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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 88

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HENRY NATHANIEL ANDREWS, JR.

June 15, 1910—March 3, 2002

BY TOM L. PHILLIPS

HENRY N. ANDREWS JR. WAS an outstanding pioneer in North American paleobotany during the twentieth century. His explorations of past plant life, especially in the structure, development, and reproductive biology of Devonian and Carboniferous plants, provided benchmark foundations for paleoecological and evolutionary studies. Andrews is noted in part for the discovery of determinate growth in lepidodendrid trees (1958), seminal interpretations of early seed structure and the evolutionary origin of the integument (1963), his advocacy of the significance of seed ferns in gymnosperm evolution toward flowering plants (1948, 1966), and the exploration and evolutionary studies of Devonian plants from the high Canadian Arctic, Maritime Canada, Maine, and West Virginia (1984).

Andrews (1951) recognized early the significance of coal-ball studies in North America. During his years at Washington University (1935-1964) and at the Missouri Botanical Garden (1947-1964) in St. Louis, Henry Andrews contributed, as did his many students, a sustained series of fossil plant studies generally entitled "Contributions to Our Knowledge of American Carboniferous Floras," published in the *Annals of the Missouri Botanical Garden*. The anatomically based studies from coal-ball concretions encompassed every major group of vascular plants in the Pennsylvanian-age coal swamps, often presenting a first modern description and assessment of their significance (Gensel, 2002).

Andrews's (1947) *Ancient Plants and the World They Lived In* conveyed a broad agenda of research interest early in his career and his now classic paleobotanical text *Studies in Paleobotany* (1961) has inspired generations of students to follow in his footsteps. Andrews (1955, 1970) also compiled and published two volumes of the *Index to Generic Names of Fossil Plants* as part of his work with the U.S. Geological Survey. These herculean efforts were major contributions for all of paleobotany and, in turn, scholarly works that drew most of the early paleobotanical literature into Andrews's hands.

At the University of Connecticut at Storrs (1964-1975), Henry Andrews's research shifted to the Devonian, from West Virginia to Ellesmere Island, and along with his students and other colleagues, provided fundamental insight into the nature of early land plants. Each contribution is noted for the splendid reconstruction of plants. Andrews (1970) also prepared a comprehensive account on fossil ferns for the *Traité de Paleobotanique*. The pioneering Devonian studies with former student Patricia G. Gensel ultimately led after retirement to the benchmark contribution *Plant Life in the Devonian* (1984).

Henry Nathaniel Andrews Jr. was born in Melrose, Massachusetts, on June 15, 1910, the son of Henry N. Andrews, lawyer and trust officer with the First National Bank of Boston, and Florence M. Hollings Andrews, housewife. Reared as a New Englander, Andrews enjoyed a congenial and comfortable home environment with parents sympathetic to his early interests in natural history despite their lack of scientific training. His father was a good practical gardener in his spare time and one of Andrews's earliest recollections was helping him with a small victory garden during World War I. Early on, Andrews began collecting plants (a small herbarium), studying them, and hiking in the White Mountains of New Hampshire. He always found it necessary to devote some time to working with his hands, mainly woodworking, which he ascribed to his Swedish paternal grandfather, a machinist and inventor of sorts, and his Yorkshire maternal grandfather, a cabinetmaker.

In high school Andrews built and victoriously raced a small pram, which exemplified masterful boat building (Mamay, 1975). His outdoorsmanship and athletic abilities translated into many activities with friends. His father is said to have reminisced that "Junior was always a leader, and without ever saying a word." A more apt characterization is not likely (Mamay, 1975, p. 4). Andrews graduated from Melrose High School in 1928, and in an attempt to get better prepared for college, spent a year at New Hampton School near the family farm in Laconia, New Hampshire, from which he graduated again in 1929. He then attended Northeastern University for a year. Andrews was searching for a field of interest, and at that time he decided that he did not want to be an engineer. He transferred to the Massachusetts Institute of Technology, majored in food technology, and received a B.S. in 1934.

At MIT Andrews was particularly influenced by Professor Bernard Procter, a food technologist and his major advisor, and by Professor Hervey Shimer, the great authority on invertebrate index fossils. Proctor was much respected as a teacher, and kindly permitted Andrews to substitute a paleontology course for a "less desirable requirement." Andrews wrote: "This probably was the turning point in my career. I loved the fossils and, like many others, I loved Shimer" (Andrews, 1975, p. 2).

Upon graduation from MIT in the days of the Great Depression, Andrews asked his father for partial support to spend a year at the University of Massachusetts to study with Professor Ray E. Torrey, "one of the great botanical teachers of his time" (1980, p. 229). Andrews wrote the author of this memoir, "I wanted to study fossil plants but I knew that I must have a much better background in my knowledge of living plants. The year at the University of Massachusetts was a great one and toward the end of it I met Professor Edgar Anderson of the Missouri Botanical Garden in St. Louis, who offered me a teaching assistantship at Washington University. I accepted and, officially, became the student of Professor Robert E. Woodson from whom I learned a great deal about living plants."

Andrews enjoyed hiking and mountain climbing, at least on a modest scale. He spent most of the winter vacation time when at MIT hiking in snow shoes in the White Mountains. This led to some climbing in the Rockies in the summers of 1932 and 1934, when he made general fossil collections, including some coniferous woods that he later studied under Torrey's direction. A colleague, Cortland Pearsall, shared the exploration of the Grand Tetons of Wyoming with Andrews, who attributed the fossil wood discoveries to his classmate. Andrews greatly valued the ability of colleagues to locate fossils in the field: real fossil hunters! His top three in such abilities were Pearsall; William H. Forbes, a geologist at the University of Maine; and Andrew E. Kasper Jr., a former student and paleobotanist at Rutgers University.

The 1935 move to St. Louis for graduate studies was the beginning of a 30-year Missouri home base for Henry Andrews, during which he would complete his M.S. in 1937 and his Ph.D. in 1939. In 1939 he married Elizabeth ("Lib") Claude Ham, a Missourian whom he had met and courted as a student at Washington University. Their home in Webster Groves, Missouri, was literally a Little New England and was celebrated for their memorable hospitality. This was home to the three Andrews children—Hollings, Henry III, and Nancy—except in the summers when the family returned to the paternal home farm near Laconia, New Hampshire, to be joined by Henry when his field excursions or other travels were completed.

Andrews's doctoral studies were initiated in England in 1937 when his advisor at Washington University shipped him off to Cambridge University to study under H. Hamshaw Thomas at Downing College, which was noted for its many leaders in paleobotany. When Andrews's arrival at Cambridge coincided with the departure of Dr. and Mrs. Thomas to the continent for a vacation, he was directed to the Natural History Museum in London for his research project on seed-fern wood anatomy. It was at the British Museum (Natural History) where Andrews received mentor-level guidance from F. Maurice Wonnacott, which was considered "the closest thing to a formal course in paleobotany that I have received" (1980, p. 150). The two became lifelong friends and enjoyed much field collecting then and later (Andrews, 1990). At Cambridge, Andrews was influenced by Thomas's tutorial style of asking questions and was aided by him in securing suitable research material. Perhaps because of the primitive lab equipment available, Andrews became aware of what was needed and later fabricated it in his own lab in Rebstock Hall on the Washington University campus. For Andrews the time at Cambridge and the associated museum visits proved memorable experiences, which inspired him both in his research goals and as a mentor in the coming years.

As a faculty member at Washington University, beginning in 1940, Andrews established a dynamic and productive research program and he also became a paleobotanist at the Missouri Botanical Garden (1947-1964). With the beginning of World War II his earliest graduate students went into military service; the next group of students, mainly veterans, was close to his own age. In between graduate student generations Andrews taught mathematics to service members and maintained his paleobotanical interests by writing his first book, *Ancient Plants and the World They Lived In* (1947). His interests in paleobotany were quite broad and, through his entertaining presentations, lent themselves to a broad audience. The chapter titles were, in part, agendas for future exploration and writing, including "Past Epochs of the Arctic" and "The Fossil Hunters." Regarding climate change and floristic distribution Andrews (1947, p. 249) wrote, "The great Carboniferous flora of North America is so similar to that of Europe that it is hardly conceivable to think of its origin as other than a continuous unit forest." About Alfred Wegener's "great unit land mass" Andrews shared, "On first thought this theory of continents drifting about may seem a bit fantastic, but there is a great mass of physical, geological, and biological evidence to support it" (1947, p. 250). From a practical standpoint the book conveyed how fieldwork was done mostly on foot, horseback, or with rides by the U.S. Mail Service, especially in the western United States. Andrews experienced the shift in technology to 2½ ton trucks, bulldozers, helicopters, DeHaviland Otters with oversized tires, and Lockheed Hercules transports.

By 1951, with an influx of veterans, the graduate training program in coal-ball studies had expanded, each specializing in different plant groups: Robert W. Baxter (pteridosperms), Sergius H. Mamay (ferns), Charles J. Felix (lepidodendrids), and Burton R. Anderson (calamites). Andrews set an admirable tradition by requiring that students publish their own theses. His own research included plants from every major group and in his review of American coal-ball floras Andrews (1951, p. 464) stated that such research "should ultimately be able to work out a very interesting picture of the sequence of Upper Carboniferous floras and contribute notably to an understanding of the evolution of certain pteridophytic groups and early seed plants".

Funding for coal-ball studies was almost nonexistent until the National Science Foundation was established in 1950. In turn, field trips to coal-ball localities became feasible, and the author shared some rather adventurous field trips with Andrews and his students (see Phillips et al., 1973; Phillips and Cross, 1995; Phillips and Gensel, 1995).

From the late 1930s Henry Andrews and James M. Schopf at the Illinois State Geological Survey—and still later at the U.S. Geological Survey Coal Geology Laboratory in Columbus, Ohio—were great friends and colleagues. Jim on numerous occasions passed along coal-ball specimens and locality data to Henry, asserting that he had more than his share of research projects. Both were ardent field trippers. Henry, his students, and their students were recipients of much assistance, and encouragement from Jim Schopf across the years. Another longtime friend and champion of Henry's research was Harold C. Bold, the distinguished phycologist. The first time I met Henry Andrews (on a field trip to clay pits in western Tennessee) Harold Bold with his class had driven out from Vanderbilt University to meet with Henry and his class. Bold, later at the University of Texas, visited Henry at Washington University several times and maintained that the needed plant evolutionary answers had to come from paleobotany.

Throughout most of Henry's academic employment he also served as an administrator. Early in his service at the Missouri Botanical Garden, Henry was an assistant to the director for about five years. At Washington University from the early 1950s to 1964 he was "the dean," an administrative title that uniquely marked the head of the Botany Department. At the University of Connecticut he was department head, first of Botany (1964-1967) and then the Systematics and Environmental Section (Biological Sciences Group) (1967-1970). When asked why he had served so many years in administration, Henry told me that the responsibility actually permitted better control of his own schedules and projected plans. Indeed, Henry was a notably successful leader of his faculty colleagues as well as a prized advocate for all the graduate students.

Henry developed the ability to swiftly move from departmental affairs to his teaching, research, and writing, with deliberate focus on each in their turn. His nonacademic activities were similarly welcomed, and these included a quiet early morning hour laying a brick walkway or tending to his garden before going to school. He made time for almost everything and everybody and usually seemed unhurried as he shifted focus from one activity to another. He was noted for his enthusiasm and self-discipline, but most of his colleagues and friends could not distinguish where one left off and the other began. Nevertheless, most of his co-researchers could sense about when Henry's focus on a research project, usually at manuscript stage, was about to shift to another research task.

A few years before Henry moved to the University of Connecticut his growing interests in Devonian land plants and wanderlust for the high Arctic were solidified by an exploratory grant from the Guggenheim Foundation. In his chapter on "Past Epochs of the Arctic" (1947, p. 239) this is the opening sentence: "If any single phase of man's activity through the past four or five thousand years is most indicative of his desire to break into the dark recesses of the unknown, to pit himself against Nature's most formidable forces, his adventures beyond the Arctic Circle must be considered as a likely candidate for first place."

During the summer field seasons of 1962 and 1963 Henry and I shared camp sites on Ellesmere Island in the Northwest Territories of Canada, exploring for such fossil plants as *Archaeopteris* (Andrews et al., 1965) and observing the Arctic tundra from glaciers to fiords. Andrews (1980, p. 271) later wrote, "I think one loves the Arctic very much or not at all," and he indeed did. The first trip yielded the *Archaeopteris* and taught us much about logistical needs. Henry's cooking and Lib's food planning more than met our needs. The second trip permitted further exploration but emphasized the needs for mechanical transport. Perhaps the highlight of the last trip was a diverted Royal Canadian Air Force Lockheed Hercules transport flight from Alert (northernmost station in North America) to Thule Air Force base in Greenland for an emergency landing. While there were many attendant uncertainties, Andrews was delighted to see Greenland's great continental ice sheet if only for 24 hours.

During these travels and field work Andrews was a superb leader, wise planner, congenial companion, and a very creative cook. He was an explorer-naturalist in his special milieu. Andrews (1980, pp. 271-272) did not return to the Arctic because "in the summer of 1964 I became involved in diggings in northern Maine and later in southeastern Canada, and these areas proved so highly productive that they occupied my time for the next ten years—and there is still much to be done there."

Productive field trips to the Upper Devonian in West Virginia also ensued for a decade (1965-1975) following James M. Schopf's discovery of *Archaeopteris* beds and his guidance of Andrews and Phillips to the Valley Head site. This resulted in reconstructional studies of *Rhacophyton* (1968; Cornet et al., 1976) and *Archaeopteris* (1972).

As an experienced outdoorsman Henry knew the risks of his sustained explorations; however, he was quite brave, self reliant, and usually wise in judgment. These traits along with stamina were dearly tested in the exploration of the Long Range Mountains of Newfoundland (Andrews et al., 1968a) when Henry, Francis M. Hueber, and Andrew E. Kasper found themselves in a misadventure. One mistake—leaving behind the main food supply box—led to others as the three became stranded for five days with few rations in the secluded krummholz amidst fog and pouring rain, which hampered searches from the air. Henry faulted his judgment in abandoning the base camp and attempting to walk out. Those who read their account may draw other lessons about his determination.

Henry's international travels, particularly in the company of his wife, Lib, had exceptionally positive influences among their new friends and guests—often occasioned by the cooking of Lib as well as by Henry. Andrews (1980, p. 167) wrote, "I believe my wife contributed much with her culinary talents as she has done in several other countries. Good food helps considerably in establishing good international relations."

Henry had many personality traits that endeared him to his family, students, and colleagues as well as some total strangers. He was a good listener and there was an alertness of concentration that conveyed to the source an appreciative reception of information, whether it was directions to a fossil plant locality, advice on academic matters, or explanations for some minor fiasco about his cat and squirrels. His generosity of time and companionship indicated to me that he regarded the best way to share experiences with his students and colleagues was to be with them and learn with them.

At Storrs, Connecticut, from 1964 to 1975 Henry and his family were an integral part of the community, and there was more than a decade of happy productive challenges. It was not until 1964 that he devoted research efforts entirely to Devonian plants. This developed, in part, by his being invited by James M. Schopf to assist Ely Mencher in a paleobotanical-stratigraphical investigation in northern Maine, with a great deal of assistance by University of Maine geologist William Forbes. This was the beginning of more than a decade of extensive fieldwork by Henry and his graduate students, with the aid of William Forbes.

The summers were spent exploring the Lower to Middle Devonian of Baxter Park, Maine, and later digging along the northern New Brunswick coast and the Gaspé Bay. Out of these repeated collecting trips came outstanding specimens and a flood of reconstructional and evolutionary studies on trimerophytes, such as *Psilophyton* (Andrews et al., 1968b; Kasper et al., 1974); *Pertica* (Kasper and Andrews, 1972; Granoff et al., 1976; Doran et al., 1978); lycopsid-like plants, such as *Kaulangiophyton* (Gensel et al., 1969); plants of uncertain affinity, including *Chaleuria* (1974) with incipient heterospory; *Oocampsa* (Andrews et al., 1975); and the enigmatic *Renalia* (Gensel, 1976). A major review of the Early Devonian flora in Maine was published after Andrews's retirement (1977). This series of studies ultimately led Gensel and Andrews (1984) to develop their *Plant Life in the Devonian*, a benchmark synthesis and summation.

In 1975 Henry and Lib moved from Storrs back to the family farm in New Hampshire. There—among house renovation, hobbies in picture framing, gathering photographs of paleobotanists, carpentry, and gardening—Henry set about his most enjoyable project upon retirement: "to reveal the fossil hunters of the past three centuries in their best light" (1980, p. 396). As a student of paleobotany for more than 40 years, Henry had fully embraced the breadth of geologic time and paleobotanical topics from the Precambrian origins of life through angiosperm radiations, as well as the myriad of paleobotanists he knew or had known personally or had corresponded with or discovered in his research, readings, or travel. Indeed, Henry's travels and courteous correspondence with paleobotanists around the world were born of a shared kindred spirit with paleobotanists of the present as well as the past.

Henry Andrews was elected to the National Academy of Sciences in 1975. By this time Andrews had received many awards for his contributions in paleobotanical research and service. Andrews was a member of Sigma Xi and Phi Beta Kappa, serving as chapter president both at Washington University and the University of Connecticut. He was a fellow of the Geological Society of America and the American Association for the Advancement of Science. He was twice a John Simon Guggenheim Memorial Foundation fellow (1950-1951, 1958-1959). On the second fellowship he worked two months in Belgium with Suzanne Leclercq at Liege, and then continued his research at Oslo, Stockholm, Moscow, and Leningrad. Andrews also received a special grant for 1960-1965 from the Guggenheim Foundation for exploratory research, such as his Arctic expeditions.

He was a Fulbright lecturer at Poona University, India (1960-1961), and was selected for the Sir Albert Charles Seward Memorial Lecture at the Birbal Sahni Institute of Palaeobotany in Lucknow, India (Andrews, 1961). He was also a special lecturer for Oklahoma State University in Ethiopia (May 1961), and a National Science Foundation postdoctoral fellow in Sweden (1964-1965), working at the Natural History Museum in Stockholm.

Henry was a member of the Botanical Society of America and recipient of the Merit Award in 1966 for his pioneering studies of late Paleozoic land plants. In 1977 he received an award from the Paleobotanical Section of the Botanical Society of America for his "Distinguished Service to the Paleobotanical Section and Outstanding Contributions to American Paleobotany." He was a member of the International Organization of Palaeobotany and served as secretary and vice-president. He was also an honorary member of the Palaeobotanical Society of India and a charter member of the Connecticut Academy of Science and Engineering (Gensel, 2002).

Upon retirement Andrews taught for one semester in 1976 at the University of Aarhus in Denmark. In his community he was very active in volunteer work and received numerous awards and special recognition for conservation work with the Lakes Region Conservation Trust, in Merrimack County, and at a long-term reconstruction project of the Canterbury Shaker Village. Henry had many longtime friends who shared in these projects, and he often mentioned them in his correspondence.

Henry N. Andrews was a twentieth-century naturalist, explorer, educator, administrator, historian, and a consummate fossil hunter and writer. His original research along with that of several generations of his students helped define the priorities of research in the late Paleozoic and provided the stepping stones to synthesis and summaries found in his books. Henry Andrews was one of the most positive and inspirational influences in paleobotany in the twentieth century and hopefully in the present with his legions of articles sharing the adventures of fossil hunting and celebrating the accomplishments of his colleagues of the past and present. He was a generous scholar in all aspects of his profession.

Graduate students who were mentored by Andrews were Eloise Pannell, Lee W. Lenz, Robert W. Baxter, Sergius H. Mamay, Burton R. Anderson, Charles J. Felix, Karen Alt Grant, William H. Murdy, R. Bradley Ewart, Tom L. Phillips, Shripad N. Agashe, Kuldeep Rao, Andrew E. Kasper Jr., Judith E. Skog, Bruce Cornet, Jeffry Doran, Jeffrey Granoff, and Patricia G. Gensel.

Henry N. Andrews Jr., Professor Emeritus at the University of Connecticut died on March 3, 2002, in Concord, New Hampshire, at the age of 91. He had moved from his farm to the Peabody Home in Franklin, New Hampshire, about a year and a half before his death.

