inhibitor of viral hemagglutination present in urine. *Proc. Soc. Exp. Biol. Med.* 74:108-114.

1952

With K. Folkers and F. L. Horsfall Jr. Inhibition of influenza virus multiplication by 2,5-dimethylbenzimidazole. *Yale J. Biol. Med.* 24:559-567.

1953

With K. Folkers, C. H. Shunk, D. Heyl, and F. L. Horsfall Jr. Inhibition of influenza virus multiplication by alkyl derivatives of benzimidazole. III. Relationship between inhibitory activity and chemical structure. *J. Exp. Med.* 98:245-259.

1956

With K. Folkers and C. H. Shunk. Certain benzimidazoles, benzenes, and ribofuranosylpurines as inhibitors of influenza B virus multiplication. *J. Bacteriol.* 72:59-63.

1957

With J. R. Overman. Relationship between structure of benzimidazole derivatives and inhibitory activity on vaccinia virus multiplication. *Virology* 3:185-196.

1959

With E. F. Wheelock. Mitosis and division in HeLa cells infected with influenza or Newcastle disease virus. *Virology* 8:532-536.

1960

With M. M. Nemes and S. Osterhout. On the role of ribonucleic acid in animal virus synthesis. I. Studies with 5,6-dichloro-1β-D-ribofurano-sylbenzimidazole. *J. Exp. Med.* 111:339-349.

With R. Bablanian. On the role of ribonucleic acid in animal virus synthesis. II. Studies with ribonuclease. *J. Exp. Med.* 111:351-368.

With P. W. Choppin. Studies of two kinds of virus particles which comprise influenza A2 virus strains. I. Characterization of stable homogeneous substrains in reactions with specific antibody, mucoprotein inhibitors, and erythrocytes. *J. Exp. Med.* 112:895-920.

1962

With H. J. Eggers. Differences in the selective virus inhibitory action of 2-(α-hydroxybenzyl)-benzimidazole and guanidine HCl. *Virology* 18:439-447.

1963

With P. J. Gomatos. The secondary structure of reovirus RNA. *Proc. Natl. Acad. Sci. U. S. A.* 49:707-714.

With D. Baltimore, H. J. Eggers, and R. M. Franklin. Poliovirus-induced RNA polymerase and the effects of virus-specific inhibitors on its production. *Proc. Natl. Acad. Sci. U. S. A.* 49:843-849.

With H. J. Eggers and D. Baltimore. The relation of protein synthesis to formation of poliovirus RNA polymerase. *Virology* 21:281-282.

With P. J. Gomatos. Animal and plant viruses with double-helical RNA. *Proc. Natl. Acad. Sci. U. S. A.* 50:878-885.

1964

With P. J. Gomatos and R. M. Krug. Enzymic synthesis of RNA with reovirus RNA as template. I. Characteristics of the reaction catalyzed by the RNA polymerase from Escherichia coli. *J. Mol. Biol.* 9:193-207.

With P. W. Choppin. Genetic variants of influenza virus which differ in reactivity with receptors and antibodies. In *Ciba Foundation Symposium on Cellular Biology of Myxovirus Infections*, eds. G. E. W. Wolstenholme and J. Knight, pp. 218-235. London: A. Churchill.

1965

With R. Bablanian and H. J. Eggers. Studies on the mechanism of poliovirus-induced cell damage. II. The relation between poliovirus growth and virus-induced morphological changes in cells. *Virology* 26:114-121.

1968

With L. A. Caliguiri. Action of guanidine on the replication of poliovirus RNA. *Virology* 35:408-417.

1970

With L. A. Caliguiri. The role of cytoplasmic membranes in poliovirus biosynthesis. *Virology* 42:100-111.

1976

With P. B. Sehgal and J. Vilcek. Regulation of human interferon production. II. Inhibition of interferon messenger RNA synthesis by 5,6-dichloro-1-β-ribofuranosyl-benzimidazole. *Virology* 70:542-544.

With P. B. Sehgal, E. Derman, G. R. Molloy, and J. E. Darnell. 5,6-dichloro-1-β-ribofuranosyl-benzimidazole inhibits the initiation of hnRNA chains in HeLa cells. *Science* 194:431-433.

1979

With L. M. Pfeffer and J. S. Murphy. Interferon effects on the growth and division of human fibroblasts. *Exp. Cell Res.* 121:111-120.

1982

With E. Wang, F. R. Landsberger, and L. M. Pfeffer. Interferon modulates cell structure and function. In *UCLA Symposia on Molecular and Cellular Biology, vol. 25, Interferons*, eds. T. C. Merigan and R. M. Friedman, pp. 159-179. New York: Academic Press.

1984

With L.M. Pfeffer, E. Wang, and J. S. Murphy. Interferon-induced changes in cell motility and structure. In *Lymphokines vol. 9*, ed. E. Pick, pp. 37-70. New York: Academic Press.

1987

With M. Kohase, L. T. May, J. Vilcek, and P. B. Sehgal. A cytokine network in human diploid fibroblasts: Interactions of β-interferons, tumor necrosis factor, platelet-derived growth factor, and interleukin-l. *Mol. Cell. Biol.* 7:273-280.

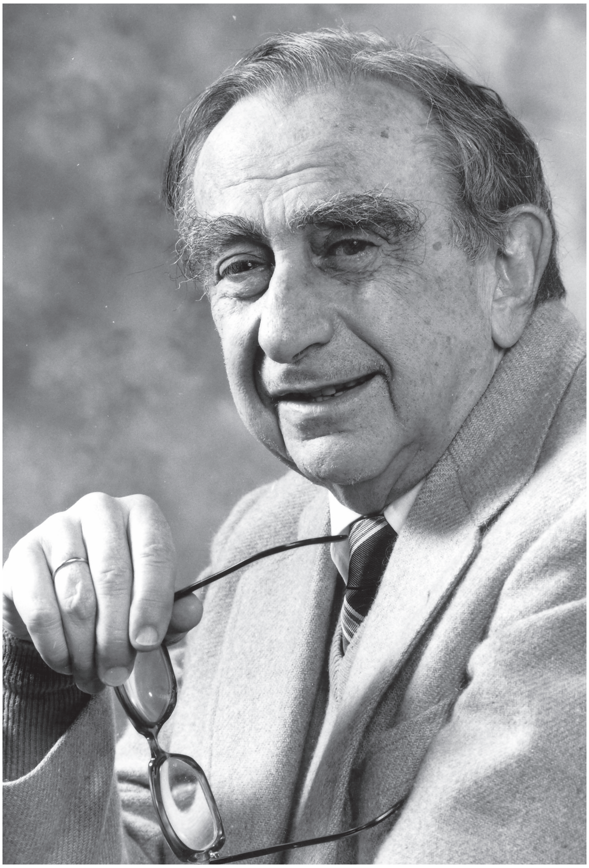
1989

With I. Cardinale, J. Krueger, J. S. Murphy, L. T. May, and P. B. Sehgal. Interleukin-6 decreases cell-cell association and increases motility of ductal breast carcinoma cells. *J. Exp. Med.* 170:1649-1669.