The first biography of Henry N. Andrews Jr. was provided by Sergius H. Mamay, anonymously at his request, for a special issue of the *Review of Palaeobotany and Palynology* in 1975 upon the occasion of Henry's retirement. The second biography (Phillips and Gensel, 1995) drew upon Henry's middle and last groups of graduate students who spent much field time with him. The third "biography" is intercalated in Henry's outstanding history of paleobotany and paleobotanists as seen in their best light, entitled *The Fossil Hunters*. His book says more about Henry's view of the field of paleobotany, his colleagues of all centuries, his philosophy of science, his opinions on many aspects of nature and those who share interests in nature's research and enjoyment than can be shared herein. When Henry submitted his manuscript on *The Fossil Hunters* to Cornell University Press, a chief response was that there ought to be an epilogue, which he provided, emphasizing human value. I suspect that Henry viewed each chapter of his book as epilogue in design. I have drawn heavily on these resources as well as 40 years of correspondence with him.

I WISH TO THANK Patricia G. Gensel, University of North Carolina at Chapel Hill, and Karl J. Niklas, Cornell University, and Nancy Andrews Adams of Sanbornton, New Hampshire, for their assistance and suggestions.

The photograph of Henry Andrews in 1973 is courtesy of his son Hollings T. Andrews. The photograph was originally published in the Review of Paleobotany and Palynology, Volume 20, Henry N. Andrews Jr.: A biographical sketch, pp. 3-11, copyright 1975, used with permission of Elsevier.

REFERENCES

Andrews, H. N. 1961. Plant riddles in the rocks—their contribution to evolutionary studies. Sir Albert Charles Seward Memorial Lecture. Birbal Sahni Institute of Palaeobotany, Lucknow, India.

Andrews, H. N. 1975. Unpublished biographical notes provided by Membership Office, National Academy of Sciences, p. 1-5.

Andrews, H. N. 1990. Frederick Maurice Wonnacott 1902-1990. *Int. Organ. Palaeobot. Newsl.* 43:6-8.

Andrews, H. N., P. G. Gensel, and A. E. Kasper. 1975. A new fossil plant of probable intermediate affinities (Trimerophyte-Progymnosperm). *Can. J. Bot.* 53:1719-1728.

Andrews, H. N., F. M. Hueber, and A. E. Kasper Jr. 1968a. The Long Range Mountains of Newfoundland. *Appalachia* Dec.:288-299.

Andrews, H. N., A. E. Kasper, and E. Mencher. 1968b. *Psilophyton forbesii*, a new Devonian plant from northern Maine. *Torrey Bot. Club Bull.* 95:1-11.

Andrews, H. N., T. L. Phillips, and N. W. Radforth. 1965. Paleobotanical studies in Arctic Canada. I. *Archaeopteris* from Ellesmere Island. *Can. J. Bot.* 43:545-556.

Cornet, B., T. L. Phillips, and H. N. Andrews. 1976. The morphology and variation in *Rhacophyton ceratangium* from the Upper Devonian and its bearing on frond evolution. *Palaeontographica* 158B:105-129.

Doran, J. B., P. G. Gensel, and H. N. Andrews. 1978. New occurrences of trimerophytes from the Devonian of eastern Canada. *Can. J. Bot.* 56:3052-3068.

Gensel, P. 2002. Henry N. Andrews, Jr., Paleobotanist, Educator and Explorer, 1910-2002. *Plant Sci. Bull.* 48:48-49.

Gensel, P. G. 1976. *Renalia hueberi*, a new plant from the Lower Devonian of Gaspé. *Rev. Palaeobot. Palynol.* 22:19-37.

Gensel, P. G., A. E. Kasper, and H. N. Andrews. 1969. *Kaulangiophyton*, a new genus of plants from the Devonian of Maine. *Torrey Bot. Club Bull.* 96:265-276.

Granoff, J. A., P. G. Gensel, and H. N. Andrews. 1976. A new species of *Pertica* from the Devonian of eastern Canada. *Palaeontographica* 155B:119-128.

Kasper, A. E., and H. N. Andrews. 1972. *Pertica*, a new genus of Devonian plants from northern Maine. *Am. J. Bot.* 59:897-911.

Kasper, A. E., H. N. Andrews, and W. Forbes. 1974. New fertile species of *Psilophyton* from the Devonian of Maine. *Am. J. Bot.* 61:339-359.

Mamay, S. H. 1975. Henry N. Andrews, Jr.: A biographical sketch. *Rev. Palaeobot. Palynol.* 20:3-11.

Phillips, T. L., and A. T. Cross. 1995. Early and mid-twentieth century coal-ball studies in North America. In *Historical Perspective of Early Twentieth Century Paleobotany in North America*, eds. P. C. Lyons, E. D. Morey, and R. H. Wagner, Geological Society of America Memoir 185, pp. 314-339. Boulder, Colo.: Geological Society of America.

Phillips, T. L., and P. G. Gensel. 1995. Henry Nathaniel Andrews, Jr. (1910-): Paleobotanist, educator, and explorer. In *Historical Perspective of Early Twentieth Century Paleobotany in North America*, eds. P. C. Lyons, E. D. Morey, and R. H. Wagner, Geological Society of America Memoir 185, pp. 245-254. Boulder, Colo.: Geological Society of America.

Phillips, T. L., H. W. Pfefferkorn, and R. A. Peppers. 1973. Development of Paleobotany in the Illinois Basin. Illinois State Geological Survey Circular 480. Urbana, Illinois: Illinois State Geological Survey.

SELECTED BIBLIOGRAPHY

1940

On the stelar anatomy of the pteridosperms with particular reference to the secondary wood. *Ann. Mo. Bot. Gard.* 27:51-118.

1941

Dichophyllum moorei and certain associated seeds. *Ann. Mo. Bot. Gard.* 28:375-384.

1945

Contributions to our knowledge of American Carboniferous floras. VII. Some pteridosperm stems from Iowa. *Ann. Mo. Bot. Gard.* 32:323-360.

1947

With E. M. Kern. The Idaho Tempskyas and associated fossil plants. *Ann. Mo. Bot. Gard.* 34:119-186.

Ancient Plants and the World They Lived In. Ithaca, N.Y.: Comstock Publishing.

1948

Some evolutionary trends in the pteridosperms. *Bot. Gaz.* 110:13-31.

1950

With S. H. Mamay. A contribution to our knowledge of the anatomy of *Botryopteris. Torrey Bot. Club Bull.* 77:462-494.

1951

American coal ball floras. *Bot. Rev.* 17:430-469.

1955

Some recent advances in morphological palaeobotany. *Phytomorphology* 5:372-393.

Index of Generic Names of Fossil Plants, 1820-1950. U.S. Geological Survey Bulletin No. 1013. Washington, DC: U.S. Geological Survey.

1958

With W. H. Murdy. *Lepidophloios*—and ontogeny in arborescent lycopods. *Am. J. Bot.* 45:552-560.

1959

Evolutionary trends in early vascular plants. *Cold Spring Harb. Sym.* 24:217-234.

1960

With S. Leclercq. *Calamophyton bicephalum*, a new species from the Middle Devonian of Belgium. *Ann. Mo. Bot. Gard.* 47:1-23.

1961

Studies in Paleobotany. New York: John Wiley.

1963

Early seed plants. *Science* 142:925-931.

1966

Some recent developments in our understanding of pteridophyte and early gymnosperm evolution. In *Plant Biology Today*, eds. W. A. Jensen and L. G. Kavaljian, pp. 114-145. Belmont, CA: Wadsworth Publishing.

1968

With T. L. Phillips. *Rhacophyton* from the Upper Devonian of West Virginia. *J. Linn. Soc. Lond.* 61:37-64.

1970

With C. A. Arnold, E. Boureau, J. Doubinger, and S. Leclercq. Filicophyta. In *Traité de Paleobotanique,* vol. 4, fasc. 1, ed. E. Boureau, pp. 17-406. Paris: Masson et Cie.

Index of Generic Names of Fossil Plants, 1820-1965. U.S. Geological Survey Bulletin No. 1300. Washington, DC: U.S. Geological Survey.

1972

With T. L. Phillips and P. G. Gensel. Two heterosporous species of *Archaeopteris* from the Upper Devonian of West Virginia. *Palaeontographica* 139B:47-71.

1974

With P. G. Gensel and W. H. Forbes. An apparently heterosporous plant from the Middle Devonian of New Brunswick. *Palaeontology* 17:387-408.

1977

With A. E. Kasper, W. H. Forbes, P. G. Gensel, and W. G. Chaloner. Early Devonian flora of the Trout Valley Formation of northern Maine. *Rev. Palaeobot. Palynol.* 23:255-285.

1980

The Fossil Hunters. Ithaca, N.Y.: Cornell University Press.

1984

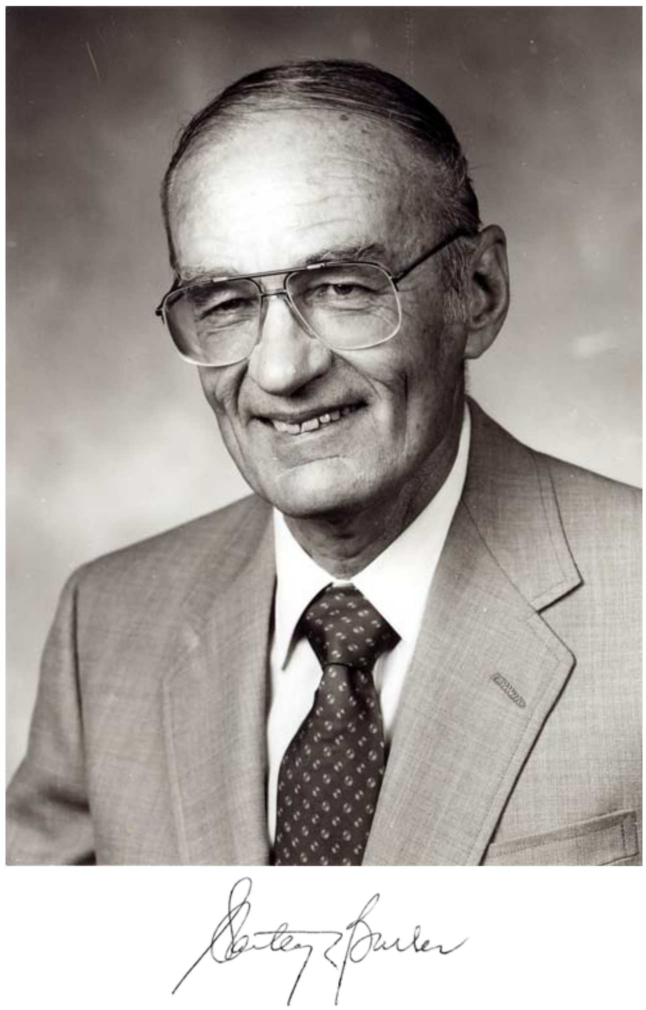
With P. G. Gensel. *Plant Life in the Devonian*. New York: Praeger.

1987

With P. G. Gensel. The early evolution of land plants. *Am. Sci.* 75:478-489.

1988

With A. E. Kasper, P. G. Gensel, and W. H. Forbes. Plant Paleontology in the state of Maine, a Review. In *C. T. Jackson 150th Anniversary Volume*, pp. 109-128. Augusta, ME: Maine Geological Survey.



STANLEY ARTHUR BARBER

March 29, 1921—December 12, 2002

BY W. R. GARDNER AND WILLIAM MCFEE

STANLEY A. BARBER, J. B. Petersen Distinguished Professor of Agronomy, was born on March 29, 1921, in Wolesey, Saskatchewan. He grew up on a wheat and dairy farm in that western part of rural Canada. As a youth he attended a one-room school that had an attendance ranging from 20 to 25 students. After passing the first three years of high school by correspondence, Barber skipped a year to help with the family farm. He completed his high school studies by driving 20 miles each day to a school that had only three teachers for the four grades—assisting with the family farm all the while. After graduation he remained at home for two more years before following in the footsteps of his two older brothers and enrolling at the University of Sasakatchewan. During his time at Saskatchewan, which coincied with World War II, his schedule alternated farming in the summer with studying and training with the University Officer Training Corps during the winter. Barber majored in agriculture, taking the most advanced courses offered. He took, for example, Physics II from Gerhard Hersberg, a German who had immigrated to Canada and who was later to receive a Nobel Prize. Barber received his B.S. degree with J. W. T. Spinks, later the president of the university, who had returned from his war effort eager to use the radioactive tracers to which he had been introduced while in the military. To our knowledge Barber conducted the first field studies with radioactive tracers as part of his M.S. studies. After completing the M.S. in 1947, Barber applied for a two-year research fellowship and, upon its receipt, elected to study with C. E. Marshall of the University of Missouri. Professor Marshall, known as a rigorous mentor, was one of the best known and most accomplished soil chemists in the United States. By coming to the United States, Barber followed in the footsteps of many Canadians—such as Philip Low—who chose to study agriculture in the USA, thus enriching the lives of his colleagues in the United States. He completed studies for his Ph.D. and was immediately (in 1949) hired by Prof. J. B. Peterson, also a well-known soil physicist and chemist, who had left Iowa State University to become department head at Purdue. From the most humble of beginnings Barber rose to become one of the best known and respected soil scientists in the world.

At Purdue, Barber was given wide latitude in the choice of specialty to follow. With his strong background in physics and chemistry he elected to study the uptake of nutrients by plants. Until this time, plant nutrition had been studied in nutrient solutions and through field trials that were analyzed statistically. The introduction of statistics by Fisher and others provided agricultural science with a powerful tool, but a tool that had its limitations. In studying plant nutrition by combining knowledge of plant physiology, chemistry, physics, and mathematics, Barber pursued a line of research that was to go far beyond statistical techniques. With the aid of some 55 graduate students and 30 visiting scientists he pushed the empirical understandings further and further aside and replaced them with an increasingly theoretical understanding of the mechanisms of nutrient uptake by plants. Barber created an elegant and sophisticated mathematical model for determining the rate of any nutrient's uptake by any plant in any type of soil.

THEORY

Barber's achievements can most easily be explained by considering the theory and the experiments separately, but it must be pointed out that every theoretical advance was both preceded and followed by experiments designed to revise and improve the theory. He started first with phosphorous (P) and potassium (K), which are found in almost any commercial fertilizer. Furthermore, soils around the world are often deficient in these nutrients. There is a second and important reason for the selection of these two nutrients. Potassium is relatively mobile and can move with water as it moves toward the plant roots in response to plant transpiration. Phosphorous, on the other hand, is strongly adsorbed by the clay minerals in the soil and can move only a short distance, much of it by diffusion. This means that a great deal of phosphorous can be held in a relatively small volume and can be available to the plant roots if they can find and proliferate through the zones of this adsorbed or "fixed" phosphorous. In soils there might be one, two, or as many as three different sites, each with a different binding energy.

Barber began by dividing the system of plant root uptake into three parts. The first is the amount of nutrients in the soil and how quickly they reached the soil surface. This was the soil surface phase. The second part is the group of forces that move the nutrient ions (electrically charged atoms or molecules) from the soil-water solution surrounding the plant root into the root themselves. This he called the "plant uptake kinetics." The third component of the system was the root diameter, length, and growth rate (which measures the way the root system changes in size and arrangement with time).

A soil system always surrounds soil particles (unless they are dry). Some nutrient ions are in that soil solution. The nutrients get from place to place by moving with the soil water. It has been shown that nutrients of this type, if applied in the spring, are not taken up until the water in which they reside is taken up by the roots and transpired by the plant leaves. The water that is pulled up through the soil by transpiration is replaced by water farther away from the roots. Nutrients close to the roots move by mass-flow with the water to the root surfaces. When nutrients, such as calcium and magnesium, are abundant in the soil solution they may often move faster than the plant can take them up, and they actually pile up in the soil surrounding the roots.

Other nutrients, especially those in short supply in the soil, move through the soil by diffusion or a random movement into regions where they are in lesser concentration. Next to the roots the concentration is reduced by ion or nutrient uptake and these are in turn replaced by this diffusion process. Nutrient ions move through the soil at different rates depending upon the geometry of the soil particles, so the diffusion coefficient is characterized by the soil water content and the soil texture.

Certain nutrient ions are adsorbed by the soil particles, as mentioned above. These adsorbing soil particles act like a storehouse for the ions thus adsorbed. The ability of the soil to give up stored quantities to replace those that are removed by the uptake by the plants is called the "buffering power" of the soil and is given a quantitative numerical value, b, in Barber's equations.

Next, Barber examined uptake by the plant roots themselves. He started with the simplest explanation to describe how this process is related to the concentration of the uptake of such ions as P and K. Plants take these ions in at special sites on the root surface. When nutrients are sufficiently numerous to fill all the uptake sites, they are said to saturate the mechanism, and the roots are taking in ions at their maximum rate. This fastest rate is labeled *Imax* in the model, which is now beginning to take shape. It will become the mathematical model that was the long-range goal of Barber's research. He assigned the symbol, Cmin, to the ion concentration at the root surface, which drops below the concentration that can be taken up by diffusion.

A third concentration is required in order to describe the uptake process. This process is like an adsorption process and another concentration term, *Km*, the nutrient concentration at which the plant exhibits the maximum uptake results. These parameters completely describe the uptake mechanism insofar as the root controls it.

The final part of Barber's model describes the roots themselves. Root diameter, length, and growth rate define the amount of root surface area available for nutrient uptake and increase as a plant grows. Barber calculated total root surface by measuring root radius and length to calculate the area of the roots, as though they were a long cylinder for which one could calculate the surface area by multiplying the circumference by the length. Fine roots take up ions faster than larger roots because they are better supplied by nutrients. This is caused by interference between adjacent roots so that the uptake by large roots tends to be more nearly one dimensional than small roots, which tend to be three dimensional. This competition is more serious with ions that are more mobile (such as nitrates) than ions that are adsorbed and less mobile (phosphorous). Generally, a longer root means more nutrient uptake because the number of sites for uptake tends to increase with increasing length. To account for this property Barber included a measurement not only of root length and radius but also how long a root is at the beginning of an experiment, by adding one more measurement, k, to describe how fast or how much a root grows with time.

Barber then had all the measurable parameters he needed to write two basic equations. The first—a second-order differential equation—described the change of concentration with time, which combined a term for diffusion and a term for mass-flow. This equation described the change of nutrient concentration in the soil at the root surface as a function of time. A second equation incorporated the plant properties and gave the uptake per unit length of root and gave the rate of uptake per unit rate of length.

$$\frac{\partial C_1}{\partial t} = \frac{1}{r}\frac{\partial}{\partial r}\left(rD_e\frac{\partial C_1}{\partial r} + \frac{r_0 v_0 C_1}{b} \right) \quad (1)$$

$$\text{Deb}\,\frac{\partial C_1}{\partial r} + v_0 C_1 = \frac{I_{max}\,(C_1 - C_{min})}{K_m = C_1 - C_{min}}\ ,\ r=r_0\,,\ t > 0 \quad (2)$$

In the equations the subscript, 0, refers to the initial condition, C is the concentration of the ion in question, K_m, characterizes the adsorption isotherm, and D_e is the effective or average diffusion coefficient and b is the buffer capacity. To account for the competition between roots, Barber used an iterative process starting with one section of root at the start of a plant growth. The equations must be solved simultaneously, and then this result must be used with the knowledge of the plant's growth rate to calculate the total uptake of the growing plant.

At that time, solving the equations Barber had developed was as difficult as their derivation. The task required the use of Purdue University's large mainframe computer, and it was difficult for anyone at another location to test or otherwise make use of the equations. It was the eventual development of newer and faster computers that finally made the equations accessible to anyone on any IBM-compatible microcomputer.

Other scientists had developed equations equivalent to individual components of Barber's equations, such as the flow of an ion or the diffusion to an individual root. It was Barber's almost daring combination of all the roots of a plant during growth while keeping in mind the movement in the soil that made his equation complete; however, it remained to test each component in turn and all of them together. This required a long series of experiments in the laboratory, greenhouse, and field.

EXPERIMENTS

Given the number of nutrients, both major and minor, and the wide range of adsorption sites and the permeabilities of the different types of soils, the testing of the model was no small chore. With ever more severe tests, this task occupied Barber's career for much of the rest of his time at Purdue. He was one of the first to use P^{32} to understand the uptake of fertilizer. He studied the potassium-calcium relationships in montromillonite group clays and in attapulgite. His next significant publication was a field study of the dependence of the effect upon the initial amount of soil phosphorous of an application of phosphorus placed in the plant row. This and similar studies began to explain the uptake of P by the plant and helped him begin the first states of his eventual model. By 1961 and 1962 Barber had published two papers that contributed to this understanding of the uptake of ions by plant roots followed by a concept of movement of ions to the plant root combing diffusion and mass-flow. This was followed immediately by papers in 1963 and 1966 of studies in situ that confirmed his ideas about diffusion and mass-flow. He expanded his confidence through two papers that compared the uptake of different cations by soy beans. These studies provided a critical test of the movement of cations and showed how these mechanisms, coupled with root interception, explained their limiting effect upon ion uptake from soils.

Having satisfied himself as to the quantitative measures of diffusion and mass-flow upon uptake, Barber then turned to studies aimed at exploring the effect of the plant roots themselves upon the uptake process. He produced a number of papers investigating this process, resulting in publication in 1970 of the physiological effectiveness of root systems followed by a comparison of root systems by the use of autoradiographic techniques. Soon after, he included the effect of pH changes that are induced by the plant at its root surface comparing ammonium, nitrate, and phosphorous uptake.

During the ensuing years Barber studied such variables as ammonium and nitrate uptake as influenced by the NH_4^+/NO_3 ratio, the uptake per unit of corn roots, and a method for characterizing the relation between nutrient concentration, the development of corn roots, and the effect of this development upon uptake.

By this time Barber had studied the uptake process from soil to the soil-root interface, and on into the plant itself. He was sufficiently confident that he had considered and quantified the major component of the uptake process that he began setting ever more severe tests of the model, such as those that he presented in a significant paper (1977). His confidence in this model was now so sufficient that he had branched out beyond nutrient uptake to the uptake of other ions and molecules, such as metals and, in general, almost any molecule. He capped his many years of study on ion uptake by publishing the definitive book on the subject in 1995.

Barber would be the first to agree that many details of his model are as yet not fully understood and require future study. Especially where the uptake mechanisms are concerned, it is very complex, but Barber gave a very good approximation. Barber's model is more that just a first approximation: it will be the scaffold upon which future advances will depend.

By any measure Barber's work reflects a prodigious effort maintained over a full career. To understand the significance of Barber's model one only need consider that though 11 parameters are involved, they are all measurable. Barber's model eliminates the need for conducting a large number of field experiments and can be used to predict the uptake of any nutrient by any plant. His sensitivity analyses of the model showed that root morphology and initial nutrient concentration in the soil solution had the greatest effects on nutrient uptake. The value of each parameter incorporates temperature, if necessary, which may have a significant effect upon water uptake. The basis for the model is firmly grounded in chemistry and physics and their interaction means that the experimentalist can focus on the parameters that are most essential.

By comparing the model predictions with six different species of plants having a wide variance in phosphate uptake with actual experiments, Barber showed excellent agreement between the two. Using the model, one can predict everything from the effect of a given root morphology, to soil properties, and to proper fertilizer placement.

HONORS AND AWARDS

Stanley Barber was a self-effacing scientist who pursued his field of science with an almost uncanny sense of the next experiment that needed to be done or the next term that needed to be included in a comprehensive model of ion uptake. He was appropriately the recipient of many awards and honors. These culminated in his receipt in 1986 of the Alexander von Humboldt Award in Hamburg, Germany, and election to the National Academy of Sciences in 1987. He served as the associate editor of five journals and was elected to the boards of the American Society of Agronomy, the Soil Science Society of America, and the International Soil Science Society. His honors also included election as a fellow of the Soil Science Society of America and the American Society of Agronomy, both in 1964. He was made an honorary member of the National Society by the National Fertilizer Solutions Association in 1978, and the University of Missouri honored him with its Alumni Citation of Merit Award in 1981. In 1983 Purdue gave him its Sigma Xi Research Award. In 1983 and 1984, respectively, the American Society of Agronomy gave him its Agronomic Research Award and the Agronomic Achievement Award. This was followed by the Bouyoucos Soil Science Distinguished Career Award in 1985. These are the most prestigious awards given by either society. He was given the Herbert Newby McCoy Award by Purdue University and an honorary doctor of laws degree and the Distinguished Graduate in Agriculture Award by the University of Saskatchewan, all in 1986. In 1987 he was awarded the Certificate of Distinction by the Purdue Alumni Association and the Gamma Sigma Delta International Award for Distinguished Service to Agriculture, and was the Canadian Industries Ltd. Distinguished Visiting Lecturer. He was also a fellow of the Indiana Academy of Sciences.

INFLUENCE ON THE FIELD OF SOIL SCIENCE

Stanley Barber's influence on soil science and its related fields, such as ecology and plant breeding, to name just two, is unparalleled by any of his peers. Barber provided a theory-based understanding of all the processes involved in nutrient uptake, while rendering superfluous the conduction of the uptake by plants from every soil and in every climate, as was heretofore the practice. His life's work taken as a whole is not only remarkable in its influence but is also especially courageous considering that understanding just one component of his model would be considered a major achievement. He was a scientist who was capable of working on the basic chemistry, physics, and mathematics while seeing the practical application for every parameter in his model. This enabled him to predict the outcome of experiments and explain why native plants were distributed as they were, based on differences in the nutrient availability of the soil material upon which they grew. Barber's work will be supplanted eventually by other scientists, but it is safe to say his work will always be recognized as a landmark in the progress of soil science and his name will be remembered and his work cited for generations to come.

Professor Barber was also a devoted family man, who with his wife, Marion, had two daughters and three grandchildren. His first love was his family, followed by his science and his many colleagues and friends. He was a gracious and kind man to all who met him, whether longtime friend or chance acquaintance. He loved doing puppetry, and enjoyed the traveling that his profession provided. His wife's death followed his by just over three months.

SELECTED BIBLIOGRAPHY

1946

With J. W. Spinks. Study of fertilizer uptake by using P^{32}. *J. Am. Chem. Soc.* 68:2748.

1951

With C. E. Marshall. Ionization of soils and soils colloids. *Soil Sci.* 72:373-385.

1958

Relation of fertilizer placement to nutrient and crop yield. Interaction of row phosphate and the soil level of phosphorous. *Agron. J.* 50:535-539.

1961

With J. M. Walker. Ion uptake by living plant roots. *Science* 133:881-882.

A diffusion and mass-flow concept of soil nutrient availability. *Soil Sci.* 93:39-49.

1963

With J. M. Walker and E. H. Vasey. Mechanisms for the movement of plant nutrients from the soil and fertilizer to the plant root. *J. Agr. Food Chem.* 11:204-207.

1966

With S. Oliver. An evaluation of the mechanisms governing the supply of Ca, Mg, K, and Na to soybean roots. *Soil Sci. Soc. Am. Proc.* 30:82-86.

The roles of root interception, mass-flow, and diffusion in regulating the uptake of ions by plants from soil. In *Limiting Steps in Ion Uptake by Plants from Soil.* International Atomic Energy Agency Technical Report No. 65, pp. 39-45. Vienna: IAEA.

1970

With C. D. Raper Jr. Rooting systems of soybeans. II. Physiological effectiveness as nutrient absorption surfaces. *Agron. J.* 62:585-588.

With P. G. Ozanne. Autoradiographic evidence for the differential effect of four plant species in altering the Ca content of the photosphere of soil. *Soil Sci. Soc. Am. Proc.* 34:635-637.

With D. Riley. Effect of ammonium and nitrate fertilization on phosphorous fertilization on phosphorous uptake as related to root induced pH changes at the root-soil interface. *Soil Sci. Soc. Am. Proc.* 35:301-306.

Effect of tillage practice on corn (*Zea mays L.*) root distribution and morphology. *Agron. J.* 63:724-726.

1973

With D. D. Warncke. Ammonium and nitrate uptake corn (*Zea mays L.*) as influenced by nitrate concentration and NH_4^+/NO_3 ratio. *Agron. J.* 65:950-953.

1974

With D. B. Mengel. Rate of nutrient uptake per unit of corn root under field conditions. *Agron. J.* 66:399-402.

With N. Claassen. A method for characterizing the relation between nutrient concentration and flux into roots of intact plants. *Plant Phys.* 54:564-568.

With D. B. Mengel. Development and distribution of the corn root system under field conditions. *Agron. J.* 66:341-344.

1977

A mathematical model to simulate metal uptake by plants growing in soil. In *Symposium Proceedings, 15th Hanford Life Sciences Symposium on Biological Implications of Metals in the Environment (Hanford, QA, Sept. 29-Oct. 1, 1975),* ed. Mary N. Hill, pp. 358-364. Springfield, Va.: USERDA, National Technical Information Service.

1978

Growth and nutrient uptake of soybeans under field conditions. *Agron. J.* 70:457-461

1979

With M. K. Schenk. Phosphate uptake by corn as affected by soil characteristics and root morphology. *Soil Sci. Soc. Am. J.* 43:880-883.

1980

With I. Anghinoni. Predicting the most efficient phosphate placement for corn. *Soil Sci. Soc. Am. J.* 44:1016-1020.

1981

With J. H. Cushman. Nitrogen uptake model for agronomic crops. In *Modeling Wastewater Renovation-Land Treatments,* ed. I. K. Iskander, pp. 382-409. New York: Wiley-Interscience.

1983

With M. Silberbush. Prediction of phosphorous and potassium by soybeans with a mechanistic mathematical model. *Soil Sci. Soc. Am. J.* 47:262-265.

With S. Itoh. Phosphorous uptake by six plant species as related to root hairs. *Agron. J.* 75:457-461.

1984

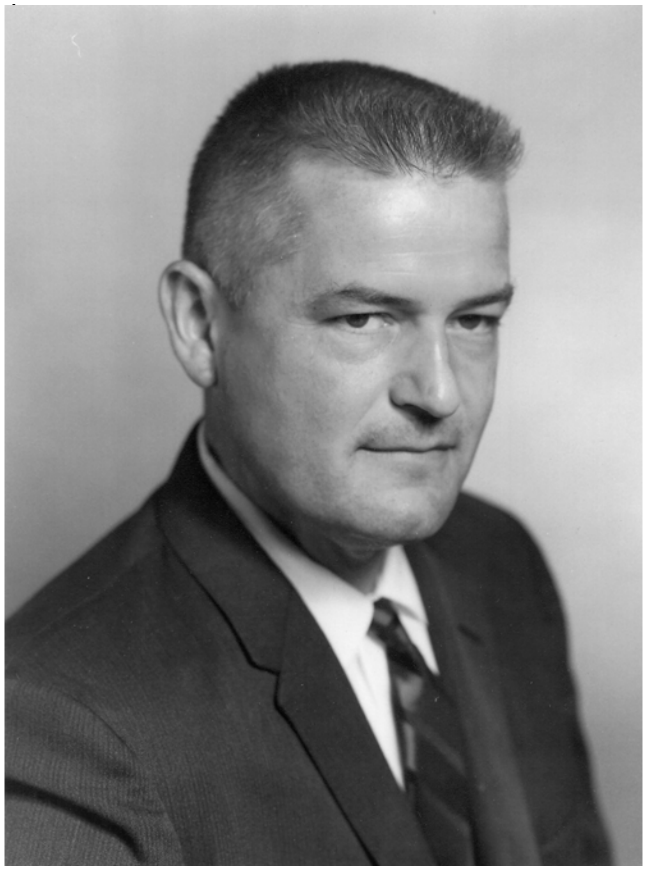
With M. C. Drew, L. R. Staker, and W. Jenkins. Changes in the kinetics of phosphate and potassium absorption in nutrient-deficient barley roots measured by depletion technique. *Planta* 160:490-499.

1992

With J. M. Kelly. Modeling magnesium, phosphorous, and potassium uptake by loblolly pine seedlings using a Barber-Cushman approach. *Plant Soil* 139:209-218.

1995

Soil Nutrient Availability: A Mechanistic Approach. 2nd ed. New York: Wiley.



David Bonner

DAVID MAHLON BONNER

May 15, 1916—May 2, 1964

BY MAARTEN J. CHRISPEELS

DAVID MAHLON BONNER'S short scientific career—he died at the age of 48—spanned the bloom period of *Neurospora* biochemical genetics and he was one of its main practitioners and contributors. He started life as a plant physiologist and became a biochemical geneticist working with *Neurospora crassa* after joining the group of George Beadle and Edward Tatum as a postdoctoral researcher at Stanford University. Initially he explored the use of *Neurospora* for biochemical investigations and identified intermediary steps in biochemical pathways. Finding that mutations that affect one enzyme are located on the same small segment of genetic material, he provided support for the "one gene, one enzyme" theory proposed by Beadle and Tatum in 1941. The nature of the genetic unit fascinated him: Was a genetic unit simple or complex? By analyzing 25 different mutants altered at the *td* (tryptophan desmolase) locus and a number of *td* revertants, he came to the conclusion that the genetic material controlling the formation of one enzyme represents a genetically indivisible unit, but admitted that (in 1955) it was still too early to decide whether this conclusion was fantasy or fact. His research group also found mutations that appeared to affect the rate at which an enzyme is formed, rather than its structure, and work by them and others then led to the realization that enzyme formation is regulated by repressors. After going to Yale (with Tatum) in 1942 he advanced through the ranks and became a professor of microbiology at the School of Medicine. In 1960 he was lured to San Diego to become the founding chair of the Department of Biology at the then newly established University of California, San Diego (UCSD). He had the major role in setting the direction of that department and in formulating a novel plan for integrating the teaching of the basic sciences into the curriculum of the new School of Medicine.

FAMILY MATTERS AND THE SOURCES OF HIS INSPIRATION

David Bonner was born on May 15, 1916, in Salt Lake City, Utah, the fourth child in a large family with seven children. His father, Walter D. Bonner, was head of the chemistry department at the University of Utah. His mother, Grace Gaylord, also studied chemistry at Nebraska Wesleyan University, where they were classmates, and graduated in 1906. She briefly taught chemistry at the secondary level. The family moved to Utah from Kingston, Ontario, the year before David was born. The Bonner siblings in addition to David were James (b. 1910), Lyman (b. 1912), Priscilla (b. 1914), Robert (b. 1917), Walter (b. 1919), and Francis (b. 1921). Five of the children received doctoral degrees; four of them became biochemists, two became physical chemists, and one (Robert) became an applied mathematician and computer specialist. The family lived in a semirural environment on the outskirts of Salt Lake City. These surroundings were chosen by the parents so the children could have the opportunity to experience the rewards of gardening and work in an agricultural setting. The homestead included a fully developed 2-acre orchard, and the responsibility for managing, maintaining, harvesting, and marketing fell to each of the boys in succession. Spraying of orchards was still done with lead arsenate in those days. Profits from the enterprise were applied to college tuitions. According to their brother Francis, it is likely that this intense agriculture experience influenced first James and then David and also Walter to become plant biologists. One might assume that a large family in Salt Lake City would be affiliated with the Mormon Church, but this was never the case. (All the details about family life were kindly supplied by Francis T. Bonner, the youngest brother.)

In 1929, when Dave was 13 years old, his father took a sabbatical leave at the California Institute of Technology in Pasadena. James and Lyman, the two older sons attended Caltech with tuition scholarships. It was an exciting time for the family because of the close contact with renowned Caltech scholars. For example, brother James was a research assistant for Theodosius Dobzhansky during the family's year in Pasadena. After returning to Salt Lake City and graduating from high school, David majored in chemistry at the University of Utah and received his honors A.B. degree in 1936. For his doctoral work he followed in the footsteps of his brothers James and Lyman who both obtained Ph.D.s from Caltech. Just as Dave moved to Caltech for graduate work, his brother James became a biology instructor there after spending a year abroad. A year after receiving his Ph.D. at Caltech, on August 2, 1941, Dave married Miriam Thatcher. Many years later they had two sons, Matthew (b. 1956) and Nicholas (b. 1958). Nicholas was afflicted with cerebral palsy and needed crutches to get around, but Dave and Miriam treated him like any other child.

DAVID AS PLANT PHYSIOLOGIST

In the late 1930s and early 1940s the Caltech Division of Biology was a hotbed not only for *Drosophila* genetics but also for plant physiology, having attracted several famous European plant biologists, including the Dutchmen Herman E. Dolk, Fritz W. Went, and Arie J. Haagen-Smit. Working in the Netherlands, Went had discovered the first plant growth hormone, which he named auxin. The Caltech group of plant physiologists, including Kenneth V. Thimann, was trying to find the chemical identity of auxin. They isolated indole-acetic acid from human urine and showed that it had auxin activity in the *Avena* coleoptile elongation test. Factors (chemicals) that affect the growth of plants either in situ or in vitro (organ culture) were then a major field of research. David Bonner's Ph.D. thesis with Arie Haagen-Smit dealt with leaf growth factors. He found that "adenine in the presence of potassium nitrate largely replaces the effect of crude pea diffusate in promoting leaf growth in excised pea embryos and in immature excised leaves. Adenine too exerts a marked positive effect upon the vegetative growth of plants in sand culture. . . . Adenine should, therefore, be included in the list of phytohormones." Much later, adenine derivatives called cytokinins were found to be plant growth hormones in the laboratory of Folke Skoog.

Between 1937 and 1943 David Bonner published a number of articles (I found at least seven) dealing with various plant growth factors. A paper by David as sole author, published in 1937 in *The Botanical Gazette,* was entitled "Activity of the Potassium Salt of Indole-Acetic Acid in the Avena Test."[1] In 1938 he published an article with his brother James that dealt with the effect of ascorbic acid on the growth of excised pea embryos.[2] After completing his Ph.D. in 1940, David was appointed as a research assistant at Caltech, and during this time he continued his work on plant growth factors.

David greatly enjoyed the out-of-doors and went on numerous outings, taking full advantage of the proximity of the Sierra Nevada and the Pacific Ocean. Fred Addicott, James Bonner's first graduate student, recalled a two-week camping trip with the two Bonner brothers (David and James) where David learned the fine points of trout fishing.

FROM NUTRITIONAL GROWTH FACTORS IN PLANTS TO AUXOTROPHIC MUTANTS IN *NEUROSPORA*

David is best known for his work on the chemical genetics of the bread mold *Neurospora crassa.* The switch from plants to molds occurred in 1942 when he became a research associate at Stanford University in the integrated research group of George Beadle, a geneticist who was then professor of biology, and Edward Tatum, a biochemist who had been a research associate and had just become an assistant professor. At Stanford, Beadle and Tatum were exploring the relationship between genes and metabolism. From 1937 to 1941 they concentrated on the biosynthesis of eye pigments in *Drosophila melanogaster*. Beadle visited Haagen-Smit's lab (where David was a Ph.D. student) to learn microchemical techniques to isolate and characterize chemical substances.[3] In 1941, as a result of a course on the nutrition of yeasts and fungi that Tatum had organized at Stanford, the group also started working on *Neurospora*. It had been shown that *Neurospora* could be grown on a defined medium and only required biotin. They used X-ray-irradiation-induced mutants of *Neurospora* that had specific nutritional deficiencies (auxotrophic mutants) to identify the genes associated with specific metabolic enzymes. In a landmark paper published in 1941[4] they showed that each nutritional deficiency was associated with a mutation in a single gene. George Beadle gave a memorable seminar about this work at Caltech.[5]

When David Bonner joined the group in 1942 it was but a small step to go from nutritional growth factors in plants to growth requirements of auxotrophic mutants in *Neurospora* and from indole acetic acid (a tryptophan derivative) in plants to tryptophan synthesis in *Neurospora* (1944). David also studied mutants of *Neurospora* requiring choline, isoleucine, valine, and anthranilic acid and in 1945 the group summarized its findings in a review article that was published in the *American Naturalist*.[6] They showed that their analysis of mutants made it possible to describe biosynthetic pathways. By all accounts these were exciting times for the Stanford research group: They were breaking new ground in understanding the connection between the genetic material and metabolism or biochemistry. In 1958 George Beadle and Edward Tatum shared the Nobel Prize in physiology or medicine with Joshua Lederberg. They were cited for their discovery that "genes act by regulating chemical events," work to which David Bonner contributed substantially while at Stanford. At Stanford, David was also engaged in isolating strains of *Penicillium notatum* that overproduce penicillin; this was a major project in the Beadle and Tatum group and was their contribution to the national war effort.