1994

With T. Kikuchi, I. Cardinale, and J. Krueger. Cell adhesion-disrupting action of interleukin-6 in human ductal breast carcinoma cells. *Proc. Natl. Acad. Sci. U. S. A.* 91:3329-3333.

With I. Cardinale, T. Kikuchi, and J. Krueger. E-cadherin distribution in interleukin-6-induced cell-cell separation of ductal breast carcinoma cells. *Proc. Natl. Acad. Sci. U. S. A.* 91:4338-4342.



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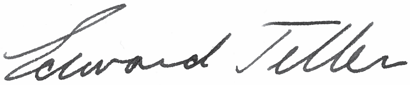

EDWARD TELLER

January 15, 1908–September 9, 2003

BY FREEMAN J. DYSON

AT THE END OF HIS LONG LIFE Edward Teller with the help of his editor Judith Shoolery published his memoirs (2001), a lively and poignant account of his adventures in science and politics. Hostile reviewers of the memoirs pointed out that some details of his stories are inaccurate. But Teller writes in his introduction, "Our memories are selective; they delete some events and magnify others. Just the simple act of recalling the past affects the recollection of what happened. That some of my remembrances are not the commonly accepted version of events should not be surprising." Memoirs are not history. Memoirs are the raw material for history. Memoirs written by generals and politicians are notoriously inaccurate. A writer of memoirs should make an honest attempt to set down the course of events as they are recorded in memory. This Teller did. If some of the details are wrong, this detracts little from the value of his book as a panorama of a historical epoch in which he played a leading role. I have used the memoirs as the basis for this brief summary of his career.

Teller was born in 1908 into a prosperous middle-class Jewish family in Budapest. He lived through the turbulent years of World War I, the dismemberment of the Austro-Hungarian Empire, the short-lived Communist regime of Bela

Kun, and the devastating currency inflation that followed, protected by loving and resourceful parents. All through his life, from childhood to old age, he had a gift for friendship. His memoirs are full of stories about his friends and the tragic fates that many of them encountered. He cared deeply for them as individuals and described them with sympathetic understanding. He escaped from sharing their fate when he emigrated from Hungary to Germany in 1926, from Germany to Denmark in 1933, to England in 1934, and to America in 1935. He always retained an acute sense of the precariousness of human life and the fragility of political institutions. He was one of the lucky survivors of a great tragedy, when barbarians overran Europe and destroyed the world of his childhood. He saw America as the last refuge, for himself and for the civilization that he cherished. That was why he saw it as his inescapable duty to keep America armed with the most effective weapons, with bombs for deterring attack, and with missiles for active defense.

The springtime years of Teller's professional life, the years when he was happiest with his work and his friends, were the seven years between 1926 and 1933 that he spent as a student in Germany. His stay in Germany started badly. Riding a trolley car in Munich to meet some friends for a hiking trip, he overshot the meeting place, jumped off the moving trolley car, and fell under the wheels. His right foot was chopped in half. As he lay in the road assessing the damage, he thought how lucky he was not to be one of the millions of young men who had lain wounded on the muddy battlefields of World War I a few years earlier. At least he was alive, with a clean wound and the certainty of being rescued. The surgeon in Munich reconstructed what was left of his foot so that he could still walk on the heel. With the help of a prosthesis he became agile enough to go hiking in the mountains and to play a respectable game of

Ping-Pong. He observed that his mother suffered more than he did from the accident. As she sat grieving by his bedside at the hospital, he tried unsuccessfully to cheer her up. For her it was a deeply tragic event, while for him it was merely a nuisance that did not touch the important things in his life. The accident gave him confidence. As he liked to say when I got to know him 20 years later, if it doesn't kill you, it makes you stronger. The accident gave him a good excuse to leave Munich and go to Leipzig to work with Heisenberg.

The years 1926-1933 were the time when German science was blazing with creative activity while the Weimar Republic was crumbling. When Teller joined the group of young people working at Leipzig with Heisenberg as leader, Heisenberg was 28. He had invented quantum mechanics in 1925 and then invited all and sundry to join him in using quantum mechanics to understand the workings of nature. Quantum mechanics described the behavior of atoms, and so it should be able to explain everything that atoms do. It should be possible with quantum mechanics to explain all of atomic physics, most of solid-state physics, most of astrophysics, and all of chemistry. There were enough good problems, so that every student could find something important to do. Teller, having been trained as a chemist, chose chemistry as the subject to be explained with quantum mechanics. He started well by beating Heisenberg at Ping-Pong. He and Heisenberg remained friends for life. After World War II, when many American physicists condemned Heisenberg for staying in Germany through the Hitler years, Teller went out of his way to befriend him. He knew that Heisenberg had never been a Nazi, and he respected Heisenberg's decision to stay loyal to his country and share its fate.

In Leipzig Teller wrote a Ph. D. thesis (1930) on the hydrogen molecule ion, the simplest of all molecules. He was able to calculate not only the ground state but also the excited quantum states of the molecule, using an old-fashioned mechanical calculator. But he did not enjoy working alone. He much preferred the give-and-take of working together with friends. Almost all his work after the thesis was done jointly with others. During his years in Germany he collaborated fruitfully with Lev Landau, George Placzek, and James Franck, solving various problems on the borderline between physics and chemistry. As he himself said, he was a problem solver rather than a deep thinker. He enjoyed solving problems, whether or not they were important. The years 1926-1933 were harvest time for problem solvers. In those years the problem solvers laid the foundations for most of modern physics and chemistry. Teller's main contributions during this time were to explain diamagnetism in solids (1931), and to explain spectra of polyatomic molecules (1933). Both these problems required the application of quantum mechanics to systems involving many electrons.

When Hitler took power in 1933, Teller moved to Copenhagen. There he met George Gamow, a young Russian who had been the first to apply quantum mechanics to nuclear physics. In 1934 Gamow moved to George Washington University in Washington, D.C, and Teller moved to London with his newly wed wife, Mici. She was a childhood friend from Budapest who loved and sustained him through all his joys and sorrows, and remained by his side for 66 years until her death in 2000. In 1935 Gamow invited Teller to join him at George Washington University, and Mici, who had spent two years in America as a student, encouraged him to accept. During the years 1935-1939 that Gamow and Teller were together in Washington, they almost recreated the golden age of German physics in America. They found many of their European friends already in America, and quickly made new friends among the natives.

Gamow was four years younger than Heisenberg and almost as brilliant. But Gamow had no skill as an organizer and no desire to be a leader like Heisenberg. He produced brilliant new ideas at a rapid rate, and left it to Teller to work out the details. He also left to Teller the chores of administration, organizing meetings, and taking care of students. Teller worked happily with Gamow and also with other collaborators. The most important results of Teller's research during this time were the Gamow-Teller theory of weak interactions (1936) and the Jahn-Teller theory of polyatomic molecules with electrons in degenerate states (1937). The Gamow-Teller theory was in competition with an alternative theory due to Fermi. This was one of the very few occasions on which Fermi guessed wrong. The Gamow-Teller theory was Teller's first venture into nuclear physics. Twenty years later it became the basis for a unified theory of weak interactions.

One of Teller's friends in Washington was Merle Tuve, an American and a first-rate physicist who built particle accelerators and used them to do nuclear experiments at the Department of Terrestrial Magnetism of the Carnegie Institution. Tuve was one of the pioneers of accelerator physics, and made the first accurate measurements of the nuclear interaction between two protons. After Teller had spent a summer teaching in Chicago, the University of Chicago was thinking of offering him a permanent job. The Chicago physicists wrote to Tuve asking for his opinion of Teller. Tuve wrote back, "If you want a genius for your staff, don't take Teller, get Gamow. But geniuses are a dime a dozen. Teller is something much better. He helps everybody. He works on everybody's problem. He never gets into controversies or has trouble with anyone. He is by far your best choice." Teller quotes this letter in his memoirs and remarks, "I do believe it described me as I was during those happy years in Washington." He looked back on those years with nostalgia as a time when he could do science with everyone and be friends with everyone, before the bitter struggles over nuclear politics took him away from science and tore apart his friendships.

The record of Teller's publications confirms Tuve's statement. Teller in the first half of his life had an unusual gift for fruitful collaborations. I have taken 1952 as the point of division between the two halves of his life. In 1952 he moved from the University of Chicago to the new weapons laboratory that he founded at Lawrence Livermore National Laboratory in California. That was the year when he stopped being an academic scientist and became a full-time nuclear entrepreneur. In the bibliography of his technical publications there are 146 papers. Before 1952 he wrote 7 papers alone and 77 with collaborators. In that period most of his papers describe research done with one collaborator. Many of the leading physicists of that time appear as collaborators. After 1952 he wrote 42 papers alone and 20 with collaborators. In that period most of the papers are reviews or lectures, describing plans for the future or surveys of the past. The transition from a gregarious to a solitary pattern of intellectual life is painfully clear.

In January 1939 Gamow and Teller were hosts at the meeting of theoretical physicists, which was held annually at George Washington University. That year's meeting was supposed to be devoted to low-temperature physics. On the first morning of the meeting Gamow introduced Niels Bohr, who had just arrived on a ship from Denmark, and Bohr told the assembled physicists the news of the discovery of fission of uranium in Germany a month before. In the evening of the same day Merle Tuve invited everyone to his laboratory to see a demonstration of the intense bursts of ionization produced by uranium fission in a Geiger counter. The age of nuclear energy had arrived, and Teller was involved in it from the first day.

In February 1939 Teller's friend Leo Szilard called him from New York to announce that he had found abundant secondary neutrons emitted in uranium fission. This meant that an explosive nuclear chain reaction was certainly possible. In March 1939 an informal strategy meeting was held in Princeton. Present were Bohr, Wheeler, Wigner, Weisskopf, Szilard, and Teller. One American, one Dane, one Austrian, and three Hungarians. Two decisions were made; first to keep further discoveries about fission secret so far as possible, second to try to bring the situation to the attention of responsible people in the American government.

In June 1939 Teller moved from Washington to Columbia University to help Fermi and Szilard with their project to build the first nuclear reactor. In New York a few weeks before the outbreak of World War II, Heisenberg came to visit Teller. He was on his way back to Germany from a lecture tour in America. He had many offers of jobs in America and could easily have stayed. Teller asked him why he was going back to a country that was clearly headed for disaster. Heisenberg replied, "Even if my brother steals a silver spoon, he is still my brother." Teller understood that nothing he could say would cause Heisenberg to change his mind.

A few days later Szilard, who could not drive a car, came to see Teller and asked him for a ride. Szilard had written a letter to President Roosevelt informing him of the discovery of fission and the possibility of nuclear bombs. The letter asked the President to set up a channel of communication between the government and the physicists working on nuclear chain reactions in America. Szilard's plan was to persuade Einstein to sign the letter. Teller was needed as a chauffeur to bring Szilard and the letter to Einstein's summer home on Long Island. Einstein signed the letter, and Szilard successfully delivered it to Roosevelt. As a result, an official Advisory Committee on Uranium was established, and the bureaucratic machinery that later grew into the Manhattan Project slowly began to grind.

Teller worked on nuclear energy from 1939 to 1945: two years at Columbia University helping Fermi design the first nuclear reactor, two years at the Metallurgical Laboratory in Chicago helping to design the Hanford plutonium production reactors, and two years at Los Alamos National Laboratory working on bombs. In all three places he worked on a variety of projects. His wide knowledge of physics and chemistry made him useful as a liaison between different parts of the enterprise. The one thing that he could not and would not do was to sit down and do precise theoretical calculations. His thesis work, calculating the states of the hydrogen molecule ion, had given him a lifelong distaste for lengthy calculations. At Los Alamos this brought him into collision with Hans Bethe, the head of the Theoretical Division, who was Teller's boss. Bethe asked him to do a massive calculation of the physics and hydrodynamics of an imploding bomb. Teller refused, saying that if he tried to do such a calculation he would not make any useful contribution to the war effort. Teller's friendship with Bethe never recovered from this disagreement. Oppenheimer moved Teller out of Bethe's division and made him leader of an independent group. After that, Teller reported directly to Oppenheimer, and Oppenheimer kept him busy with a variety of assignments more suited to his temperament. Teller enjoyed working for Oppenheimer and considered him an excellent director.

During the wartime years Teller worked only intermittently on hydrogen bombs. This work started in the summer of 1942 when Oppenheimer held a meeting in Berkeley to explore the possibilities. The meeting concluded that if a fission bomb could be made to work, it could probably be used to ignite a hydrogen bomb. After the meeting Teller found reasons why the ignition would not work. He became seriously interested in the problem and continued to think about it. During his two years at Los Alamos he spent about one-third of his time working on hydrogen bombs. The result of his efforts was a very sketchy design called the Classical Super. The question whether the Classical Super would work could only be decided by massive calculations, using electronic computers that did not yet exist. There matters stood from 1945 to 1950.

From 1946 to 1952 Teller was a professor at the University of Chicago. He enjoyed the return to academic life and especially enjoyed interacting with a brilliant bunch of students, including Chen Ning Yang, Tsung Dao Lee, Marshall Rosenbluth, and Marvin Goldberger. Two of his closest friends, Enrico Fermi and Maria Mayer, were colleagues. During these years he worked with Fermi on the capture of negative mesons in matter (1947), with Mayer on the origin of the chemical elements (1949), and with Robert Richtmyer on the origin of cosmic rays (1949). I met Teller for the first time in March 1949 when I gave a colloquium in Chicago with Fermi and Teller sitting side by side in the front row. I spoke about the new theories of quantum electrodynamics. I made some very polite remarks about Schwinger's theory and then explained why Feynman's theory was better. As soon as I finished my talk, Teller asked a question and answered it himself. "What would you think of a man who cried, 'There is no God but Allah, and Mohammed is his prophet,' and then at once drank down a great tankard of wine? I would consider him a very sensible fellow." Afterward I was able to meet with Teller alone and he talked happily about all the things he was doing.