GOING TO YALE UNIVERSITY

In 1945 Edward Tatum accepted an appointment at Yale University in the newly named Department of Botany and Microbiology. Apparently, Edmund Sinnott, the chair of the Department of Botany and dean of the Graduate School at Yale at the time Tatum was hired, had the department's name changed to Botany and Microbiology to be welcoming to Tatum. David Bonner was appointed as a research associate in Tatum's group in 1946. A year later he became an associate professor in microbiology in the Medical School and the Graduate School. At Yale he continued his research on *Neurospora* mutants, exploring nicotinamide and nicotinic acid mutants. A talk presented at the "Symposium on Genes and Cytoplasm" held in Washington, D.C., during the centennial celebration of the American Academy of Arts and Sciences and published in *Science* (1948) chronicled the advances that had been made up to that point: (1) mutations were inherited as single genes; (2) mutants could be grouped in biochemical classes leading to the understanding of pathways; (3) mutants were available for the seven chromosomes of *Neurospora* (The chromosome number was determined by Barbara McClintock who later received the Nobel Prize for her work on transposons in maize.); and (4) extracts of some mutants could be shown to lack a specific activity, such as splitting lactose into galactose and glucose, or joining serine and indole to make tryptophan. These advances supported the one gene, one enzyme postulate, but instances in which several independent genes were shown to affect a single enzyme were troubling to the investigators.

BUILDING A RESEARCH GROUP AT YALE

In 1948 Ed Tatum returned to Stanford University and left Dave in charge of what remained of his research group, providing Dave with the opportunity to build his own group. In the late 1940s it was not yet known that the genetic material was DNA. Yanofsky recalled that Dave was committed to find "*the* best system" to understand the gene-enzyme relationship. Several young people soon joined his lab, including Otto Landman, Naomi Franklin, Gabriel Lester, William Jacoby, André Jagendorf, Elga Wasserman, and Charles Yanofsky. According to his own account[7], Yanofsky had applied both to Caltech to work with Beadle and to

Yale to work with Tatum. He ended up working with neither, because he was turned down by Caltech, and although admitted to Yale, Tatum had returned to Stanford. He decided to work with Bonner, who gave him the task of identifying the intermediates in niacin biosynthesis. This work led to the identification of quinolinic acid and verification of kynurenine as biosynthetic intermediates because they accumulated in different auxotrophic mutants. Using indole labeled with ^{15}N, Elga Wasserman showed that this was converted to niacin in a niacin auxotroph. Other studies by Chester Partridge in the lab showed the conversion of ^{15}N tryptophan to niacin. Since niacin is an important vitamin, they also explored the utilization of niacin by rats. For this body of work on the biosynthesis of niacin and the interrelationship between tryptophan and niacin, David Bonner received the Eli Lilly Award in Biological Chemistry in 1952. Shortly afterward *Chemical and Engineering News* did a special feature on David and his research. These were happy times for the Bonner group.

Much of the lab at this time was devoted to the further exploration of the one gene, one enzyme hypothesis. Some in the group were working on α-galactosidase in *Neurospora* (Otto Landman and Naomi Franklin), and Gabriel Lester worked on the same enzyme in *E. coli*. They were all looking for the best system. However, what was lacking to make real progress was an assay for a specific enzyme catalyzing a specific biochemical reaction postulated to occur on the basis of genetic analyses. In the third year of his dissertation research Charley Yanofsky turned his attention to tryptophan desmolase (now called tryptophan synthase), the enzyme that catalyzes the coupling of L-serine with indole to form L-tryptophan. An assay for this enzyme had been developed in the laboratory of W. W. Umbreit, and Yanofsky showed that two tryptophan-requiring mutants that could not use indole for growth lacked this enzyme activity. This discovery energized the entire Bonner lab to look for other mutants at this locus (*td*). Use of these mutants allowed them to show that all mutations inactivating this single enzyme appeared to be located in the same gene or genetic segment.[8] Joseph A. Roper had made the same discovery in *Aspergillus nidulans* at the same time.[9]

André Jagendorf, who joined the group as a graduate student in 1948, did the last work on plants with which Dave's name is associated. However, Dave was principally immersed in biochemical genetics of *Neurospora*, and most of Jagendorf's guidance on a project involving the effect of the synthetic auxin 2,4-D on root growth in cabbage seedlings came from Aubrey Naylor, then a young faculty member at Yale.

LIFE'S PLEASURES AND PAINS

In Connecticut David and Miriam lived out in the country, first in Woodbridge and later in Bethany, a small town of 3,000 people, where they purchased the house built by Henry and Mary ("Polly") Bunting. The house, long located on a dirt road, was a favorite gathering place of the grad students to play croquet and have barbecues. There was an apple orchard and small animals. André Jagendorf recalled that the Bonners kept two "bovines" named Porterhouse and Sirloin. Undoubtedly, David who thought of himself as a country boy, wanted to recreate the atmosphere of his own youth in Utah, when as a boy, he had been in charge of a cow on his parents little farm. It was always open-house at the Bonner home.

Miriam was not only a welcoming hostess but also the no-nonsense presence in Dave's lab. She was in charge of the *Neurospora* crosses and of the culture collection and coordinated the work of several other technicians, including Carol Yanofsky, Charley's wife. Grad students came back to the lab most evenings to hand-wash the dishes and discuss science. A large sign over the front door proclaimed it to be the Bonner Institute of Fundamental Research. A major advantage of being a student in this lab was that the congeniality of the group was accompanied by serious but lively discussions of scientific issues. Dave took a personal interest in every student and in every project.

David had no formal training in genetics because he had studied chemistry at the University of Utah and later worked with chemists (Haagen-Smit, a chemist, was his Ph.D. advisor and Tatum was a biochemist). According to his own account, his understanding of genetics greatly profited from his interactions with Lewis J. Stadler, the renowned maize geneticist, who spent a sabbatical semester in the Bonner lab in 1950.

In 1952 David was diagnosed with Hodgkin's lymphoma and to get the needed treatment (first surgery and then periodic radiation therapy) he would have to travel to New York and elsewhere. The disease was then considered incurable, and he managed to keep the disease at bay for 13 years until the side effects of the massive radiation therapy finally took their toll in 1964 and he died. His doctors initially predicted that he could last at most five years. According to his friends, he lived life as if he were immortal, even riding his motorcycle after his radiation treatment caused him to develop a tendency to bleeding. As the years went by and David survived, they began to believe their own wishes, and his death came as a great shock. Following David's death in 1964, Miriam Bonner and her two sons moved to Stanford University, where she worked in the laboratory of Charley Yanofsky as a laboratory assistant until her retirement.

CROSS-REACTING MATERIAL

An examination of suppressor mutations of *td* mutants showed that they were allele-specific. Most suppressor mutations only restored enzyme activity when they were combined with the respective *td* mutant allele. The group at the Institut Pasteur had started to use an antiserum against α-galactosidase as a tool to understand gene activity and this prompted Sigmund Suskind, a grad student in the Bonner lab to explore the possibility of making an antiserum against tryptophan desmolase partially purified by Yanofsky. In 1953 the Bonner group moved from the Osborn botanical laboratory to Brady Hall (a Medical School building), and this permitted greater interaction with the immunochemists housed there. Peter Treffers, an immunochemist, was chair of the Microbiology Department, and the Bonner lab grad students interacted with grad students like Stanley Mills who were using immunochemical techniques. Antibodies against enzymes were then known as antienzymes. After graduating in 1954, Sig Suskind became a postdoc in the immunology laboratory of A. M Pappenheimer at New York University, where he continued to collaborate with Charley Yanofsky and the Bonner group. The immunochemical approach opened the way to find out whether mutants that lacked enzyme activity might nevertheless contain inactive protein as shown by the presence of cross-reacting material (which they called CRM). All the suppressible mutants were shown to be CRM-positive and the nonsuppressible ones were shown to be CRM-negative (1955). This work led to the conclusion that the genetic unit may be more complex than anticipated and may be composed of genetically separable subunits. Bonner presented this view in a tightly argued paper (1956) entitled "The Genetic Unit" at the 1955 Cold Spring Harbor Symposium. He wrote: "We have an increasing number of facts with which to work and from these facts each of us enjoys a number of fantasies. At present, however, neither facts nor fantasies give rigorous proof of the nature of the genetic unit nor its action." (1956, pp. 163-170).

Meanwhile, the group worked on the biochemistry of tryptophan synthetase (subsequently renamed tryptophan synthase) as the enzyme became known around that time. However, Charley Yanofsky, who had moved to Case Western Reserve University School of Medicine in 1954, was now a friendly competitor rather than a valued collaborator. Yanofsky had decided to study tryptophan synthetase in *E. coli,* and *E coli* extracts were found to catalyze three reactions: the reversible hydrolysis of indole-3-glycerol phosphate to indole and triose phosphate; the condensation of indole and serine to form tryptophan; and the overall reaction, the conversion of indole-3-glycerol phosphate (InGP) and serine to form tryptophan. These findings suggested that the reaction proceeded in two steps: conversion of InGP to indole and condensation of indole and serine to form tryptophan; and *E. coli* was found to have a separate enzyme for each step. The Bonner lab applied these biochemical findings to *Neurospora* and showed that in this organism both steps were catalyzed by the same enzyme (1959). The use of sera to detect CRM led much later—after Bonner had moved to San Diego—to fine mapping of the antigenic sites of the enzyme and an understanding that mutations can affect the structure of an enzyme in different ways. David Bonner's contributions to our understanding of gene structure and function resulting from his work on the biochemical genetics of *Neurospora* were recognized in 1958 by his election to the American Academy of Arts and Sciences and in 1959 by his election to the National Academy of Sciences. Charles Yanofsky was elected to the National Academy of Sciences in 1966.

A SQUARE PEG IN A ROUND HOLE

David Bonner's style was not exactly a good fit for an Ivy League school. He was not only outspoken in his views but he could also be loud and outrageous. His attire (khakis or jeans) and his favorite mode of transport (a motorcycle)—typical of the rough and tumble West where he grew up—were not in keeping with the decorum expected of Yale faculty. No doubt many Yalies did not care for his persona. Dave was variously described as "straight talking," "irreverent," and "speaking his own version of the English language, laced with strong verbals." Some may have been put off by this straight-talking cowboy, but David had many close friends as well, and he inspired much affection and loyalty among the people to whom he was close. In 1953 space became available in Brady Hall, a building on the school of medicine campus, and the group moved there. It is likely that this move was facilitated by Henry Bunting, a medical faculty member who was one of David's closest friends. In 1956 David was promoted to full professor in the Medical School and the Graduate School.

According to Stanley Mills, a graduate student with Peter Treffers in the Microbiology Department in the mid-1950s, Dave's status at Yale was a subject of discussion and speculation among the students. He had the biggest and most active lab in the Microbiology Department, he had all the prerogatives of a professor, but he was said not to be "on the tenure track." Dave's not being on the tenure track is one of the most pervasive myths I have encountered in writing this memoir. According to Yale records, Dave's appointment was in the Medical School and the Graduate School. After he was appointed to full professor, it was an "appointment without term," meaning that it was a tenured appointment. However, appointments in the Medical School were different from appointments in Yale College (liberal arts and sciences); Medical School appointees were not automatically allowed to teach Yale undergraduates. Around 1958 Dave told several colleagues that he saw no future for himself at Yale and started looking for other positions. Henry Bunting's sudden and tragic death may have contributed to his gloomier outlook at that moment in his life. David's Hodgkin's disease appeared to be in remission, but he had an insurance policy at Yale that he could not afford to give up. A move depended on the new institution's willingness to pick up the policy.

At about that time the head of the Oak Ridge National Laboratory offered David the opportunity to come to Oak Ridge to pursue his research with abundant and secure research support. David invited Charley Yanofsky and Gabe Lester to move with him. They all went to Oak Ridge to scout out the possibilities. The Biology Division at Oak Ridge was located within the most secure area of the national laboratory and working there required Q-level security clearance. On the basis of an FBI investigation, David was told that questions had been raised about his "fitness" to obtain a clearance. A number of "charges" were listed, the most serious of which resulted from an "incident" in a Spanish language class that David and several other Yale faculty attended. Asked to make a controversial statement in Spanish so the class could discuss it, David stated that the United States had invaded North Korea. This is exactly what the North Koreans and Chinese were claiming as their excuse for invading South Korea. We can only speculate that someone in the class had passed this information to the FBI in a malevolent spirit. Dave fought the charges and after George Beadle testified on his behalf, the charges were eventually dropped. After this experience he lost interest in going to Oak Ridge but also did not see his own future at Yale.

THE CALL OF THE WEST COAST

In the 1950s the administration of Governor Pat Brown decided to add three new campuses to the University of California system as part of the new Master Plan for Education for the state. One of these was to be located in the San Diego area. In 1960 Dave accepted an appointment as professor of biology in the School of Science and Engineering at the newly created University of California, San Diego (then still—and only briefly—called the University of California in La Jolla). David, who had always abhorred administrative duties, was now charged with the challenging tasks of setting up not only a new Department of Biology but also becoming the unofficial acting dean to initiate a new school of medicine at UCSD, and to play a key role in the choice of founding faculty in the planned social sciences and humanities departments on campus. His enthusiasm was unbounded. At the invitation of Roger Revelle, Dave moved his entire lab to La Jolla and invited several colleagues from Yale to come along, including S. Jonathan Singer of the Department of Chemistry. Together with other former or present Yale grad students or postdocs like Stanley Mills and Jack DeMoss they would nucleate the new department. What attracted these Yale scholars and other Ivy Leaguers to come Out West was "the youthful vigor of the new campus and the clear dedication of its leaders to the long-term development of an uncompromisingly first-rate institution," according to S. J. Singer. Dave and the others were attracted by Revelle's concept of "building from the top down," meaning that Revelle would hire department chairs, who would have a free hand to appoint faculty, who would bring and attract grad students. This would all have to occur before the first undergraduates showed up on campus.

It was a wrenching tragedy that David had so few years left to begin all of this work. Nevertheless, he left an indelible mark upon UCSD. Even those who knew him fairly well did not expect him to exhibit such academic administrative skills and extraordinary vision of the future of academic biology and medicine, in contrast to his devil-may-care and free-wheeling approach to everyday matters at Yale.

Having experienced the benefits of an integrated biology department at Stanford and the drawbacks of the fragmentation of biology into different departments at Yale as well as their partitioning between the main campus and the Medical School, Dave saw the opportunity to create "a forward-looking community of scholars, teachers, and students in biology and medicine, unhindered by the dead hand of the past."[10] His vision was that UCSD would have only one group of science departments that would provide the education for all graduate students (Ph.D. and M.D.) as well as undergraduates. There was to be no biochemistry department, but biochemists were to be hired by both the biology and the chemistry departments. The Basic Science Building of the new Medical School housed faculty from the biology, pediatrics, chemistry, medicine, engineering, and surgery departments, all side by side. In addition, Bonner was totally opposed to staffing the Medical School with part-time faculty, whose attention would be diverted by their private practice. This was still the common practice at the University of California medical schools in San Francisco and Los Angeles. Bonner vowed that San Diego would be different and encountered much opposition from the Office of the President of the university. However, in fighting these battles he had the full support of his faculty. In writing about these early years, Robert Hamburger, who had also come from

Yale, noted: "The initial results were astounding and exciting but as with most radical innovations, with time, they tended to drift back toward the 'norm.'" In his four years at UCSD Bonner began the creation of a Department of Biology that emphasized molecular and cellular processes. The department had strong ties to other departments, chemistry for example, and strong ties to the medical faculty.

Research went on as well, although Dave's major effort was in the organization of the campus. In 1961 Prentice Hall published his small paperback *Heredity* in its Foundations of Modern Biology Series. In the lab he had never been a hands-on man, but he was always ready to discuss ideas and experiments with students and postdocs. Certain lifelong traditions continued in La Jolla: camping trips and life in the country. After moving west, David and Miriam bought a home in Sorrento Valley, an area of San Diego that in the early 1960s was rural and yet close to UCSD. Although he was busy, research on the problems close to his heart was carried out in his lab and collaboratively in the labs of two associates he had brought to UCSD: Jack DeMoss and Stanley Mills, who were now UCSD faculty members. In 1961 David spearheaded the organization of the first *Neurospora* conference, which was held in La Jolla with about 100 scientists in attendance.

Soon after the untimely death of this visionary scientist in 1964, UCSD honored him by naming its first biology building after him. "The loss of this uncommon man is a tragedy for his friends, his colleagues in science, and his university associates who knew his intellectual force, his physical vitality, his impatience with sham, his courage, his audacity," wrote his former colleagues in their obituary. "Although rough around the edges, he had a warm personality, was generous, and had a sensitive and liberal nature."

I am indebted to many former associates of David Bonner who contributed details and remembrances: Eliot Meyerowitz, Stanley Mills, Debbie Delmer, Sam Kaplan, Fred Addicott, Sigmund Suskind, Robert Hamburger, S. Jonathan Singer, Elga Wasserman, André Jagendorf, William Loomis, Donald Helinski, Arthur Galston, and Sue Bonner. I am especially grateful to Charles Yanofsky for his careful editorial and substantive corrections. Donna Harris from the provost's office at Yale University verified the employment history. Many details about David's life were supplied by his brother Francis T. Bonner, and some are at variance with the description of the Bonner family in the National Academy of Sciences biographical memoir of James Bonner.

NOTES

1.Bonner, D. M. Activity of the potassium salt of indole (3) acetic acid in the avena test. *Bot. Gaz.* 99 (1937): 408-497.
2.Bonner, D. M. and J. Bonner. On the influence of various growth factors on the growth of green plants. Amer. Journ. Bot 27 (1940): 38-42.
3.For details see J. Lederberg. Edward Lawrie Tatum. In *Biographical Memoirs National Academy of Sciences*, vol. 59, pp. 356-387. Washington, D.C.: National Academy Press, 1990.
4.G. W. Beadle and E. L. Tatum. Genetic control of biochemical reactions in *Neurospora. Proc. Natl. Acad. Sci. U. S. A.* 27(1941):499-506.
5.For details see N. H. Horowitz. George Wells Beadle. In *Biographical Memoirs National Academy of Sciences*, vol. 59, pp. 26-53. Washington, D.C.: National Academy Press, 1990.
6.*Am. Nat.* 79(1945):304-317.
7.*J. Biol. Chem.* 278(2003):10859-10878.
8.*Genetics* 35(1950):655-656.

9. *Nature* 166(1950):956.

10. J. A. DeMoss, S. E. Mills, S. J. Singer, and C. Yanofsky. David Mahlon Bonner. Online. *University of California: In memoriam, April1965*. Available from the online Archive of California; http://ark.cdlib.org/ark:/13030/hb338nb1j4. Accessed November 10, 2005

SELECTED BIBLIOGRAPHY

1938

With A. J. Haagen-Smit. The activity of pure substances in leaf growth. *Proc. Natl. Acad. Sci. U. S. A.* 25:185-188.

1943

With E. L. Tatum. Synthesis of tryptophan from indole and serine by *Neurospora. J. Biol. Chem.* 151:349.

1944

With E. L. Tatum. Indole and serine in the biosynthesis and breakdown of tryptophan. *Proc. Natl. Acad. Sci. U. S. A.* 30:30-37.

1946

Biochemical mutations in *Neurospora. Cold Spring Harb. Symp. Quant. Biol.* 11:14-24.

1948

Genes as determiners of cellular biochemistry. *Science* 108:735-739.

1949

With C. Yanofsky. Quinolinic acid accumulation in the conversion of 3-hydroxyanthranilic acid to niacin in *Neurospora. Proc. Natl. Acad. Sci. U. S. A.* 35:576-581.

1950

With C. Yanofsky. Accumulation of a substance possessing niacin activity by a mutant strain of *Neurospora. Fed. Proc.* 9:250.