I quote now from a letter that I wrote to my parents in England, dated March 11, 1949.

Teller to me has always been an enigma. He has done all kinds of interesting things in physics, but never the same thing for long, and he seems to do physics for fun rather than for glory. However, during the last few years there have been reports that he has been engaged in perfecting the most fiendish engines of destruction; and I have always wondered how such a man could do such things. In Chicago I found without difficulty the answer. I started a long argument with him about political questions, and it appears that he is an ardent supporter of the 'World Government' movement, an organization which preaches salvation in the form of a world government, to be set up in the near future with or without Russia, and to have sovereign powers over the economic and social policies of its member nations. Teller evidently finds this faith soothing to his conscience; he preaches it with great charm and intelligence; all the same, I feel that he is a good example of the saying that no man is so dangerous as an idealist.

In the same letter there is a passage describing the community of physicists in Chicago.

The most striking thing about all these people, and also their wives whom I met as I went from house to house and from family to family, is how happy they seem to be. All of them say they have never found any place on earth so pleasant to be in as Chicago. There seems to be an exceptionally free and easy atmosphere, rather like Cornell, and with the added advantages of a metropolitan city.

These were the golden years of physics in Chicago, when Fermi was king and Teller was his court jester. Teller enjoyed those years to the full. But during those same years he could not stop thinking about the question that he had left unanswered when he left Los Alamos in 1946. Could a hydrogen bomb be made to work? In June 1949 he returned to Los Alamos to continue his lonely effort to understand what Nature allows us to do. In August the first Russian nuclear bomb was tested, and in January 1950 President Truman announced that work on the "so-called hydrogen or super bomb" would continue. After the President's announcement Teller wrote to Maria Mayer from Los Alamos. "Whatever help and whatever advice I can get from you—I need it. Not because I feel subjectively that I must have help, but because I know objectively that we are in a situation in which any sane person must and does throw up his hands and only the crazy ones keep going."

In 1950 electronic computers were able to simulate in a rough fashion the Classical Super design for a hydrogen bomb and showed that it did not work. George Gamow drew a famous cartoon of Teller trying to set fire to a wet piece of rock with a match. But to Teller the downfall of the Classical Super came as a liberation. For eight years his thoughts had been fixed on the Classical Super, which required deuterium to burn at low density, so that radiation could escape from the burning region and not come to thermal equilibrium with the matter. The idea was to achieve a runaway burn, with the temperature of the matter remaining much higher than the temperature of the radiation. The computers showed that runaway burn did not work. So Teller started to look seriously at the opposite situation, with deuterium at high density and the radiation trapped in thermal equilibrium with the matter.

Teller quickly found that at high density, deuterium could burn well in thermal equilibrium. From that point it was a short step to design an arrangement by which a fission bomb could compress deuterium to high density and then ignite it. Teller's colleague Stanislas Ulam at Los Alamos thought of a similar arrangement at the same time, and so the idea became known as the Teller-Ulam design. It was successfully tested in 1952 and has been the basis for American hydrogen bombs ever since. Andrei Sakharov had the same idea in 1954, and it quickly became the basis for Russian hydrogen bombs too. Many years later Teller and Sakharov met. They did not agree about political questions but expressed a deep respect for each other. Sakharov remarked in his memoirs that Teller's treatment at the hands of his American colleagues was "unfair and even ignoble."

In 1951 Teller returned briefly to his academic life in Chicago, but in 1952 he moved permanently to the Livermore laboratory, a brand-new weapons laboratory that his friend Ernest Lawrence had organized in California to give some competition to Los Alamos. He stayed at Livermore for 23 years, attracted a brilliant group of young collaborators, and saw the laboratory quickly rise to become an equal partner with Los Alamos in weapons development and in many other enterprises. Livermore was more adventurous than Los Alamos and more willing to try out crazy ideas. A much larger fraction of Livermore bomb tests failed, but Teller considered failed tests a badge of honor rather than a disgrace. In the end the Livermore-designed weapons proved to be as rugged and reliable as those designed at Los Alamos.

Soon after Teller moved to Livermore he was invited to testify at the Oppenheimer security hearings in Washington. At the hearings he was asked whether he considered Oppenheimer to be a security risk, and answered, "Yes." For this the majority of physicists, including many of his friends, never forgave him. The estrangement caused Teller tremendous grief. The community of physicists was split in two, and Teller became a symbol of the division. At the time when this happened I was puzzled and shocked by the violence of the reaction against Teller. To me it seemed that the main question was whether the security rules should be applied impartially to famous people and unknown people alike. It was a question of fairness. If any unknown person had behaved as Oppenheimer behaved, telling a lie to a security officer about an incident that involved possible spying, he would certainly have been denied clearance.

The question was whether Oppenheimer, because he was famous, should be treated differently. Should there be different rules for peasants and princes? This was a question concerning which reasonable people could disagree. I tended to agree with Teller that the rules ought to be impartial. And I saw no reason why people who disagreed with him should condemn him for speaking his mind. Teller's estrangement from the community of physicists became worse when three of his closest friends, Enrico Fermi, John von Neumann, and Ernest Lawrence, happened to die prematurely within a few years after the Oppenheimer hearings. Each of them died in his fifties and should have remained vigorously active for at least another 20 years. The loss of all three made Teller even more isolated as he started his new life at Livermore.

In the summer of 1956 I had one of my happiest experiences, working with Teller on the design of a safe nuclear reactor. Teller's friend Frederick de Hoffmann, a young physicist from Los Alamos, had started a company called General Atomic in San Diego to manufacture reactors for civilian use. Teller and I came for the summer with a group of physicists and chemists and engineers to help the company get started. Teller had been saying for many years that the essential problem for public acceptance of nuclear power was safety. He proposed that General Atomic should start by building a spectacularly safe reactor. His definition of safe was that you could give the reactor to a bunch of children to play with and be sure that they would not get hurt. Safety must be guaranteed by the laws of nature and not by engineered safeguards. For three months Teller and I argued furiously about the design. Every day Teller would think of some brilliant new idea and the rest of us would do calculations to show why it would not work. Finally we found a scheme that worked and used it to design a small reactor called TRIGA, short for Training, Research, and Isotope-production, General Atomic.

The TRIGA was designed, built, licensed, and sold within two years. The company sold 75 of them, mostly to hospitals for making short-lived isotopes, and they have never run into any safety problems. Teller and I had hoped that big power reactors using the TRIGA design could give rise to a nuclear power industry without safety problems. Unfortunately, the nuclear power industry was stuck with designs borrowed from the submarine-propulsion reactor program of Admiral Rickover, and never considered the TRIGA design as a serious competitor. Many years later Teller and his colleagues at Livermore developed designs for safe nuclear power reactors that could be buried deep underground, operated with a single loading of fuel for 50 years, never refueled, and never unloaded. Teller remained always hopeful that nuclear power would one day be so safe that the public would finally accept it.