With C. Yanofsky. Evidence for the participation of kynurenine as a normal intermediate in the biosynthesis of niacin in *Neurospora. Proc. Natl. Acad. Sci. U. S. A.* 36:167-176.

1951

With C. Yanofsky. Studies on the conversion of 3-hydroxyanthranilic acid to niacin in *Neurospora. J. Biol. Chem.* 190(1):211-218.

Gene-enzyme relationships in *Neurospora. Cold Spring Harb. Symp. Quant. Biol.* 16:143-57.

1955

With C. Yanofsky. Gene interaction in tryptophane synthase formation. *Genetics* 40:761-769.

With S. R. Suskind and C. Yanofsky. Allelic strains of *Neurospora* lacking tryptophan synthetase: A preliminary immunochemical characterization. *Proc. Natl. Acad. Sci. U. S. A.* 41:577-582.

1956

The genetic unit. *Cold Spring Harb. Symp. Quant. Biol.* 21:163-170.

1959

With J. A. DeMoss. Studies on normal and genetically altered tryptophan synthetase from *Neurospora crassa. Proc. Natl. Acad. Sci. U. S. A.* 45:1405-1412.

1961

With A. M. Lacy. Complementation between alleles of the Td locus in *Neurospora crassa. Proc. Natl. Acad. Sci. U. S. A.* 47:72-77.

1962

With H. M. Schulman. A naturally occurring DNA-RNA complex from *Neurospora crassa. Proc. Natl. Acad. Sci. U. S. A.* 48:53-63.

1964

With S. Kaplan, S. E. Mills, and S. Ensign. Genetic determination of the antigenic specificity of tryptophan synthetase. *J. Mol. Biol.* 12:801-813.

With S. Kaplan, S. Ensign, and S. E. Mills. Gene products of CRM—Mutants at the TD locus. *Proc. Natl. Acad. Sci. U. S. A.* 51:372-378.

With Y. Suyama and A. M. Lacy. A genetic map of the TD locus of *Neurospora crassa. Genetics* 49:135-144.

C. S. French

CHARLES STACY FRENCH

December 13, 1907–October 13, 1995

BY GOVINDJEE AND DAVID C. FORK

STACY FRENCH WAS BORN on December 13, 1907, in Lowell, Massachusetts, and died at Stanford, California, on October 13, 1995. The Botanical Society of America Merit Award, in 1973, described C. Stacy French as a "skillful and persistent investigator of the spectral properties and state of chlorophyll in tissues; inventor and *gadgeteer* par excellence; able and genial administrator of a productive center of botanical research—Carnegie Institution of Washington at Stanford."

Stacy was a key figure in the revolution that deciphered the photochemical events of photosynthesis, beginning with his elegant work in 1952, where he demonstrated, with Violet M. K. Young, efficient excitation energy transfer from the red and blue pigments (the phycobilins) to the green pigment chlorophyll (Chl) *a*. Stacy's later work is known mostly for the state-of-the-art analysis of the different spectral forms of Chl *a*-protein complexes and their function in the different photosystems of algae and plants.

The invention of instruments and the pioneers who displayed them have played key roles in the advancement of all the sciences (Jardine, 1999). Stacy French was one of those inventors. His prestigious inventions include the "French pressure cell" for breaking of cells; the first automatic recording fluorescence spectrophotometer; a huge (in size) curve analyzer and general purpose graphical computer; a derivative spectrophotometer; and a patented optical range-finder-based land-surveying instrument. He received many honors, including election to the National Academy of Sciences and American Academy of Arts and Sciences, both in 1963; membership in the Academie der Naturforscher Leopoldina in 1965; the Charles Reid Barnes Life Membership in the American Society of Plant Physiologists (now American Society of Plant Biologists) in 1971; the 1973 Merit Award of the Botanical Society of America; and honorary doctorate from Göteborg University in 1974.

In composing this memoir for Stacy French we shall sometimes use the thoughts of his collaborators, which are recognized only partially in the acknowledgment at the end. One of us (D.C.F.) was Stacy's close colleague for 35 years and the other (G.) was also for almost 35 years a distant colleague, a scientific admirer, and a friend—all of which accounts for the personal tone of this account. Both of us feel that Stacy was a mentor in the true sense of the word. We have freely drawn information from the text that was written by D.C.F. and edited by G. (Fork, 1996) and by Stacy himself (French, 1979).

Stacy's father, Charles Ephraim French, born in 1864 in Berkeley, Massachusetts, was a physician who had a good practice in Lowell, specializing in ear, eye, nose, and throat diseases. Stacy's mother, Helena Stacy, born in 1867 in Colebrook, New Hampshire, was a kindergarten teacher. Stacy did not consider his early education in Lowell to be any good. As a child he suffered quite a bit from respiratory infections, but had the benefit of a well-educated tutor, Flora Ewing, who guided him in kitchen-scale experiments in addition to the basics of Latin, English, and Algebra. From his father he learned to do basic carpentry, woodworking, and house painting, skills that came in handy in his later research life. French (1979, p. 2) recalls: "I entered Loomis in Windsor, Connecticut, in 1921, failed the first year, repeated it the next and later, when the time came, was refused permission to take the Harvard entrance *so as not to spoil the school's record.*" His mother enrolled him for one year in Lowell High School, and from there he was admitted to Harvard via a summer school course in botany.

Helena Halperin, daughter of Stacy, recalls:

> From his father, Stacy also learned his life-long love of wilderness. As a young man, he was an avid skier and a serious and capable climber. He climbed the Matterhorn in the 1920s, before it was common. During part of his childhood in Massachusetts, he went camping every weekend, year round. He helped build the Appalachian Mountain Club (AMC) hut at Franconia Notch, New Hampshire, and spent a great deal of his youth in the Appalachians. As an adult, he took his wife and children camping many weekends, and almost every vacation. Often, he'd roll out of the tent long before others, go fishing, build a fire, and serve fresh trout when his family was ready for breakfast.

In his first year at Harvard, Stacy seemed interested in pursuing engineering (including mathematics and physics), but in his second year he became interested in biological sciences, particularly general physiology, mainly because of the laboratory with good instruments (instruments seem to be one of the threads in Stacy's life). Stacy's undergraduate adviser was a prominent mathematician George D. Birkhoff. He met in 1928 his first mentor, Robert Emerson (1903-1959),[1] from whom he first learned about photosynthesis, and he wrote, "I have stayed with the subject ever since." His undergraduate thesis dealt with the temperature coefficient of catalase action under the guidance of W. J. Crozier and A. E. Navez. Stacy received his B.S. degree from Harvard in 1930. Another example of his open and frank personality is in his statement, "[My undergraduate thesis] very nearly kept me from graduating with my class in 1930 through lack of attention to the math and organic chemistry classes" (French, 1979, p. 3).

For his Ph.D., Stacy attended Harvard, where he was among a group of graduate students who organized the Chlorella Club. The club was organized because students "felt the department seminars were too dominated by the professor" (French, 1979, p. 4). William Arnold, who had in 1932 discovered with Robert Emerson what later became known as the photosynthetic unit concept[2] (i.e., thousands of chlorophyll molecules feeding excitation energy to a few photoenzyme molecules) and Caryl Haskins, who later became president of the Carnegie Institution of Washington, were prominent actors in the Chlorella Club. Stacy states: "[It] gave us more education than any of the biology courses" at Harvard. For his doctoral research Stacy worked on the rates of respiration in the green alga *Chlorella* at different temperatures. He accumulated a lot of experimental data and found it almost impossible to write his thesis, but according to Stacy (French, 1979, p. 4), a postdoctoral fellow from China, Pei-Sung Tang, saved his academic life by helping him separate the scientific wheat from the chaff of his innumerable experiments. Stacy worked with Tang on the effects of oxygen pressure on respiration of *Chlorella*; this led to Stacy's first scientific paper in the *Chinese Journal of Physiology* (1933). Stacy received his Ph.D., in biology, from Harvard in 1934.

Not being able to stay at Harvard, Stacy accepted a position at the California Institute of Technology with Robert Emerson to study photosynthesis of purple bacteria. In preparation for this area Stacy spent the summer of 1934 taking Cornelis B. van Niel's famous course in microbiology at the Hopkins Marine Station in Pacific Grove, California. Van

Niel was known to lecture for four to six hours without losing his students' interest. There were, however, distractions at Caltech for Stacy, such as excellent skiing in the mountains in the area. Emerson, who one of us (G.) knew well, was in Stacy's words "justifiably disgusted with my performance and we were barely on speaking terms for the academic year. . . However, in spite of my poor performance, he arranged for me to spend the next year in Berlin with Otto Warburg, which was what saved my scientific career" (French, 1979, p. 5). It seems that there were a number of times when the assistance and generosity of others allowed Stacy to develop his own skills as a scientist and a successful scientific career.

Since research with the Nobel Laureate Otto Warburg[3] was a crucial turning point in Stacy's career, we quote here Stacy's own statement (French, 1979, p. 6):

> The year with Professor Otto Warburg at the Kaiser Wilhelm Institut, (now the Max Planck Institut) was one of intense concentration on the efficiency and action spectrum of photosynthesis in *Rhodospirillum rubrum.* Living in the laboratory building and eating at Harnack House just around the corner in Dahlem was most convenient. The laboratory was excellently supplied with optical equipment left over from Warburg's determination of the action spectrum for dissociation of the CO complex with the respiratory enzyme, which had brought him the Nobel Prize. I was treated very well by the professor and all his staff. The rigid discipline and long hours without outside distraction were just what was needed to convert an easygoing freewheeling academic type into a professional scientist.

Stacy further wrote: "I am grateful to Warburg for refusing to speak English though he could do it better than I." Stacy's measurements of hydrogen and carbon dioxide photoassimilation in purple bacteria were published in *Science* (1936); his measurements on the quantum yield and action spectrum of bacterial photosynthesis were published soon thereafter (1937).

French held a teaching fellowship from 1936 to 1938 at the Harvard Medical School. Here, with the help of Professor Theodore Lyman (of Harvard's physics department), he measured absorption spectra of photosynthetic bacteria. It was at this time that the seeds of the French press may have developed in his mind. With the help of Professor G. W. Pierce, he was the first to use a supersonic vibrator to break photosynthetic organisms.

Stacy went to work with another Nobel laureate, James Franck,[4] at the University of Chicago in 1938. The appointment with Franck (known for the Franck-Condon principle, among many other things) was that of an instructor (research) in chemistry (on an annual salary of $2,400). This job in 1938 required Stacy to help Franck establish a photosynthesis laboratory. It was during this period that Stacy married Margaret Wendell Coolidge, who was the daughter of the first master of Lowell House at Harvard, Professor Julian L. Coolidge. It was Margaret who helped repair Stacy's friendship with Robert Emerson.

Although Stacy published, with Franck and Ted T. Puck in 1941, one of the earliest quantitative papers on the relationship between chlorophyll *a* fluorescence and photosynthesis, Stacy's philosophy and personality did not match Franck's. Franck's approach was too theoretical for Stacy's tastes. One of us (D.C.F.) wrote in 1996: "In essence, Stacy was Franck's technician and was expected to do the critical measurements to support Franck's current thinking. Stacy, being an inveterate experimentalist, wanted to measure as many of the parameters of photosynthesis as possible and then try to explain their significance but Franck had no appreciation for this approach." Although Stacy was unhappy working with Franck, he did associate with Hans Gaffron[5] (a former Warburg associate), Roderick Clayton,[6] Robert Livingston, and Warren Butler.[7] At Chicago, Stacy measured oxygen evolution in isolated chloroplasts according to Robin Hill's procedure with visiting biochemist Mortimer Anson of the Rockefeller Institute. They proposed the name "Hill reaction"[8] in 1941 for the effect (French, 1979, p. 10). An aside: When Stacy was with Franck, Clayton was an undergraduate student helper whose assignment was to prepare neutral density filters. Stacy recalls (French, 1979, p. 10) that these very filters made from metal screens had been used by him in many experiments.

Stacy was able to "escape" from Franck after accepting a position in 1941 as an assistant professor of botany at the University of Minnesota. The invitation had come from George Burr (who was once Jack Myers's thesis advisor). Here he mentored his one and only doctoral student: A. Stanley Holt. French and Holt together studied the Hill reaction with various dyes, and, using ^{18}O experiments, showed that oxygen came from water in isolated chloroplasts (1948,1,2). (Sam Ruben, Martin Kamen, and coworkers had already shown it to be the case in whole algal cells.) Stacy had a stimulating academic time in Minnesota. With Holt and Glenn Rabideau, Stacy measured absorption spectra of leaves and algal cells, after first setting up a laboratory-made monochromator, and a large Ulbricht integrating sphere. In Minnesota, Stacy met, among others, Allan Brown (who has done some of the pioneering mass spectroscopic measurements, separating respiration from photosynthesis) and Albert Frenkel (who later discovered photophosphorylation in photosynthetic bacterial membranes).

During World War II, Stacy asked his neighbors to give him all their grapefruit rinds so that he could grow penicillin for war use, thus avoiding conscription. In addition to teaching plant physiology he taught elementary physics, and researched chlorophyll-containing paint for camouflage purposes in order to satisfy the draft board.

Stacy was promoted to an associate professor's position at the University of Minnesota, but left for Stanford in 1947 when he was appointed director of the Department of Plant Biology. The offer of the directorship from Vannevar Bush, president of the Carnegie Institution of Washington, and from Carnegie trustee Alfred Loomis was the best thing for Stacy's life and scientific career. He was happy to get away from Minnesota's long and cold winters and the associated respiratory problems they caused him. He accepted this challenging position and succeeded Herman Spoehr on July 1, 1947. Spoehr, Bush, and Caryl P. Haskins were his greatest supporters throughout their common period at Carnegie. Spoehr wrote on May 31, 1946, to V. Bush :

> I have been watching French for some time and have known him ever since he took his degree. . . French has grown very well, he is doing excellent work at Minnesota and impresses me as a very level-headed fellow and a hard worker. We have (*handwritten:* been) pretty close professionally because he is probably the livest [perhaps, he meant 'liveliest'?] worker in the field.

On November 14, 1946, Caryl P. Haskins gave the recommendation to V. Bush that he should hire French :

> It would be my opinion that Dr. French is definitely of the caliber to occupy a post of this sort [director's position]. As you know, he is personally very highly qualified and I would definitely feel that his qualifications on the scientific and technical side are equally high. He did very fine work in the field of photosynthesis at Harvard and his work since that time has been outstanding. . . I think I can say unreservedly that I would have no hesitation in recommending him for this important work. . . I think that both he and the Institution would be very fortunate."

Photosynthesis research reigned supreme during Stacy's long tenure (1947-1973) as director of the Department of Plant Biology; however, Stacy was equally supportive of the experimental taxonomy group with Bill Hiesey, David Keck, Jens Clausen, and Malcolm Nobs. Innovative biochemical investigations were devoted to photosynthesis by James H. C. Smith (formation of chlorophyll in greening systems), Harold Strain (chromatography of photosynthetic pigments), Harold Milner (chemistry), and Herman Spoehr (chemistry). A major project was the large-scale culturing of algae. This research led to a well-known book, *Algal Culture from Laboratory to Pilot Plant* (Burlew, 1953). Stacy was, however, most interested in the machine shop and the wood shop in the basement of the Plant Biology building. He spent more time in these shops, smoking his cigars and tinkering with instruments, than at his desk or in the laboratory. A most dramatic sight was the assemblage of ingenious pieces of biophysical equipment that Stacy and his postdoctoral fellows were building to study photosynthesis. Over the years the shop had the expertise of George Schuster, Louis Kruger, Richard Hart, Frank Nicholson, and Brian Welsh, and was open to all.

Stacy was a master at building ingenious instruments. He loved to conceptualize and develop new pieces of apparatus that would permit him to investigate new and different aspects of photosynthesis. Stacy was a pioneer in integrating methods from physics, engineering, and chemistry. In 1935 he had met the Russian professor N. N. Lubimenko, who believed in the existence of different forms of chlorophyll *a* in vivo. At Harvard he had broken photosynthetic cells by using supersonic vibrations. Vannevar Bush had made some suggestions about breaking cells through a fixed hole. Stacy wrote:

> To have a small hole that could be opened up when plugged and then constricted again led me to the idea of a *needle valve* instead of a fixed hole. I threaded an ammonia needle valve into a steel cylinder with a hole in the center to contain chloroplast suspension. The suspension was forced through the valve by a steel piston with a seal. This extrusion principle worked well and has since found much use for disruption of bacteria and

other cells as well as for chloroplasts. When I told Dr. Bush that the device was being made commercially, he suggested that I ask the manufacturer to attach my name to the device and to give a free one to the laboratory. The first request was acceded to at once, and about 20 years later the American Instrument Company gave the department a "*French Press*." We still drive it with an old hydraulic jack made for automobiles (French, 1979, p. 17).

In other words, Stacy accomplished the task of breaking difficult cells by releasing the pressure suddenly through a needle valve, the cells being forced out and broken by the resulting large shear forces (1948,1; 1951,2). This device is currently in use in many laboratories around the world.

Stacy also constructed the first automatic recording fluorescence spectrophotometer that simultaneously measured and corrected fluorescence emission spectra. This was done with two homemade grating monochromators and a rotating plastic drum on which was inked a correction curve followed by a photocell. We doubt that with the advent of many commercial fluorometers, anyone today would bother to build such an instrument. One of us (D.C.F.) wrote (Fork, 1996):

Stacy had a good relationship with the Carnegie astronomer Ira Bowen of Carnegie's Mt. Wilson/Palomar Observatories and he helped Stacy obtain large diffraction gratings and optical components needed to construct the big monochromators he used. At one time when results were not what were expected, Stacy found that a mouse had taken up residence inside one of these large monochromator enclosures. After this experience Stacy would sometimes claim an unexpected result was probably produced as a result of the mouse "peering out the monochromator slit."

Both of us loved this lighter side of Stacy's personality.

Later, Stacy fabricated an ingenious instrument to measure the first derivative of the absorption spectrum. With this instrument, he successfully detected minor spectral forms of chlorophyll *a*, whose absorption spectrum would have otherwise gone undetected. The method used the vibration of the slit of the monochromator over a few nanometer wavelength intervals. Although it took several years of Stacy's time to complete this spectrophotometer, it towered imposingly above its tables, and was a sight to be seen. Interestingly, several of its optical and electronic parts were from military surplus equipment; this was amusingly obvious from the note on one of its aluminum circuit boxes: "Do not remove from the airplane." Almost all visitors noticed it, and some even laughed. Stacy taught Jeanette Brown to run this "monster" (1959). Although he believed that these derivative spectra showed the existence of several forms of chlorophyll *a* in vivo, some remained unconvinced until the pigment-protein complexes were biochemically isolated and their absorption spectra measured. As a companion to the new spectrophotometer, Stacy built, with the help of his electronics assistant, Gordon Harper, another innovative instrument, a curve analyzer (1954). It consisted of five separate, vertically moving tables on which were drawn Gaussian curves. What a sight: The curves were followed by a photocell that tracked the curve as the table moved; a potentiometer recorded the changes as a voltage. This provided the data for the ordinate as the wavelength was recorded on the abscissa. This giant machine produced a curve that was the sum of the curves on the five tables.

Stacy would often change the parameters on each of the movable tables, using trial and error to estimate the various spectral forms of chlorophyll necessary to produce a resulting combined curve that would match the measured overall absorption spectrum. There was some relief as one could sit on a stool in the middle of the semicircle of tables and contemplate what and how much to change next. (Luxury was in sight when Stacy decided that the operator would sit on a swivel armchair.) Later, Stacy added a curve digitizer. Then, normal Gaussian-shaped component curves could be added together to fit the measured absorption spectrum: For most this was easier to accept and understand. When digital computing became available, Stacy and his coworkers were able to more easily analyze absorption, fluorescence, and action spectra; the existence of several forms of chlorophyll *a* (particularly, Chl *a* 662, Chl *a* 670, Chl *a* 677, and Chl *a* 684) became generally accepted (1972).