Teller pushed hard to develop at Livermore other programs besides weapons development. He started a very successful educational program informally known as Teller Tech, which brought graduate students to the University of California campus at Davis. The students were enrolled in the College of Engineering at Davis and received Ph.D. degrees in applied science from Davis, but spent half their time at Livermore. Courses were taught by leading scientists at Davis and at Livermore. Teller enjoyed doing his share of the teaching. Many of the graduates remained at Livermore as members of the staff, while others went on to distinguished careers in universities and in industry.

Roughly one-half of the Livermore budget went into weapons. In addition, there was a large program to build controlled fusion reactors, both magnetic and inertial. There was a program to develop a supersonic nuclear ramjet that could fly nonstop around the world at low altitude. And there were two projects that were particularly dear to Teller's heart, the PLOWSHARE program to use nuclear explosions for peaceful purposes, and the strategic defense program to shoot down enemy missiles using X-ray lasers and brilliant pebbles. The PLOWSHARE program aimed to use nuclear explosions to excavate large masses of dirt or rock cheaply, the main purpose being to create artificial harbors and canals. To minimize the contamination of the landscape by radioactive fallout, the PLOWSHARE experts designed bombs whose explosive yield came mostly from fusion and as little as possible from fission. The X-ray laser was a device that could convert a substantial fraction of the energy of a fission bomb into a collimated beam of X rays. It was supposed to kill missiles a long way away by firing X rays at them with extreme accuracy. The brilliant pebble was a small interceptor rocket that was supposed to kill a missile by direct impact.

In the end neither the PLOWSHARE program nor the strategic defense program fulfilled Teller's hopes. None of the places that were candidates for PLOWSHARE excavations welcomed the idea with enthusiasm. Nobody had any urgent need for new harbors and canals, and as environmental regulations became more stringent the chance that any PLOWSHARE project would ever be approved became increasingly remote. Livermore's proposals for strategic defense also ran into difficulties. The X-ray laser was designed to destroy missiles in the boost phase while they were still accelerating with rocket power, but the X rays could not penetrate any considerable depth of atmosphere. As a result, the missiles could defeat the defense by accelerating more rapidly and shortening the boost phase. The brilliant pebbles were supposed to weigh a couple of pounds and turned out to weigh a couple of hundred pounds. Extravagantly large numbers of them would be required to be sure of having one at the right place and time to intercept a missile. However, the Strategic Defense Initiative that President Reagan started in 1983 embodied some of the Livermore proposals, and Teller gave it strong support.

After the Strategic Defense Initiative had spent a lot of money and accomplished very little, Teller and I went together to the Pentagon to talk with General Abrahamson, who was then running the program. Teller and I agreed that strategic defense was in principle a good idea, and that secrecy was in principle a bad idea. The SDI was a technically flawed program whose failures were concealed by excessive secrecy. Teller and I went to the general to tell him that the only way to make SDI technically effective was to abolish the secrecy and bring it out into the open. If the program were open, it might receive the expert criticism and the influx of new ideas from the outside that it desperately needed. Teller delivered the message with his usual eloquence, and the general responded by saying that of course he agreed with us, and he would be removing the secrecy within a few weeks. Needless to say, nothing of the kind ever happened. Teller remained publicly supportive of SDI but privately furious at the general for deceiving us.

In 1975 Teller retired from Livermore and became a senior fellow at the Hoover Institution on the campus of Stanford University. Here he spent the sunset years of his life, in close touch with the work of the laboratory at Livermore, writing books, and giving lectures, politically active to the end, still fighting for strategic defense and nuclear energy. At the end of his memoirs is a chapter titled "Homecoming," describing his seven visits to Hungary between 1990 and 1996. In Hungary he felt immediately at home after an absence of 54 years. He had never stopped speaking Hungarian with his wife, so that he remained fluent in the language. He was welcomed not only as a national hero but as a long lost brother. He was as proud of Hungary as Hungary was proud of him. His homecoming gave his life the happy ending that was denied to him in America.

SELECTED BIBLIOGRAPHY

1930

Über das Wasserstoffmolekülion. *Z. Phys.* 61:458-480.

1931

Der Diamagnetismus von freien Elektronen. *Z. Phys.* 67:311-319.

1933

With G. Herzberg. Schwingungsstruktur der Elektronenübergänge bei mehratomigen Molekülen. *Z. Phys. Chem.* 21:410-446.

1936

With G. Gamow. Selection rules for the beta-disintegration. *Phys. Rev.* 49:895-899.

1937

With H. A. Jahn. Stability of polyatomic molecules in degenerate electronic states. I. Orbital degeneracy. *Proc. R. Soc. A* 161:220-235.

1947

With E. Fermi. The capture of negative mesotrons in matter. *Phys. Rev.* 72:399-408.

1949

With M. G. Mayer. On the origin of elements. *Phys. Rev.* 75:1226-1231.

With R. D. Richtmyer. On the origin of cosmic rays. *Phys. Rev.* 75:1729-1731.

2001

With J. L. Shoolery. *Memoirs: A Twentieth-Century Journey in Science and Politics.* Cambridge: Perseus.

Anthony Turkevich

ANTHONY L. TURKEVICH

July 23, 1916–September 7, 2002

BY R. STEPHEN BERRY, ROBERT N. CLAYTON, GEORGE A. COWAN, AND THANASIS E. ECONOMOU

ANTHONY LEONID ("TONY") TURKEVICH WAS born in New York City in 1916, one of three children of a Russian Orthodox clergyman, Leonid Turkevich, who became head of the entire Russian Orthodox Church in North America and Japan. Tony went to Dartmouth College for his undergraduate studies, completing his B.A. in 1937. From there he went to Princeton, working with J. Y. Beach on structures of small molecules for his Ph.D., which he received in 1940. Turkevich then went to Robert Mulliken in the Department of Physics at the University of Chicago as a research assistant, studying molecular spectroscopy. He also worked on the radiochemistry of fission products.

Soon after the outbreak of World War II Tony joined the Manhattan Project as one of its youngest scientists. In 1942 he worked at Columbia and the next year went to the Metallurgical Laboratory at the University of Chicago where he stayed until 1945, when he moved on to Los Alamos. During that period Turkevich worked closely with Charles P. Smyth and Enrico Fermi. At Chicago he studied the separation of uranium isotopes by gaseous diffusion of the volatile uranium hexafluoride. He also studied the radiochemistry of reactor products, including plutonium produced by neutron capture in uranium. At Los Alamos he participated in the first test of a nuclear bomb, at Alamogordo in July 1945. Turkevich made accurate estimates of the energy released in that explosion—rather more accurate than the estimate Fermi made by measuring how far the blast wave carried bits of torn paper.

After the Alamogordo test, Turkevich moved to the theory group under Edward Teller to investigate thermonuclear reactions and the potential of fusion. In particular, he worked with Nicholas Metropolis and Stanley Frankel, using the ENIAC computer to demonstrate the feasibility of a fusion bomb. This was one of several stimuli that led to the development of the Monte Carlo random-selection statistical computational method (so named by Metropolis, but actually invented independently and much used in the 1930s by Enrico Fermi but not published then). Tony worked closely with Nick Metropolis to develop and apply the Monte Carlo method. This method, in turn, was the one Tony used in the next-generation computer, the MANIAC, at Los Alamos in 1952 to study cascades of nuclear processes.