A visit by Francis Haxo to the department was a great pleasure to Stacy; he instantly fell in love with the rate-measuring oxygen electrode of Haxo and Lawrence Blinks for the measurement of the action spectra for oxygen evolution. The advantages of this polarographic technique—very small samples and the fast speed of measurement—became obvious to Stacy. Together with Per Halldal, Stacy obtained an action spectrum for oxygen evolution, using the Haxo-Blinks method. There was much excitement in the air: Robert Emerson and his coworkers had discovered an enhancement effect in photosynthesis when far-red light (absorbed in Chl *a*) was combined with light absorbed by Chl *b* and other pigments (Emerson et al., 1957), and Blinks had discovered a two-light effect in oxygen transients (Blinks, 1957). Ideas and experiments around the world were beginning to shape the concept that two light reactions must cooperate to bring about photosynthesis in plants (see Govindjee and Krogmann, 2004). Jack Myers and Stacy measured the action spectra for the Emerson enhancement and the Blinks effects and showed that alternation of lights absorbed preferentially by (what we now call) pigment system 1 and pigment system 2, even at intervals separated by 0.6 s could still produce enhancement: This was clear evidence for separate and lasting effects produced by the two photosystems of photosynthesis (1960). Both of us recall our own involvement in the Emerson enhancement effect that was parallel to this work of Stacy. Fork (cited in Haxo [1960]) had shown that one photosystem (now called "I") was indeed sensitized by Chl *a*, as he had shown both the blue (Soret) and the red bands of Chl *a* in the action spectrum of the Emerson enhancement when the second light beam was absorbed by the red pigment phycoerythrin in the red alga *Porphyra*. Govindjee and Rabinowitch (1960) showed that Chl *a* 670 was in the same system as Chl *b*, and it turned out that Stacy had also independently observed this (see 1961,1).

After Stacy became director of the Department of Plant Biology, President Vannevar Bush (see Trefil and Hazen, 2002) started a fellowship program for postdoctoral fellows and visiting investigators from around the world. Stacy and James Smith would visit laboratories in Europe to recruit outstanding scientists. Among others, they recruited Louis N. M. Duysens and Bessel Kok. These programs were very good for the Carnegie laboratory and for the visitors, creating friendships and cultivating the exchange of scientific ideas. Collaboration between Carnegie scientists and scientists throughout the United States and the world flourished under Stacy's leadership. Stacy felt rather strongly that for most scientists, significant intellectual life from within may not be that easy, and that intellectual growth is often dependent on interactions with others. An efficient way to become successful is to interact, he thought, with the "right people" and to learn from their lives. Stacy French was an outgoing friendly person, always jovial and full of humor. He seemed to buy scientists' time and then give it back to them with trust, and above all, he had great fun doing research with them. Stacy promoted a laboratory style that allowed scientists to think and do research independently. He stressed cooperation over competition among scientists. It was a wonderful place to be, as frequent, informal, and friendly discussions were encouraged. We remember well the scientific discussions that continued over lunch as we sat in the Adirondack chairs under the trees in the native plant garden and shared our food with scrub jays and brown towhees.

One of us (G.) remembers Stacy's generosity over the years: his acceptance of the independent observation of the presence of Chl *a* 670 in the Chl-*b*-containing system; his acceptance that G. was the first to see Chl *a* 670 in Chl-*c*-containing systems; and his insistence that we publish our work on the two-light effect, that we did in his laboratory in 1963, without his name, since as young beginning scientists we needed independent publications (Govindjee and Govindjee, 1965).

Though many of his friends did not appreciate Stacy's cigar smoking, his habit did have some use in the laboratory: There were plenty of cigar boxes to keep lenses, prisms, filters, photocells, and other small items. Some of these boxes may still be hidden in the basement of the current laboratory. Hemming Virgin told us a story that he once observed what he thought was an exciting new fluorescence band, but soon thereafter discovered this new emission band was seen only when Stacy was standing near the equipment, smoking his cigar. One of us (D.C.F.) remembers that Stacy seemed to enjoy leaning over a complicated optical or mechanical setup with a long ash clinging precariously to the end of his cigar with a comment such as, "What are you doing?" Sometimes the ash fell, causing minor embarrassment. His smoking did stop when his doctors told him to stop. (Also see Govindjee, 1989, for a presentation at Stacy's eightieth birthday.)

Everybody who met Stacy knew that he was not one for appearances. He usually drove an old car with an interior well worn from transporting dogs. He wore rumpled coats and baggy pants. Franck Nicholson told one of us (D.C.F.)

an amusing story about Stacy coming into the shop one day and asking for a needle and thread, and when these could not be found, mending his trousers with a stapler.

Stacy married his first wife, Margaret W. Coolidge, in 1938. They had a daughter, Helena Stacy Halperin, and a son, C. Ephraim French, as well as four grandchildren. Stacy and Margaret were wonderful and hospitable people; they hosted many great parties at their homes in Palo Alto and later in Los Altos Hills. Stacy and Margaret had one or two dogs of their own and at these parties even neighbor dogs would be welcomed to mix with the group, wagging their tails and hoping for some of the hors d'oeuvres. Almost everyone we know felt Stacy and Margaret's genuine friendship and loved the informal manner with which they made everyone feel welcome in their relaxed, warm home.

Stacy felt a great loss when Margaret died in 1992. After Margaret's death, Stacy met Lee Penland, a retired lawyer and they fell in love and were married in 1993. Together Lee and Stacy continued the tradition of extending warm hospitality to visitors of their hillside home. Memorial services for Stacy were held at Hidden Villa Ranch in Los Altos Hills on October 28, 1995.

The legacy of Stacy can be felt from the recollections of others. We present some selected quotes below.

Jack E. Myers, a longtime associate of Stacy French wrote (personal communication, e-mail message on May 4, 2005, to G.):

> I first met Stacy in 1936. . . James Franck evidently heard of my observation [on oxygen uptake in high light in *Chlorella*] by grapevine and sent Stacy (his post doc) to see me in Minneapolis. Stacy and I sat outside the old botany building overlooking the Mississippi and talked about my experiments. . . Actually, Stacy was not very much interested in my experiments.

He wanted to follow up on Robin Hill's then recent observation of O_2 evolution by chloroplasts but Franck thought that chloroplasts were a waste of time. Stacy later got a faculty position (at Minnesota) and set about following chloroplast activity with dye reduction. Later, when Stacy had become the Director at the Carnegie laboratory, I spent several sessions as a visiting investigator. One session came at a time when Vannevar Bush, Head of the Carnegie Institution of Washington, housed in Washington (DC) headquarters, was goosing the laboratory to follow up on the idea that algae seemed to be more efficient than higher plants. Everyone at the laboratory had a special project on algae. One of them was Ed Davis; Ed was studying growth of a *Chlorella* Culture pumper through a long length of Tygon tubing under Sunlight. You may remember that Tygon tubing used to darken with age. This led Ed to order a new batch from a supply house. When it came we all admired it and Ed was set to install it the next day. Stacy then playfully substituted a batch of the old dark tubing just to see the look on Ed's face the next morning. . .

In 1959, I went to the Carnegie for a semester on a Guggenheim Fellowship to work with Stacy. He had spent some time with Francis Haxo learning the technology of the O_2 electrode. . . I had no family with me and, thus, I spent much of each night doing experiments (watching the recording of O_2 exchange). Stacy did all the "instrumental improvements" in light beams and shutters. It was the most productive year of my life. At heart Stacy was an inventor who loved to make optically based instruments from surplus bomb sights. He carried to extreme an analog analysis of spectra which he called a curve analyzer. He inked in and made opaque one side of a spectral curve; then, he devised a feedback system that made a photocell follow the curve. That allowed adding, subtracting or multiplying spectra. It was the basis of his intense study of absorption and fluorescence spectra in search of putative in vivo chlorophylls. The graph paper on which the curve follower worked was held in place by suction from a vacuum-cleaner motor. I remember that when I last admired the machine several parts were still held in place by C-clamps from its development days. Stacy did not go for fancy store-bought instruments and preferred to build his own. He was a great collaborator whom I remember with affection and admiration.

Yaroslav de Kouchkovsky, a 1963-1964 Carnegie research fellow, wrote (personal communication, e-mail message on May 2, 2005, to G):

Stacy French's policy was very open-minded: once he trusted people, he gave them all opportunities to enrich themselves through multiple contacts and encouraged them to follow their own research line, provided it was original. He let people conduct their investigations as they thought best, but his door was always open for on-the-spot discussions. I started thus my new research work with complete freedom and eager to do something new. The Plant Biology Laboratory of the Carnegie Institution of Washington, at Stanford, California, had an excellent reputation in the field and was a mecca for all "photosynthesizers." Many essential discoveries of that period originated from there and many original set-ups, that could not be found elsewhere, were built by Stacy, for example, the first derivative spectrophotometer. The French laboratory was a place of open and friendly exchanges, with morning coffee breaks that gave the opportunity to discuss the latest research advances or projects presented by a colleague. This maintained a good level of cooperation, and no publication could be submitted to a journal before first being edited by all the other members of the laboratory. Of course, this special atmosphere, that looks "old-fashioned" nowadays, was possible not only because it was then a small community, but at the first place, it was due to the rich personality of Stacy French.

Jan M. Anderson, a 1996 Carnegie research fellow, wrote (personal communication, email message on May 1, 2005, to G.):

I enjoyed my time with Stacy French immensely, the peaceful lab and the marvelously friendly group. However, I missed my beautiful Cary 14 spectrophotometer back in Canberra. Jan Amesz and I persuaded Stacy to request a Cary on loan. Despite good reports from all who tried it out, Stacy was not persuaded to buy the machine, as he said there was no one there who would really understand just how good it was. We greatly admired his string-and-sealing-wax approach for all the ingenious machines he built, but regretted the outcome of the Cary battle, which Stacy won. I greatly admired his writing, and on rereading his papers can hear his unique accent. I was only there for six months, the first-ever Aussie and thought it was heaven.

Tasso Melis, a 1979-1981 Carnegie research fellow, wrote (personal communication, e-mail message on May 2, 2005, to G.):

> Stacy French had already retired when I arrived at the Carnegie in June 1979. He was at the Institute daily, albeit for a few hours. He became very interested to learn that I was building a "Duysens-type" split beam absorbance difference spectrophotometer . . . He encouraged me in this effort, in fact he gave me his high-resolution high-throughput Bausch and Lomb monochromator, which I successfully used in conjunction with this spectrophotometer during my entire stay at the Carnegie Institution. Stacy was fascinated (perhaps even awed) by the effect of the "silicon" revolution on scientific equipment. The advent of semiconductors had made a huge difference in the size of electronic scientific devices. Looking over my shoulder, as I was trying to calibrate components of the spectrophotometer, Stacy once marveled at how small the digital components of the lock-in amplifier were, compared to the electronic lamp amplifiers of the yesteryear. He also recognized the novelty of the absorbance difference spectrophotometry approach in the measurement of the photosystems, as he once quipped, "No one really ever measured the stoichiometry of the photosystems." Stacy French exemplified the ultimate research scholar, and I could see how, under his leadership, many of the great colleagues of his generation walked, worked, and contributed to the advancement of the field of photosynthesis research at the Carnegie Institution.

Arthur Grossman, one of the current staff members of the Department of Plant Biology of the Carnegie Institution, Stanford, California, wrote (personal communication, email message on May 2, 2005, to G.):

> Stacy would come by every couple of months and find me and talk about what I was doing, although he might have been a little skeptical about my approaches, he always seemed amused, animated and curious about what I might (or might not) accomplish. I still remember the large motor that I think Frank Nicholson scavenged from a huge chart recorder that Stacy had built; the motor was used to power the spit for the traditional hog roast that was held at Carnegie every year.

Winslow Briggs, currently director emeritus, recalls:

> Perhaps Stacy's most outstanding trait was his almost fanatic support of his staff members in protecting them from distractions (e. g., teaching, having graduate students) that would interfere with their research programs. A steady stream of both short- and long-term scientists marveled at the wonderful atmosphere for research at the Department. Another trait was his unwillingness to buy commercially produced equipment when he was certain he could build a better model himself for a fraction of the price. During his era, he was almost always right. For example, he was doing derivative spectrophotometry mechanically twenty years before computers made it routinely possible. His instrumentation provided one ground-breaking discovery after another. When I first arrived at Carnegie in 1973, and wanted to purchase a large piece of equipment before I officially became director, I almost had to stand over his shoulder to get him to sign the purchase order! Despite this inauspicious start, he and I remained very close friends after I became director.

WE ARE INDEBTED TO Helena Halperin, Stacy's daughter, for her recollections of her father, and to Jack Myers, Yaroslav de Kouchkovsky, Jan M. Anderson, Tasso Melis, Arthur Grossman, and Winslow Briggs for their impressions of Stacy. We thank Jeanette Brown and Pat Craig for sharing copies of material included in Craig (2005). Govindjee thanks John Strom for providing him access to Stacy French's file at the Archives of the Carnegie Institution of Washington in Washington, D.C., and Winslow Briggs for initiating the invitation and Jan A. D. Zeevaart for the invitation to write this memoir. We are grateful to Jeanette Brown, Pat Craig, Yaroslav de Kouchkovsky, Art Grossman, Tasso Melis, Kärin Nickelsen, Jack Myers, and Tony Ziselberger for reading and making suggestions on this manuscript.

On behalf of the two of us (D.C.F. and G.) and several others shown in a partial list below, we remember you Stacy, for your scientific accomplishments, for your friendship, your free spirit, and for all the fond personal remembrances you have given us.

Amesz, Jan
Anderson, Jan M.
André, M.
Arnold, William A.
Berry, Joseph
Björkman, Olle
Björn, Lars-Olof
Bril, Cornelis
Brown, Jeanette S.
Chen, Shao-lin
Davis, Edwin A.
de Kouchkovsky, Yaroslav
Decker, John P.
Detchev, Giorgi
Duysens, Louis N. M.
Emerson, Robert
Fewson, Charles A.
Gasanov, Ralphreed A.
Gibbs, Martin
Goedheer, Joop C.
Goodwin, Richard H.
Govindjee, Rajni
Habermann, Helen M.
Hagar III, William G.
Halldal, Per
Haxo, Francis T.
Heber, Ulrich W.
Hill, Robert
Hiyama, Tetsuo
Holt, A. Stanley
Jacobi, Günter
Jorgensen, Erik G.
Kok, Bessel
Krauss, Robert W.
Kupke, Donald W.
Landolt, E.
Latimer, Paul H.
Lewis, Charleton M.
Lewis, H.
Loeffler, Josef E.
Loos, Eckhard E.
MacDowall, Fergus D. H.
Madsen, Axel
Mantai, Kenneth E.
McGinnis, William G.
McLeod, Guy C.
Menke, Wilhelm
Michel, Jean-Marie
Michel-Wolwertz, Marie-Rose
Milner, Harold W.
Milner, Max
Müller, Alexander
Murata, Norio
Murata, Teruyo
Myers, Jack E.
Ninnemann, Helga
Pearcy, R. W.
Pickett, James M.
Raymond, Lawrence P.
Ried, August
Sager, Ruth
Schiff, Jerome A.
Schreiber, Ulrich
Schulman, Marvin D.
Šesták, Zdenek
Shibata, Kazuo
Smith, James H. C.
Soeder, Carl J.
Stanier, Roger W.
Strain, Harold H.
Strehler, Bernard
Takamiya, Atusi
Tamiya, Hiroshi
Troughton, J. H.
Urbach, Wolfgang
Van Niel, Cornelis B.
Vidaver, William E.
Virgin, Hemming I.
Vishniac, Wolf
Von Wettstein, Diter
Weaver, Ellen C.
Wiessner, Wolfgang
Wolf, Frederick T.
Wraight, Colin A.
Young, Violet M. (Koski)

NOTES

1. E. Rabinowitch. Robert Emerson (1903-1959). In *Biographical Memoirs,* vol. 25, pp. 112-131. Washington, D.C.: National Academy of Sciences, 1961. Govindjee. Robert Emerson and Eugene Rabinowitch: Understanding photosynthesis. In *No Boundaries: University of Illinois Vignettes,* ed. L. Hoddeson, pp. 181-194. Chicago: University of Illinois Press, 2004.

2. R. Emerson and W. Arnold. A separation of the reactions in photosynthesis by means of intermittent light. *J. Gen. Physiol.* 15(1932):391-420. R. Emerson and W. Arnold. The photochemical reaction in photosynthesis. *J. Gen. Physiol.* 16(1932):191-205.

3. H. A. Krebs. *Otto Warburg: Cell Physiologist, Biochemist and Eccentric.* Oxford, U.K.: Oxford University Press, 1981. B. Vennesland. Recollections and small confessions. *Annu. Rev. Plant Physiol.* 32(1981):1-20.

4. J. L. Rosenberg. The contributions of James Franck to photosynhesis: A tribute. *Photosynth. Res.* 80(2004):71-76.

5. See, e.g., P. Homann. Hydrogen metabolism of green algae: Discovery and early research—a tribute to Hans Gaffron and his coworkers. *Photosynth. Res.* 76(2003):93-103.

6. See, e.g., R. K. Clayton. Research on photosynthetic reaction centers from 1932-1987. *Photosynth. Res.* 73(2002):63-71.

7. A. A. Benson. Warren Lee Butler (1925-1984). In *Biographical Memoirs*, vol. 74, pp. 1-18. Washington, D.C.: National Academy Press, 1998.

8. D. A. Walker. "And whose bright presence"—an appreciation of Robert Hill and his reaction. *Photosynth. Res.* 73(2002):51-54.

REFERENCES

Blinks, L. R. 1957. Chromatic transients in photosynthesis of red algae. In *Research in Photosynthesis*, eds. H. Gaffron, A. H. Brown, C. S. French, R. Livingston, E. I. Rabinowitch, B. L. Strehler, and N. E. Tolbert, pp. 444-449. New York: Interscience.

Burlew, J. S., ed. 1953. *Algal Culture from Laboratory to Pilot Plant.* Pub. No. 600. Washington, D.C.: Carnegie Institution of Washington.

Craig, P. 2005. Centennial History of the Carnegie Institution of Washington, vol. 4. Department of Plant Biology. [See pp. 145-190] Cambridge, U.K.: Cambridge University Press.

Emerson, R., R. V. Chalmers, and C. N. Cederstrand. 1957. Some factors influencing the longwave limit of photosynthesis. *Proc. Natl. Acad. Sci. U. S. A.* 43:133-143.

Fork, D. C. 1996. Charles Stacy French: A tribute. *Photosynth. Res.* 49:91-101.

French, C. S. 1979. Fifty years of photosynthesis. *Annu. Rev. Plant Physiol.* 30:1-36.

Govindjee. 1989. My association with Stacy French. In *Photosynthesis: Proceedings of the C. S. French Symposium Held in Stanford, California, 17-23 July 1988.* In *Plant Biology*, vol. 8, ed. W. R. Briggs, pp. 1-3. New York: John Wiley and A. R. Liss.

Govindjee, and R. Govindjee. 1965. Two different manifestations of enhancement in the photosynthesis of *Porphyridium cruentum* in flashing monochromatic light. *Photochem. Photobiol.* 4:401-415.

Govindjee, and D. Krogmann. 2004. Discoveries in oxygenic photosynthesis (1727-2003): A perspective. *Photosynth. Res.* 80:15-57.

Govindjee, and E. Rabinowitch. 1960. Two forms of chlorophyll *a* in vivo with distinct photochemical functions. *Science* 132:355-356.

Haxo, F. T. 1960. The wavelength dependence of photosynthesis and the role of accessory pigments. In *Comparative Biochemistry of Photoreactive Systems*, ed. M. B. Allen, pp. 339-360 (see pp. 356 and 367). New York: Academic Press.

Jardine, L. 1999. *Ingenious Pursuits. Building the Scientific Revolution.* London: Abacus.

Myers, J. 2002. In one era and out the other. *Photosynth. Res.* 73:21-28.

Tang, P. 1983. Aspirations, reality, and circumstances: The devious trail of a roaming plant physiologist. *Annu. Rev. Plant Physiol.* 34:1-20.

Trefil, J., and M. H. Hazen. 2002. *Good Seeing: A Century of Science at the Carnegie Institution of Washington 1902-2002.* Washington, D.C.: Joseph Henry Press.