In 1946 he became an academic scientist, officially a chemist, when he returned to the University of Chicago's Department of Chemistry as an assistant professor. One of Tony's distinctive characteristics as a scientist was the way physicists thought of him as a nuclear physicist, and the chemists considered him a chemist. The boundaries were irrelevant to him; the challenges of the problems were what mattered. At Los Alamos he worked closely with many people who also had associations with the University of Chicago: Enrico Fermi, Herbert Anderson, Richard Garwin, Willard Libby, Harold Agnew, Murray Gell-Mann, and others. He collaborated frequently with George Cowan; to quote George:

He introduced a kind of nuclear science at Los Alamos that seamlessly merged chemistry and physics. It was a happy marriage. Tony was an invaluable asset who returned whenever he chose, particularly during those summers when he wasn't diverted by some other overriding commitment. I quickly learned to go to him with my most intractable problems.

It was Tony Turkevich who, with S. Katcoff, proposed using atmospheric sampling as a way to monitor nuclear explosions. An early application was the determination of the atmospheric concentration of the radioactive isotope krypton-85 as a means to measure the number of fissions produced in reactors and atmospheric nuclear bomb tests. This proposal was written in a letter from Tony to Philip Morrison in July 1946, but that letter and the subsequent history of the monitoring of atmospheric radioactive krypton came into the literature only in an article by Tony, Lester Winsberg, Howard Flotow, and Richard Adams, published in 1997 in the *Proceedings of the National Academy of Sciences*, after it was finally declassified. There had been two internal reports in 1949 and 1951 on which the article was based. This article demonstrates how the concentration of atmospheric krypton-85 increased in proportion to the number of fissions in reprocessed fuel elements and nuclear tests. Krypton-85 is the fingerprint gaseous fission product that is released when fuel rods are dissolved and processed. Air samples from 1934 showed no krypton-85 with its (roughly) 10-year half-life. Samples taken in the 1945-1950 period show significant levels of this isotope, which could only be related to nuclear reactors and explosions. The research also showed the effectiveness of global circulation and mixing in the atmosphere.

In 1948 Tony was promoted to associate professor, and in September of the same year he married Ireene ("Renee"), his lifetime partner. In 1954 he became professor. Throughout the 1950s he carried on his research at the university and worked steadily at Los Alamos as well. A significant part of the latter activity involved classified studies. Much of that work was in response to Soviet nuclear technology. The first Soviet atomic bomb test, in 1949, brought Los Alamos back to a near wartime state with six- and seven-day work weeks to develop and test a thermonuclear weapon. The first crash tests were in Nevada, and the next stage moved quickly to Eniwetok atoll. Tony and George Cowan worked on that project. Then George and Tony went off to see the crucial Greenhouse George test. George was the first thermonuclear fusion device, designed largely by Richard Garwin in the spring of 1952. Tony was also continuing to implement what had become a growing intelligence program to monitor Russian production of plutonium by observing the changes of krypton-85 concentrations in the northern hemisphere. The Chicago campus became a center for this activity.

Los Alamos was involved in measuring other Soviet emissions and evaluating reports of atmospheric and seismic signals, mostly but not exclusively from nuclear tests. Tony consulted with George Cowan and Rod Spence, members of the Bethe Panel that regularly met, examined data, and reported to intelligence authorities in Washington. The U.S. tests provided unique opportunities for scientific discovery, including the production of new heavy elements and other multiple neutron capture products in the Ivy Mike device. Discoveries made this way were not announced until tight secrecy requirements were relaxed. The group of collaborators included scientists at Chicago and Berkeley.

That "nuclear weapons club" of scientists involved with weapons intelligence expanded to include the British. However, there were limitations on what information could be exchanged. Tony's official role in this activity was formally as consultant, but of course his involvement was very deep.

The activities were heavily compartmentalized. The group that met to evaluate Soviet plutonium production from the isotope measurements had regular meetings at Patrick Air Force Base in Florida for several years. George Cowan, who chaired the meetings in the early seventies, recalls that they ate well at the officer's club across the street from their meeting center, and would regularly visit the best local citrus orchards to collect loads of tree-ripe fruit to carry home.

Tony spent the academic year 1958-1959 at CERN in Geneva and was at Orsay, France, the following year. During that year at CERN he was a member and a very important expert adviser of the U.S. delegation to the Conferences on Nuclear Test Suspension in Geneva.

While he was an assistant professor he and Enrico Fermi wrote a seminal but unpublished paper, "Thermonuclear Reactions in the First Hour of an Expanding Universe." Although officially unpublished, it was quoted in detail by R. A. Alpher and Robert C. Herman in their paper in *Reviews of Modern Physics* in 1950 and cited by D. Ter Haar in the same issue. This was the first detailed analysis of how elements were produced in the Big Bang, an analysis that took into account all the reactions that involve neutrons, protons, deuterons, tritons, and both helium isotopes but require less energy than the breakup of the deuteron. There were 28 reactions in all, a remarkable task at that time; fortunately, several of them could be neglected. A principal result was the evolution of the composition of the early Universe: Following the first 500 seconds, when deuterium, tritium, and the helium isotopes were forming rapidly, the composition they found was almost 75 percent hydrogen and 25 percent helium after the neutrons decay. The results implied significantly more helium and heavy hydrogen than is found now, presumably because these were consumed in later nucleosynthesis reactions.

During the 1960s Tony began another collaboration with George Cowan, who had become interested in time-of-flight neutron spectrometry studies at Columbia in John Dunning's group. Specifically, the idea was to use the single huge pulse of neutrons from a nuclear explosion in a weapons test to detect and measure epithermal neutron fission resonances in uranium-235. A long vacuum pipe led a fraction of the pulse to a collecting wheel of metallic uranium-235 that spun past a thin slit in a heavy neutron shield. From the recovered wheel they were able to isolate fission products from individual epithermal resonances and demonstrate that fission symmetry varied with spin at each resolved resonance. Tony was a major coauthor of a series of papers in *Physical Review* that reported these results. The time-of-flight technique later became widely applied by physicists and chemists who used photographic and then electronic detectors with good time resolution to take the place of those early radiochemical analyses of exposed metal.

Tony kept his association with Los Alamos and with his friends there. He came regularly in summers, and bought a house near the Cowans. They would go swimming on weekend mornings in the neighborhood pool. When he stopped to rest, Tony would sometimes softly sing Russian liturgical music. The discussions covered almost everything including science and politics, but never religion. Tony always enjoyed food and eating. The Cowan garden grew beets; Renee, Tony's wife, was skilled at making them into a proper Russian borscht that they all enjoyed together under crystalline blue New Mexico evening skies.