SELECTED BIBLIOGRAPHY

1933

With P. S. Tang. The rate of oxygen consumption by *Chlorella pyrenoidosa* as a function of temperature and of oxygen tension. *Chinese J. Physiol.* 7:353-378.

1936

Hydrogen and carbon dioxide photoassimilation in purple bacteria. *Science* 84:575-575.

1937

The quantum yield of hydrogen and carbon dioxide assimilation in purple bacteria. *J. Gen. Physiol.* 20:711-725.

The rate of carbon dioxide assimilation by purple bacteria with various wavelengths of light. *J. Gen. Physiol.* 21:71-87.

1941

With J. Franck and T. T. Puck. The fluorescence of chlorophyll and photosynthesis. *J. Phys. Chem.* 45:1268-1300.

1945

With G. S. Rabideau. The quantum yield of oxygen production by chloroplasts suspended in solutions containing ferric oxalate. *J. Gen Physiol.* 28:329-342.

1946

Photosynthesis. *Annu. Rev. Biochem.* 15:397-416.

1947

With G. S. Rabideau and A. S. Holt. The construction and performance of a large grating monochromator with a high energy output for photochemical and biological investigations. *Rev. Sci. Instrum.* 18:11-17.

1948

With H. W. Milner, M. L. Koenig, and F. D. H. Macdowall. The photochemical activity of isolated chloroplasts. *Carn. Inst. Wash. Yearb.* 47:91-93.

With A. S. Holt. Isotopic analysis of the oxygen evolved by illuminated chloroplasts in normal water and in water enriched with O^{18}. *Arch. Biochem.* 19:429-435.

1949

With H. W. Milner, N. S. Lawrence, and M. L. Koenig. The photochemical activity of disintegrated chloroplasts. *Carn. Inst. Wash. Yearb.* 48:88-89.

1951

With V. M. Koski and J. H. C. Smith. The action spectrum for the transformation of photochlorophyll to chlorophyll *a* in normal and albino corn seedlings. *Arch. Biochem. Biophys.* 31:1-17.

With H. W. Milner. The photochemical reduction process in photosynthesis. In *Symposia of the Society for Experimental Biology. V. Carbon Dioxide Fixation and Photosynthesis*. pp. 232-250. New York: Cambridge University Press.

1952

With V. K. Young. The fluorescence spectra of red algae and the transfer of energy from phycoerythrin to phycocyanin and chlorophyll. *J. Gen. Physiol.* 35:873-890.

1954

With G. H. Towner, D. R. Bellis, R. M. Cook, W. R. Fair, and W. W. Holt. A curve analyzer and general purpose graphical computer. *Rev. Sci. Instrum.* 25:765-775.

1955

With H. W. Milner. Disintegration of bacteria and small particles by high-pressure extrusion. In *Methods in Enzymology*, ed. S.P. Colowick, pp. 64-67. New York: Academic Press.

With A. T. Giese. The analysis of overlapping spectral absorption bands by derivative spectrophotometry. *Appl. Spectrosc.* 9:78-96.

1957

Derivative spectrophotometry. In *Proceedings of ISA Instrumentation and Control Symposium,* pp. 83-94. Berkeley, Calif.: Northern California Section of Instrument Society of America.

1959

With J. S. Brown. Absorption spectra and relative photostability of the different forms of chlorophyll in *Chlorella. Plant Physiol.* 34:305-309.

1960

With J. Myers. Evidences from action spectra for a specific participation of chlorophyll *b* in photosynthesis. *J. Gen. Physiol.* 43:723-736

1961

Light, pigments, and photosynthesis. In: *Light and Life,* ed. W. D. McElroy, pp. 447-472. Baltimore: Johns Hopkins University Press.

With J. S. Brown. The long wavelength forms of chlorophyll *a. Biophys. J.* 1:539-550.

With D. C. Fork. Computer solutions for photosynthesis rates from a two pigment model. *Biophys. J.* 1:669-681.

1963

With J. H. C. Smith. The major and accessory pigments in photosynthesis. *Ann. Rev. Plant Physiol.* 14:181-224.

1965

With W. Vidaver. Oxygen uptake and evolution following monochromatic flashes in *Ulva* and an action spectrum for System 1. *Plant Physiol.* 40:7-12.

1966

Chloroplast pigments. In *Biochemistry of Chloroplasts,* vol. 1, ed. T. W. Goodwin, pp. 377-386. London: Academic Press.

1967

With J. M. Pickett. The action spectrum for blue-light-stimulated oxygen uptake in *Chlorella*. *Proc. Natl. Acad. Sci. U. S. A.* 57:1587-1593.

1971

The distribution and action in photosynthesis of several forms of chlorophyll. *Proc. Natl. Acad. Sci. U. S. A.* 68:2893-2897.

1972

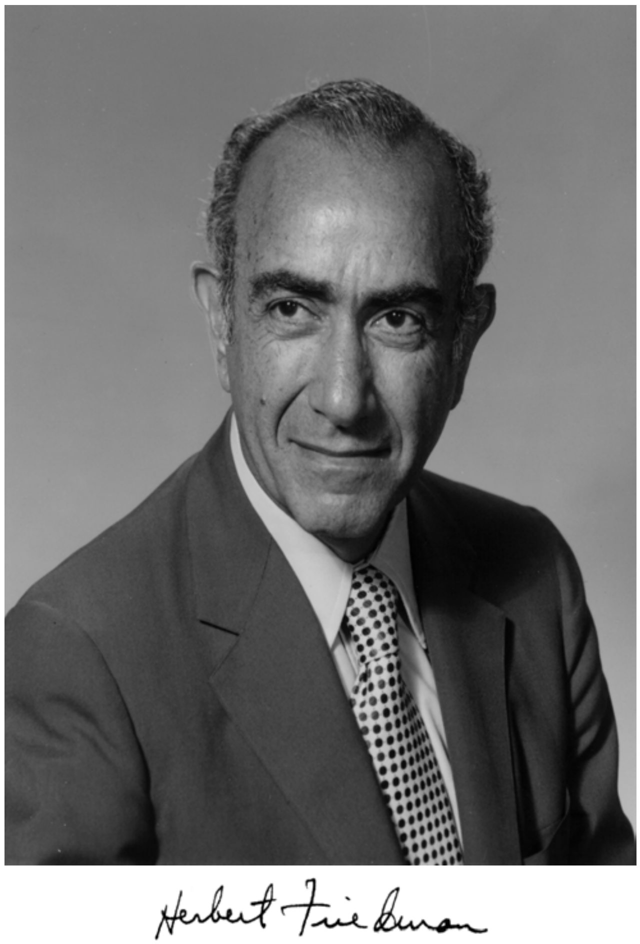
With J. S. Brown and M. C. Lawrence. Four universal forms of chlorophyll *a*. *Plant Physiol.* 49:421-429.

1973

With R. A. Gasanov. Chlorophyll composition and photochemical activity of photosystems detached from chloroplast grana and stroma lamellae. *Proc. Natl. Acad. Sci. U. S. A.* 70:2082-2085.

1979

Fifty years of photosynthesis. *Annu. Rev. Plant Physiol.* 30:1-26.



HERBERT FRIEDMAN

June 21, 1916–September 9, 2000

BY HERBERT GURSKY

"IF I WERE RICH, I would pay for the privilege of doing it," Gertrude Friedman quoted her husband as saying of the decades of research he had carried out. She was reminiscing of their life together in their home of 50 years in Arlington, Virginia. The house is well known to friends of the Friedmans, where each spring they hosted a Sunday brunch when the azaleas that overflowed their front yard were at peak bloom. Friedman died there of cancer on September 9, 2000, at the age of 84.

Herbert Friedman spent nearly his entire professional career at the Naval Research Laboratory in Washington, D.C., after arriving there in 1940 upon completion of his graduate work at Johns Hopkins University. At the NRL he had a very successful career applying X-ray analysis to the study of materials. He then pioneered the application of sounding rockets to solar physics, aeronomy, and astronomy. Later in his life he served as a statesman and public advocate for science. Friedman retired from the Naval Research Laboratory in 1980 but maintained an active association with the laboratory and the scientific community until his death. In 1996, on the occasion of his eightieth birthday, he received the NRL Lifetime Achievement Award and was named Chief Scientist Emeritus of the Hulburt Center. The Friedman Room at NRL remains as a memory of his career and stature as a scientist.

Friedman was born in Brooklyn on June 21, 1916, the second of three children of Samuel and Rebecca Friedman. His father ran a successful art-framing store on Ninth Street in Manhattan. An Orthodox Jew who closed his business on Saturday, Samuel Friedman was born in Evansville, Indiana, in 1877 and moved to New York City as a young man. Rebecca was born in Eastern Europe. Herbert grew up as an aspiring artist and developed sufficient skill as a young man to earn pocket money from the sale of his sketches. He entered Brooklyn College in 1932 as an art major but finished with a degree in physics.

Gertrude Friedman recalls no special interest in science in the Friedman family. The family environment was focused on the humanities, especially art and music. In an autobiographical note, as an example of an interest in science, Friedman mentions only having traveled to upper Manhattan in 1925 when he was nine years old to observe a total eclipse of the sun, although he did admit an interest in mathematics. No one else in the Friedman family developed any special skills as an artist, even though Samuel Friedman developed close associations with artists as part of his business. So it was something special about Friedman, developed at an early age, that led him to focus his energy so successfully, first on art, then on science.

After completing his undergraduate education, Friedman had no thought of continuing his studies, but it was still the depression and jobs were scarce. The best he might expect was teaching in a secondary school, but such positions were highly competitive. He did manage to find work as a commercial artist for five dollars a week. With such dim prospects he decided to go on to graduate school. His

Brooklyn College mentor, Bernhardt Kurrelmeyer, helped him obtain a scholarship to Johns Hopkins University, where he was given an instructor's position for $40 a month.

When Friedman began his graduate studies in 1936, science in the United States was in the midst of an explosion in basic physics research. Compton in Chicago and Millikan in Berkeley were studying the very energetic particles comprising cosmic rays; Anderson in Pasadena had discovered the positive electron, confirming one of the more bizarre predictions of quantum mechanics, and Lawrence in Berkeley was building cyclotrons for smashing atoms. Hubble in Pasadena had discovered the recession of galaxies, leading to the concept of the expanding universe. However, the bread and butter of physics was materials research. Discoveries early in the century by Rutherford and Bohr led to the modern concept of the atom. Quantum mechanics, combined with powerful new diagnostic techniques, such as the development of X-ray technology and X-ray diffraction, led to a whole industry dedicated to understanding the structure of materials, picking up where the chemists had left off.

At Johns Hopkins, Friedman conducted his thesis research under the direction of J. A. Bearden, an expert in the exploitation of X rays to study material. Bearden, a master experimenter, had built a new instrument for studying X rays using their Bragg reflection characteristics. To detect the X rays, Bearden used an ionization chamber. Friedman developed a Geiger counter as the instrument's detector, which greatly improved its sensitivity. The device had been invented only in 1928 and was just coming into general use. There is a fundamental difference between an ionization chamber and a Geiger counter, even though they are close cousins. With an ionization chamber, the exceedingly small current resulting from the interactions of X rays is recorded. In a Geiger counter the charge resulting from each X-ray photon is amplified in the gas, collected as a pulse, and counted as a single event, allowing for long measurement periods. Friedman used this instrument for his thesis work and wrote three papers studying X-ray interactions with materials, mostly metals.

Friedman was unable to find a position when he finished at Hopkins in 1939, so he remained at the university for a year as an instructor. In 1940 he was offered a position in the Metallurgy Department at NRL, but only after the intercession of the Hopkins Physics Department chair, who was apparently outraged that a Hopkins graduate would have difficulty finding a job.

NRL was still a small place, not yet swelled by wartime activities. The institution comprised no more than about 100 scientists and engineers, spread over seven research departments. The titles of his papers during the 1940s reveal the applied nature of his work and included "Lead Marking of Radiographic Films and Its Prevention," "Thickness Measurements of Thin Coatings by X-Ray Absorption," and "Determination of Tetraethyl Lead in Gasoline by X-Ray Absorption." During this time he also applied for his first of eventually 50 patents, "A Parallel Arrangement of GM (Geiger-Mueller) Counters." In 1942, because of dissatisfaction with changes in management of his research area, he verged on accepting a position at the National Bureau of Standards. Learning of this, E. O. Hulburt offered Friedman a position in his own Optics Division at NRL as the head of a newly formed section dedicated to exploiting electron microscopy and X-ray diffraction analysis. Friedman was still only 26 years old.

During this time Friedman married Gertrude Miller. She was also from Brooklyn; in fact, the two had attended Brooklyn College at the same time without ever meeting.

Herbert's sister, who was Gertrude's close friend, introduced the two in 1938. The Friedman children, Paul and Jon, were born in 1944 and 1947, respectively, and the family moved into their Arlington home in 1950.

In 1945 Friedman received the Navy's Distinguished Civilian Service Award for his wartime development of a technique for cutting and tuning RF crystals for radios by using their Bragg reflection characteristics. Up until Friedman's development, crystals were examined visually in order to find the correct orientation for installation in radio circuits. But the Japanese had bought all the good crystals on the world market in building up their armed forces. All that remained were low-quality crystals not suitable for visual examination. So Friedman took what was available and demonstrated how they could be "visualized" with X rays and converted to use in radios. The award citation stated that 50 million man-hours of effort had been saved with Friedman's technique.

Friedman also became involved in a major project of national importance, the search for airborne radioactive material as an indicator of nuclear explosions. The standard technique for the analysis of airborne particulates was then, as it still is, in situ collection on filter paper. Collecting debris from nuclear explosions required aircraft flights to carry the sampling apparatus. Friedman recognized that rainfall had the property of scouring the air of such material, which acted as nucleation centers for creating raindrops. Why go to the trouble of going up into the stratosphere to collect when nature will bring it down in the form of rain, reasoned Friedman. Combined with chemical techniques for extracting and condensing heavy elements from rainwater, this simple idea turned into a powerful means of detecting small amounts of atmospheric inclusions. The highly secret NRL project, called Project Rain Barrel, resulted in detecting the Soviet Union's first nuclear explosion in 1949. Around this time, Friedman, with his colleague LaVerne Birks, also developed the use of X-ray fluorescence as a standard tool by the materials analysis community.

These important achievements had already established Friedman as an unusually gifted scientist. But by 1949, with the encouragement of his department director E. O. Hulbert he was already shifting his primary interest to the conduct of scientific experiments from sounding rockets in order to study the sun and the upper atmosphere. Hulburt's own research had begun in the 1920s with the study of long-distance radio propagation, but by the 1930s it encompassed the physics of the upper atmosphere. Even then Hulburt recognized the potential of rockets for probing the atmosphere and resolving questions related to the ionosphere. Fortuitously Ernest Krause, an NRL physicist who had specialized in aircraft electronics, was one of the many scientists sent to Europe near the end of World War II to assess German military technology. Krause came away with an intense interest in the V-2 rockets and was instrumental in having NRL take significant responsibility for their utilization following the end of the war. With the actual opportunity to make observations from rocket altitudes, Hulburt encouraged NRL scientists to develop experiments to exploit this new capability. V-2 rockets with science payloads began flying from the White Sands Missile Range in 1946. Friedman's first rocket experiment in 1949 was designed to observe solar X-ray and ultraviolet radiation using Geiger counters. At that time fewer than half the launch attempts were successful, but with beginner's luck Friedman's rocket performed satisfactorily and he was able to obtain results up to an altitude of 150 km. The abstract to his 1951 paper published in *Physical Review* describing the results reads as follows: "Data telemetered continuously from photon counters in a V-2 rocket, which rose to 150 km at 10:00 AM on September 29, 1949, showed solar 8A x-rays above 87 km, and ultraviolet light around 1200A and 1500A above 70 km and 95 km, respectively. The results indicated that solar soft x-rays are important in E layer ionization, that Lyman a-radiation of hydrogen penetrates well below E layer, and that molecular oxygen is rapidly changed to atomic form above 100 km."

By the time of his 1949 flight, other scientists from NRL, using film and fluorescing powders, had already gathered evidence of X-ray emission from the sun. Furthermore there had been evidence from the 1930s, culminating with Edlen's identification of lines from highly ionized atoms in the 1940s, indicating that the sun had a million-degree corona. The scientific issue in 1949, as it had been for several decades, was how the structure of the upper atmosphere was created. It was Friedman's intention to understand that structure, especially the E layer, based on the ionization produced by solar radiation. Friedman's measurement was clean and quantitative, and he convinced the community that it could be used as a basis for developing a full understanding of the upper atmospheric regions. The discovery of solar X rays is widely attributed to Friedman based on this early rocket experiment.

The switch from laboratory X-ray analysis to rocket atmospheric science was not as great as might appear. The key to Friedman's rocket instrument was the small rugged gas detectors that he developed for his laboratory work, along with the associated electronics. The basic science involved in the rocket investigations, the production of X rays and their interaction with matter, were subjects he had been studying since his graduate student days.

During the next decade Friedman arranged for campaigns of shipboard rocket launches, including a series of launches from near the Pacific island of Puka Puka during the 1958 solar eclipse. He obtained the first X-ray image of the sun with a pinhole camera, flew the first Bragg spectrometer for measuring hard X rays, and developed and flew the first satellite, SOLRAD, for long-term monitoring of the sun. The observation of the 1958 solar eclipse was remarkable in showing that the solar X-ray emission extended far beyond the region of the visible sun, but was also concentrated in small regions on the surface associated with sunspots.

Around this time Friedman suffered the first episode of a life-threatening intestinal bleeding that was to plague him for the rest of his life. He went on to expand his research to stellar astronomy in 1955 with a rocket flight using collimated Geiger counters sensitive in the mid-ultraviolet that revealed significant emission associated with the Milky Way. The work represented a more general transition to astronomical observations, especially to X-ray astronomy. As a follow-up to his 1955 rocket flight, his group began a program of ultraviolet photometry of hot stars and obtained a startling observation of an apparent associated ultraviolet halo. Meanwhile part of the group that had worked with Friedman, including Albert Boggess and James Kupperian, left for NASA's Goddard Space Flight Center, where they pursued their own program of rocket astronomy, although neither the NRL nor the Goddard group were able to confirm the observation of the haloes. Friedman subsequently referred to this period of time as having held him back from pursuing the observation of X rays from stellar objects, in which he was much more interested.

Following the report of the discovery of cosmic X-ray sources in 1962, Friedman responded with a rocket flight in early 1963 that unambiguously confirmed the presence of discrete sources of X rays and of a diffuse X-ray background. In 1964 he conducted an observation of the Crab Nebula as it was occulted by the moon. But the result, although a landmark in the history of scientific rocketry, was a great disappointment to him. By 1964 a renewed interest had emerged in neutron stars, whose existence was postulated during the 1930s. These objects had been conjectured to be one of the final stages of a star, possibly born during a supernova explosion, and composed entirely of neutrons. They would be of such great density that an object the mass of our sun would only be about 10 km in diameter. If the X-ray source in the Crab Nebula had been a neutron star, its X-ray emission would have disappeared abruptly when the moon passed in front of it. Instead the X-ray emission disappeared gradually, indicating that the emission was emerging from the nebula as a whole and not from a single, small object. After the 1968 discovery of the optical and radio pulsars, Friedman finally detected the X-ray pulsations from the neutron star in the Crab Nebula. Friedman's group continued rocket observations of X-ray sources for another decade. He was also responsible for the large area proportional counter on the High Energy Astronomical Observatory, HEAO-1, that was launched by NASA in 1977.

Even though some of Friedman's most heralded scientific accomplishments occurred during the 1960s, his career went through a marked change at the beginning of that decade. NASA was created in 1958 in response to the launch of *Sputnik* by the Soviet Union. The action created turmoil at NRL because whole sections of the laboratory transferred to the new organization, including several scientists working directly for Friedman. Friedman himself was offered a senior position at the Goddard Space Flight Center, but he declined. Another effect was that NASA provided support for a broad range of programs; indeed, one of its principal initial focus areas was ultraviolet astronomy, which created competing groups.