In keeping with the spirit of the Monte Carlo method, Los Alamos was also a site for a regular poker game for a striking group of scientists. In addition to Tony himself, regular participants were Nick Metropolis, Stan Ulam, Carson Mark, Berndt Matthias, James Tuck, Paul Stein, Foster Evans, Rod Spence, Roger Lazarus, and occasionally Edward Teller. Teller writes of those games in his memoirs. It was a group that came for the company and the process of playing; like most of the group, Tony was neither a big loser nor a big winner. A quotation from George Cowan is appropriate here: “Fortunately for the majority, there was also a variable sprinkling of those who liked to bet to inside straights and three card flushes.”

In the late 1970s Tony became a visiting laboratory senior fellow, a position that gave him complete freedom to work on whatever he liked at Los Alamos. He was a close associate and friend of the resident senior fellows. That group began a series of conversations about forming a new kind of science center that would work at the boundaries between traditional sciences, particularly at the boundaries of natural and social sciences. Among that group were Herbert Anderson and Nick Metropolis, both of whom had moved to Los Alamos from the University of Chicago. That concept crystallized in a short time into the Santa Fe Institute. Tony, as one of its founders, presented a paper titled “Reconstructing the Past Through Chemistry” at its organizing meeting in 1984.

He was elected to the National Academy of Sciences in 1967. Consistent with his faculty appointment in the Chemistry Department at Chicago, he joined Section 14.

Tony was made James Franck Distinguished Service Professor in 1970. He retired in 1986 but remained active in research and in departmental activities. In a tradition long sustained at Chicago Tony was both an experimentalist and a theorist. Because his experimental research studied or used nuclear processes, he chose to publish in the *Physical Review,* rather than the more “chemical” journals, but this was irrelevant to his important role and influence in the Chemistry Department (to which we shall return).

One of us referred to Tony as the University of Chicago's analytical chemist after he accomplished one of his most widely known achievements. (The Chemistry Department had no courses in analytical chemistry and nobody on the faculty who was officially classed as an analytical chemist.) In 1961 Tony proposed to NASA that it would be possible to do a remote analysis of the elemental composition of lunar soil and rock on the first missions to the Moon. The unmanned Surveyor project came into being formally in 1960. It was a very daring project, using the first hydrogen-fueled rocket as its upper stage, to follow the launch with an Atlas. Then the 2-ton Surveyor itself was made to land softly on the lunar surface, with controllable retrorockets to achieve that landing. The scientific payload was only 100 pounds. *Surveyor I* sent to Earth the second set of pictures taken on the Moon; the Soviet *Luna 9* had sent back the first pictures four months earlier. However the Surveyor pictures had much better resolution. The second Surveyor mission was lost in a midcourse tumble. *Surveyor III* landed safely with a controllable digging device that it used to make trenches. *Surveyor IV* failed just before touchdown. But then *Surveyors V, VI,* and *VII* were successful, and all three carried Tony's analytical devices: foremost, an alpha-particle backscattering instrument that not only gave us the first complete elemental composition of the lunar surface but also showed that the Moon is a differentiated body, not a homogeneous conglomerate such as a giant chondrite, which was the prevailing view at that time. *Surveyor V*, landing on September 11, 1967, at Mare Tranquillitatis, showed that there are basalt-like materials—essentially cold lava on the lunar surface. The results stirred considerable skepticism, but the results from later expeditions, notably *Apollo 11*, gave undeniable confirmation. Then *Surveyor VI*, which had landed on November 10, 1967, at Sinus Medii site, gave very similar results. *Surveyor VII,* which landed January 10, 1968, this time at highlands near crater Tycho, was perhaps the most remarkable insofar as it was successfully repaired remotely when the backscatter instrument became stuck during deployment and was freed by commands from Earth 250,000 miles away. The other remarkable thing of the Surveyor analyses was the finding of a very high titanium level in the lunar material, comparable to the basalts on Earth.

The Surveyor missions and their findings laid the necessary groundwork for the subsequent Apollo manned missions. These were the first that returned lunar material to Earth for study here. In fact, in 1970 the astronauts of *Apollo 12* found *Surveyor III* undisturbed, with a small pile of lunar dirt that had deliberately been dropped on one of its footpads.

An unexpected byproduct of this work was Tony's development of an analytical device to detect lead in paint, at the time its poisonous character was being recognized.

That set of lunar studies was the beginning of Tony's growing interest and activity in planetary and space studies. He became more and more involved in unmanned spacecraft missions, always with deep scientific questions as his motivation. There were sometimes problems, as when U.S. scientists were not allowed to collaborate with Soviets on Soviet projects. However, Tony was able to find a way to do one of his most dramatic—but unsuccessful—experiments by participating with a group of Germans in experiments that would be carried on Soviet spacecraft. It was a very ingenious extension of the methods he had used to analyze the surface of the Moon.

The alpha backscattering technique was incorporated in the Soviet missions to the Martian moon Phobos on spacecraft *Phobos 1* and *Phobos 2* in 1988, and then on the Russian Mars-96 mission in 1996 and on NASA's *Pathfinder* in 1998. The Phobos missions and *Mars-96* all failed, a series of experiences that only someone with Tony's optimism and resilience could accept with any equanimity. One of the Phobos missions failed, it seems, because the command system did not have a "check before sending commands" structure, and a mistyped command reoriented the spacecraft so that it could no longer receive commands from Earth.

NASA's Pathfinder mission was a fine success. The spacecraft carrying the Alpha Proton X-ray Spectrometer (APXS)—a miniaturized version of his lunar instrument and in addition containing now an X-ray spectrometer—landed safely on the surface of Mars and sent back the first elemental analyses of the Martian rocks.

During the 1970s and 1980s Tony moved to studying exotic particles and the processes that would produce them. He looked for polyneutron systems, for example, in research that involved one of us (T.E.). He also looked for delta particles and for superheavy particles within nuclei. These were some of the most exotic and difficult experiments to carry out. His reasoning was that "everyone can do the easy ones."

In the 1980s and early 1990s Turkevich, Cowan, and Economou collaborated on a difficult series of experiments to measure the half-life of double beta decay in uranium-238. Despite early failures Tony persisted until they finally separated a countable sample of plutonium-238 from some old uranyl nitrate Tony found stored for many years at Chicago. The indicated half-life was $2x10^{21}$ years, an order of magnitude shorter than theory predicted. This result would suggest the existence of Majorana neutrinos, neutrinos with very small but nonzero masses. This was very much in conflict with the then-current view. When T. D. Lee and C. N. Yang's predicted parity violations were confirmed by the experiments of C. S. Wu and Leon Lederman and reported at a meeting in New York of the American Physical Society, Eugene Wigner gave an elegant talk that explained how these experiments "proved" that the neutrino could not have a finite rest mass. That specific experiment on double beta decay has never been repeated although it was attempted unsuccessfully (due to contaminating problems) by a French group. It would require a major new effort on a much larger, demonstrably uncontaminated sample of uranium to produce a generally accepted result, but Tony ran out of time in an attempt to reconfirm it. Other methods, however, have made the idea of neutrinos with small finite masses acceptable. The field has become very much more sophisticated, and finite-mass neutrinos are now considered quite reasonable objects.

Tony lunched regularly at the Quadrangle Club, which serves as the University of Chicago's faculty club. There were several *stammtischen* of which one was that of the physicists, where he normally sat. One could count on lively conversation with all the senior members of that department, with other regulars and occasionals from astronomy and chemistry. Visitors to the university, particularly to physics and astronomy, joined in these lunches. They were characterized by lively reviews of the latest results and new puzzles, some scientific and some just for fun.

Another daily routine was tea, otherwise the "coffee klatsch," in the afternoons in Nathan Sugarman's rooms in the Research Institutes' building. These were very popular and well attended not only by the institute faculty but by many visitors as well. Nathan would persuade one person or another to go to the blackboard to present their new results.

During the first few years after it had been announced, there were still people who considered cold fusion plausible.

An experiment came from Los Alamos that claimed to have detected small amounts of tritium that were presumed to be produced by the cold fusion process. When one of us (R.S.B.) asked Tony about that work, his typically insightful reply was, "Never believe a low-level tritium result from Los Alamos. Everything there is so contaminated that it always appears." He had superb taste in distinguishing the unbelievable (and wrong) from something very surprising but probably real. His own experimental results clearly showed that the cold fusion was not occurring.

After a few years in the university faculty housing development on 57th Street, the Turkevich family moved to the suburbs in Oak Brook. Tony would commute to the university or Fermilab by train and bus. If he and his colleagues worked late, they would retire to a soul food restaurant in Hyde Park and rejuvenate. Even these late night suppers of tired scientists were filled with lively discussion.

Tony and Renee had two children: a son, Leonid Turkevich, and a daughter, Darya Carney, both scientists, and three grandchildren, Elizabeth, Paul, and Julia Turkevich. Tony had two brothers, John, now deceased, who was on the chemistry faculty at Princeton, and Nicholas.

Eventually Tony was not able to live and work at the high altitude of Los Alamos and retired from his position there. He and Renee moved from Chicago to a retirement center in Virginia, where they lived until his death in 2002.

Tony carried out regular teaching of undergraduates and graduates until his retirement in 1986, at what was then the mandatory retirement age—not an absolutely strict condition, but Tony was quite ready at that time to devote his efforts entirely to research. It was at that time that he worked on the double-beta decay and its implications.

Tony received many awards, including the E. O. Lawrence Memorial Award from the Atomic Energy Commission in 1962, the Atoms for Peace Award of the Ford Foundation in 1969, an honorary doctorate of science from his alma mater Dartmouth College in 1971, the Award for Nuclear Applications of the American Chemical Society in 1972 and the Boris Pregel Award from the New York Academy of Sciences in 1988. He was a fellow of the American Academy of Arts and Sciences and of the American Physical Society.

Within his department Tony was one of the people who always insisted on maintaining traditional standards of excellence, especially when new appointments were being considered. He would ask the most penetrating questions, and look hardest at the qualities of originality and deep insight that he wanted to find in anyone appointed to a faculty position at Chicago. Praise from Tony was as strong a recommendation as a faculty candidate could receive.

Always gentle in manner and ready to listen to any well-thought arguments, he personified the Chicago style of a place where criticism is considered the best way to make things better.

SELECTED BIBLIOGRAPHY

1954

Evidence for Si^{32}, a long-lived beta emitter. *Phys. Rev.* 94:364.

1957

With H. Yamaguchi and G. W. Reed. Uranium and barium in stone meteorites. *Geochim. Cosmochim. Acta* 12:337-347.

1958

With N. Metropolis, R. Bivins, M. Storm, J. M. Miller, and G. Friedlander. Monte Carlo calculations on intranuclear cascades. I. Low-energy studies. *Phys. Rev.* 110:185-203.

With N. Metropolis, R. Bivins, M. Storm, J. M. Miller, and G. Friedlander. Monte Carlo calculations on intranuclear cascades. II. High-energy studies and pion processes. *Phys. Rev.* 110:204-219.

1960

With M. Lindner. Competition between fission and neutron emission in excited heavy nuclei. *Phys. Rev.* 119:1632-1643.

1961

Chemical analysis of surfaces by use of large-angle scattering of heavy charged particles. *Science* 134:672-674.

With G. A. Cowan and C. I. Browne. Symmetry of neutron-induced U^{235} fission at individual resonances. *Phys. Rev.* 122:1286-1294.

1966

With K. Knolle. Instrument for lunar surface chemical analysis. *Rev. Sci. Instrum.* 37:1681-1686.

1967

With E. J. Franzgrote and J. H. Patterson. Chemical analysis of the Moon at the Surveyor V landing site. *Science* 158:635-637.

1968

With J. H. Patterson and E. J. Franzgrote. Chemical analysis of the Moon at the Surveyor VI landing site: Preliminary results. *Science* 160:1108-1110.

With E. J. Franzgrote and J. H. Patterson. Chemical analysis of the Moon at the Surveyor VII landing site: Preliminary results. *Science* 162:117-118.

1969

With E. J. Franzgrote and J. H. Patterson. Chemical composition of the lunar surface in Mare Tranquillitatis. *Science* 165:277-279.

1970

With J. H. Patterson, E. J. Franzgrote, K. P. Sowinski, and T. E. Economou. Alpha radioactivity of the lunar surface at the landing sites of Surveyors V, VI, and VII. *Science* 167:1722-1724.

With H. R. Heydegger. Radioactivity induced in Apollo 11 lunar surface material by solar flare protons. *Science* 168:575-576.

With J. H. Patterson, E. J. Franzgrote, T. E. Economou, and K. P. Sowinski. Chemical composition of the lunar surface in a Terra region near the Crater Tycho. *Science* 168:825-828.

1972

Comparison of the analytical results from the surveyor, Apollo, and Luna missions. *Earth Moon Planets* 5:411-421.

1973

With T. E. Economou, W. A. Anderson, and E. M. Blume. Heavy elements in surface materials: Determination by alpha particle scattering. *Science* 181:156-158.

Average chemical composition of the lunar surface. *Earth Moon Planets* 8:365-367.

The chemical analysis of the lunar surface on Surveyor. *Bull. Atom. Sci.* 29:27-34.

1975

High energy nuclear reactions in our planetary system. *AIP Conf. Proc.* 26(1):351-364.

1977

With J. R. Cadieux, J. Warren, T. Economou, J. La Rosa, and H. R. Heydegger. Search for particle-bound polyneutron systems. *Phys. Rev. Lett.* 38:1129-1131.

1984

With K. Wielgoz and T. E. Economou. Searching for supermassive Cahn-Glashow particles. *Phys. Rev. D* 30:1876-1880.

1988

With T. E. Economou. In situ chemical analyses of extraterrestrial bodies. Lunar and Planetary Institute Workshop on Mars Sample Return Science, pp. 73-74 (SEE N89-18288 10-91). Washington, D. C.: National Academies Press.

1991

With T. E. Economou and G. A. Cowan. Double-beta decay of ^{238}U. *Phys. Rev. Lett.* 67:3211-3214.

1997

With L. Winsberg, H. Flotow, and R. M. Adams. The radioactivity of atmospheric krypton in 1949-1950. *Proc. Natl. Acad. Sci. U. S.* A. 94:7807-7810.