A new division was created at NRL in 1958, with Friedman as its head and space science as its focus, combining elements of the Optics Division that Friedman had been part of and of the Rocket Sonde Division, which had lost much of its staff, including its director, Homer Newell, to NASA. Initially the new division was named Astronomy and Astrophysics, a misnomer since its activities included atmospheric science. Since 1963 it has been known as the Space Science Division.

In 1962 Friedman proposed that the National Science Foundation fund an institute to be created at NRL, the E. O. Hulburt Center for Space Research, principally dedicated to the mentoring of young scientists in space research, following which they would go out to other research laboratories and to universities. Friedman argued that "there can be little doubt that major advances in astrophysics can be achieved in the immediate future through the use of observatories in space, yet in spite of substantial funding by NASA for space science in the universities, the opportunities to enter directly into rocket and satellite astronomy programs are very limited." He further noted that the principal focus of NASA, satellite projects such as the Orbiting Solar Observatory and the Orbiting Astronomical Observatory, "are inappropriate for graduate students and in any case are too inflexible as testing grounds for unconventional and radically new ideas." He argued that because of its history of excellence in space science, by then into its second decade and the breadth of laboratory support services, the Naval Research Laboratory was uniquely situated for training scientists to participate in space research and technology. Friedman was aware of discussions that had been taking place at the President's Science Advisory Committee of the need for strengthening university research activities by taking advantage of national and Defense Department laboratories. His proposal was supported by then director of the National Science Foundation Alan T. Waterman, who had previously been the first director of the Office of Naval Research, NRL's parent organization in the Navy.

Funding of $800,000 was provided in 1963 for the creation of the center and included the purchase of rockets and the necessary staff support. At this time the NSF was dealing with the issue of balance between supporting the large national centers such as the National Radio Astronomy Observatory in Green Bank, West Virginia, and support for individual investigators. After Waterman left NSF in 1963, funding for the E. O. Hulburt Center declined, although Friedman was able to continue support for the center with NASA funding until around 1980. The center, which still exists, left a notable legacy. In its first years, support was provided to Edward Ney at the University of Minnesota, Martin Harwitt at Cornell University, and many others. Indeed, many of the current leaders in space science at the Naval Research Laboratory started their careers as Hulburt fellows.

Friedman's emergence as a space scientist was a chance event. His first choice when he left graduate school was an industrial position, applying the experience of his thesis work. But the period 1939-1940 was still the depression, and he was a physicist competing for a position in a discipline dominated by chemists. Had it been a year later and preparations for war further along, he would probably have been successful in obtaining such a position. And because he had been befriended by E. O. Hulburt, he did not leave NRL for a position at the National Bureau of Standards. Finally, it was only because the Naval Research Laboratory took the incentive for conducting science with captured V-2 rockets, that the opportunity for his space research arose at all.

His genius as a scientist lay in devising simple experiments that resolved important problems. It is hard to find other individuals with his string of success over such a broad range of scientific activities. He had little interest in large NASA-sponsored missions, even though early in his career he had been involved with and directed major enterprises, such as the campaigns to launch rockets from naval ships, and later his group built one of the instruments for the HEAO-1 X-ray satellite, which he used to survey the sky for X-ray sources and to study X-ray variability.

In the 1970s Friedman turned to writing for the general public, with the publication of his book *The Amazing Universe* in 1975, followed by *The Sun and Earth* in 1985 and *The Astronomer's Universe* in 1990. Of the latter, *Publishers Weekly* wrote, "Friedman here writes one of the most engaging popular science introductions to astronomy to come along in memory. . . A superb book."

In 1973 Friedman wrote an article, "Undirected Research," for an internal NRL publication in which he noted that when he joined NRL in 1940, he "discovered a world of problems that had never penetrated my academic consciousness." In the article he went on to describe his work on converting quartz crystals into precise crystal oscillators, the detection of Soviet nuclear bomb explosions, and his other applied activities. Never once did he mention any of the scientific results for which he is known outside of NRL. He may have been writing for his Navy sponsors, but clearly he enjoyed applying his ability as a physicist to solving difficult problems of high interest, whether it was the nature of the cosmic X-ray sources or the thickness of layer of tin on steel.

Starting in 1959, Friedman began to take an increased interest and participation in a wide range of national advisory committees, organizations, and editorial boards, starting with his membership on the Geophysical Monograph Board of the American Geophysical Union. By 1962 Friedman was also involved in four editorial positions, membership on the panel of the International Year of the Quiet Sun, was president of the Inter-Union Committee on Solar-Terrestrial Relations, and was a director of the American Rocket Society. As a recognized leader in the space sciences, Friedman was sought after by NASA as it established an advisory structure. Furthermore, his contributions to applied research and their relation to national defense programs made him a natural choice for other groups. By 1970 he was concurrently involved in 17 outside roles, an astonishing number, since active scientists rarely become involved in more than a handful of such activities simultaneously. He was always soft-spoken, but exerted his influence through the sheer strength of character and wisdom. As Frank Press said at a memorial ceremony at the American Philosophical Society, "When Herb spoke, everyone listened." Friedman also took increasing interest in helping scientists from countries with closed borders~~a~~, especially from the former Soviet Union. He was a close friend of the Russian astrophysicist Joseph Shklovsky and arranged for translation into English of Shklovsky's autobiographical essays "Five Billion Vodka Bottles to the Moon."

Friedman was a great friend of the geophysics community and played a major role in the formation of the International Geosphere-Biosphere Program, a term that he coined. Richard Goody had chaired a NASA-sponsored workshop in the summer of 1982 to discuss a major new space initiative in the area of "global habitability." The next summer, following the solicitation of opinions of interest from various groups, Friedman organized a workshop under the auspices of the National Academy of Sciences to "begin a more systematic, detailed discussion of the composition of the International Geosphere/Biosphere Program." The workshop resulted in a report presented to the International Council of Scientific Unions that August, leading in turn to its Council issuing "A Proposal for an International Geosphere-Biosphere Programme—A Study of Global Change," that was endorsed at the IGU General Assembly in 1984. A parallel effort was begun in the United States by the National Research Council with the formation of the Committee for an International Geosphere-Biosphere Program. The net result has been an astonishing number of programs and even an act of Congress, the Global Change Research Act of 1990. Richard Goody has been called the grandfather of the IGBP, but Friedman is generally regarded as its principal architect.

Friedman was elected to the National Academy of Sciences in 1960. During his long association with the National Academies, he served on 21 different National Research Council panels, committees, boards, and commissions, including the Space Studies Board, the NRC Governing Board and the Report Review Committee, and as chair of the Commission on Physical Sciences, Mathematics, and Applications. Friedman also served numerous other national and international organizations, giving generously of his time. He was a member of President Nixon's Science Advisory Committee and was appointed to the General Advisory Committee of the Atomic Energy Commission by President Johnson. Friedman was also a member of the American Academy of Arts and Sciences, the International Academy of Astronautics, and an honorary fellow of the American Institute of Aeronautics and Astronautics. In 1964 he was elected to the American Philosophical Society, where he served on a number of committees, and shortly before his death he organized a Philosophical Society Symposium called "Ballistic Missile Defense, Space and the Danger of Nuclear War."

Over his long career Friedman authored or coauthored over 300 scientific papers. His achievements were widely recognized throughout the world and included honorary degrees from the University of Michigan and the University of Tubingen (Germany). He received many awards and prizes. Among these were the President's Distinguished Federal Civilian award, the National Medal of Science, the Wolf Foundation Prize in Physics, the Eddington Medal of the Royal Astronomical Society, and the Bowie Medal of the American Geophysical Union.

Friedman's principal scientific contributions, having to do with his observations of solar and cosmic X rays, were highlighted in the 2002 Nobel Prize in physics awarded to Riccardo Giacconi for his pioneering role in the development of X-ray astronomy. In a background paper the Royal Swedish Academy of Sciences noted that X-ray astronomy began with Friedman's 1949 pioneering discovery of X rays from the sun, and commented, "The most important leading persons through the first three decades of x-ray astronomy were, independently, Friedman and Giacconi and [Bruno] Rossi. These three persons contributed crucially to the development of methods and instrumentation, but also to the application of these methods to scientific work, leading to a very rich host of important discoveries."

SELECTED BIBLIOGRAPHY

1940

With J. A. Bearden. The X ray Kb emission lines and K absorption limits of Cu-Zn alloys. *Phys. Rev.* 58:387-395.

1946

With L. S. Birks. Thickness measurements of thin coatings. *Rev. Sci. Instrum.* 17:99-101.

1949

Geiger counter tubes. *Proc. I. R. E.* 37:791-808.

1951

With S. W. Lichtman and E. T. Byram. Photon counter measurements of solar X-rays and extreme UV light. *Phys. Rev.* 83:1025-1030.

1957

With T. A. Chubb. Solar X-ray emission and the height of D-layer during radio fadeout. In *Report of the Physical Society Conference on the Physics of the Ionosphere,* pp. 58-62: London: The Physical Society.

With T. A. Chubb, R. Kreplin, and J. E. Kupperian. Rocket observation of X-ray emission in a solar flare. *Nature* 179:861-862.

With E. T. Byram, T. A. Chubb, and J. Kupperian. Far ultra-violet radiation in the night sky. In *The Threshold of Space,* ed. M. Zelikoff, pp. 203-210. New York: Pergamon Press.

1962

With R. W. Kreplin and T. A. Chubb. X-ray and Lyman-alpha emission from the sun as measured from the NRL SR-1 satellite. *J. Geophys. Res.* 67:2231-2253.

1963

With R. L. Blake, T. A. Chubb, and A. E. Unzicker. Interpretation of X-ray photograph of the sun. *Astrophys. J.* 137:3-15.

1964

With S. Bowyer, E. T. Byram, and T. A. Chubb. X-ray sources in the Galaxy. *Nature* 201:1307-1308.

With S. Bowyer, E. T. Byram, and T. A. Chubb. Lunar occultation of X-ray emission from the Crab Nebula. *Science* 146:912-917.

1966

With T. A. Chubb and R. W. Kreplin. Observations of hard X-ray emission from solar flares. *J. Geophys. Res.* 71:3611-3622.

1967

With E. T. Byram and T. A. Chubb. Distribution and variability of cosmic X-ray sources. *Science* 156:374-378.

1969

With G. Fritz, R. C. Henry, J. F. Meekins, and T. A. Chubb. X-ray pulsar in the Crab Nebula. *Science* 164:709-712.

1971

With J. F. Meekins, G. Fritz, and T. A. Chubb. X-rays from the Coma Cluster of Galaxies. *Nature* 231:107-108.

With S. Shulman, G. Fritz, J. F. Meekins, and M. Meidav. X-ray intensity fluctuations in Cygnus XR-1. *Astrophys. J.* 168:L49-L51.

1987

Sun and Earth. New York: W. H. Freeman.

1990

The Astronomers Universe: Stars, Galaxies, and Cosmos. New York: W. W. Norton.

1994

From ionosonde to rocket sonde. *J. Geophys. Res.* 99:19143-l9153.

1996

With J. B. Lockhart and I. Blifford. Detecting the Soviet bomb: Joe-1 in a Rain Barrel. *Phys. Today* 49:38-41.

Photograph by Lars Speyder

EDWARD LEONARD GINZTON

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EDWARD L. GINZTON'S MULTIFACETED career spanned an era of immense technological advances in physics, electronics, and microwaves—and of important advances in social and political issues. Throughout his long and productive life his remarkable combination of scientific skills, leadership qualities, technological foresight, and community concerns enabled him to make distinguished technical contributions and to build enduring institutions in which others could make such contributions as well.

Ginzton's scientific career began in the late 1930s when he helped develop the understanding of feedback in early vacuum tube amplifiers and worked with the pioneers who invented the klystron. It continued through his leadership in developing modern microwave technologies and megawatt-level klystron tubes during and after World War II, and in helping make possible the development of linear electron accelerators both as mile-long "atom smashers" and as medical tools still in use worldwide for cancer radiation therapy. His abilities eventually led him to take distinguished roles in both the academic and industrial worlds and in local and national community service as well.

By the end of his career Ginzton held some 50 fundamental patents in electronics and microwave devices, had received the 1969 IEEE Medal of Honor "for his outstanding contributions in advancing the technology of high power klystrons and their applications, especially to linear particle accelerators," and had been elected to the National Academy of Sciences (1966) and the National Academy of Engineering(1965). Beyond this, to borrow from the words used by photographer Carolyn Caddes in her *Portraits of Success: Impressions of Silicon Valley Pioneers*, "Ginzton [also] contributed to the growth of Silicon Valley as scientist, educator, business executive, environmentalist, and humanitarian."

1915 TO 1929: EARLY YEARS IN RUSSIA AND IN EXILE

Ginzton was born on December 27, 1915, in the Ukrainian city of Ekaterinoslav to Natalia Philapova, a Russian physician, and Leonard Ginzton, an American medical student. So far as can be determined from the confusing records available even to Ginzton himself, his father was born in Russia but as a young man emigrated first to Germany and then to America, where he became a U.S. citizen and participated in the Klondike Gold Rush of 1897. After some success in finding gold, Leonard Ginzton traveled back to Switzerland, began his first real period of formal education, and after a few years returned to Russia to study medicine and to marry Ginzton's mother in 1905.

During the following two decades Ginzton's parents, idealistic medical students and eventually doctors, were caught up in the birth of six children, only two of whom survived infancy, and in the turmoil associated with World War I, the final years of tsarist Russia, and the rise of the new Soviet state. As Ginzton later recalled in an informal autobiography:

Since both of my parents participated as medical officers on the Eastern Front, my early childhood consisted of rapid migration with the tides of war, revolution, and other similar events. Until I was 8 we did not live in any one place for more than six months, and I was not exposed to formal education until I was 11. As I was supposed to have had tuberculosis, I was [at one period] sent to the Black Sea by myself, with only occasional visits by my relatives.

In 1927 Ginzton's parents decided to leave Russia, partly in response to the tragic death two years earlier of Ginzton's only surviving sibling, Leonard. As revolution swept through the Russian empire, the Ginzton family sought refuge in the distant city of Harbin, Manchuria. Still lacking any formal education, Ginzton had a private tutor there for a period of a year, during which he learned just enough to catch up with the requirements of the Russian school in Harbin.

1929: ARRIVAL IN THE UNITED STATES AND STUDIES AT BERKELEY

In the autumn of 1929 Ginzton's father arranged for the family to emigrate from Manchuria to the United States. On his arrival in San Francisco the 13-year-old Ginzton, "knowing not a word of English," was placed in the first grade in the public schools. Less than four years later he graduated from San Francisco's Polytechnic High School and in the spring of 1933 entered the University of California, Berkeley, to study electrical engineering. In addition to his studies Ginzton joined the Reserve Officers Training Corps, hiked in the High Sierra, enjoyed amateur photography, played chess at a competitive level, and organized an intramural water polo team. His ROTC participation eventually led to a commission as a second lieutenant in the Army Reserve, but he was never called to active duty.

Graduating from Berkeley three years later in the middle of the Great Depression, Ginzton, unable to find employment, chose to continue with graduate work at Berkeley. During the following year he took further courses, did independent research on the theory of electronic circuits, and invented the "balanced feedback principle." "It was not much of an invention," Ginzton later recalled, but the circuit analysis and experimental work were enough for an M.S. degree from Berkeley in 1937 and a publication in the *Proceedings of the Institute of Radio Engineers* (1938).

1937: FIRST ARRIVAL AT STANFORD UNIVERSITY

Ginzton's subsequent application for a graduate fellowship together with his work on negative feedback at Berkeley led to a meeting with Frederick Terman, who had been working for some time to build a radio electronics curriculum at Stanford. Terman immediately offered him a teaching assistantship and Ginzton moved to Stanford in 1937 to enroll in Terman's graduate program in electrical engineering. Ginzton later recalled that in doing this,

> I became a member of a graduate class of about 15. This group was of unusually high caliber [one of Ginzton's closest friends in the group was William Hewlett] and we learned as much from each other as from formal c1asswork. We organized seminars on topics which were not being taught but which appeared to us to be of importance. We became fascinated [with] the principle of negative feedback, and much of the experimental work in this field, as well as the theory, evolved from the research of this group of students.

The students helped revise and expand an early edition of Terman's *Radio Engineering* textbook and lectured to each other from papers in the *Bell System Technical Journal*. Ginzton's research on feedback eventually led to an engineer degree thesis on applications of feedback at radio frequencies in 1938, and a Ph.D. dissertation on stabilized negative impedances in 1940.

During his first week at Stanford, Ginzton also enrolled in a course in modern physics taught by William W. Hansen, a young but very highly regarded faculty member in the Stanford Physics Department. In 1939 as Ginzton neared the end of his Ph.D. work, Hansen, with Terman's encouragement, invited him to join the Varian brothers Russell and Sigurd in continuing the development of the klystron tube, a pathbreaking microwave device invented two years earlier by Russell Varian. Ginzton explored the characteristics and potential applications of the new tube and developed new methods for making microwave measurements—activities that set him on a path to several of the major accomplishments of his subsequent career. Ginzton thus had the good fortune during his student years at Stanford to develop lifelong relationships with many microwave and electronics pioneers, including William Hewlett, David Packard, and Karl Spangenberg in Terman's laboratory, and the Varians and others in Hansen's group. He also married Artemas A. McCann on June 16, 1939. The couple subsequently had four children: Anne, Leonard, Nancy, and David.

1941 TO 1946: WAR YEARS AT SPERRY GYROSCOPE

Stanford had entered into an agreement in 1938 under which the Sperry Gyroscope Company acquired ownership of the klystron patents and opportunities to participate in continuing klystron development at Stanford in return for the promise of future royalties to the Varian brothers and Stanford. In late 1940 as World War II broke out in Europe and American involvement came to be seen as inevitable, most of the Stanford klystron group, including Hansen, Ginzton, and the Varian brothers, transferred to the Sperry plant in Garden City, New York, to continue development of the klystron for microwave radar applications.

Very soon after arriving at Sperry, Ginzton, still in his late twenties, began to demonstrate his leadership abilities, and by 1946 he was directing a staff of some 2,000 people working on klystron microwave tubes, microwave measurement techniques, and Doppler radar systems. As Ginzton later noted, "[During these] six years I invented some 40 or 50 devices, some of which were relatively important." The Doppler radar techniques developed under Ginzton's direction at Sperry introduced the basic features of many sophisticated civilian and military radars today. Even as they devoted long working hours to these developments, however, Ginzton, Hansen, and the Varian brothers continued to think about the research plans that the war had forced them to leave behind at Stanford. In Ginzton's own words, during what free time they had, the four colleagues all "dreamed together, had lots of ideas we wanted to pursue" when the war was over—including the idea of founding a company whose directions and objectives would be set by scientists and not by businessmen.

1943: STANFORD'S POSTWAR PLANS

Others back at Stanford had similar thoughts about postwar opportunities. In the late 1930s some of Stanford's academic leaders had come to recognize that their university, though a respected regional institution, was not one of the top 10 among American universities. A 1938 article in the *Atlantic Monthly* ranked Stanford with Penn, Illinois, Iowa, and Ohio State as competing for twelfth place in national rankings. A group of senior academics led by Donald Tresidder, the unusually young and energetic president of the Board of Trustees, thus began around 1940 to formulate plans for bringing Stanford to a stature on the West Coast comparable to Harvard, MIT, and other major universities on the East Coast.

These plans resonated with Frederick Terman, who had struggled to develop his own radio research laboratory activities at Stanford in the late 1920s and 1930s with very meager resources before heading east to head the Radio Research Laboratory at Harvard during World War II. When Tresidder became president of Stanford in 1943, he along with Terman and others, set out to build postwar "steeples of excellence" (Terman's phrase) at Stanford, taking advantage of new technologies and new government and industrial funding that would become available as an outcome of wartime experiences.

Hansen, spending the war years in East Coast laboratories, had also proposed to his widely dispersed Physics Department colleagues that following the war Stanford should set up an interdisciplinary laboratory to continue the advances that had grown out of the prewar invention of the klystron and wartime developments in microwave technology, and to exploit these for both scientific and technological purposes. Felix Bloch, who first came to Stanford as a refugee from Hitler's regime in the early 1930s and was back after serving in a variety of wartime positions, agreed that Hansen's microwave laboratory was a good idea scientifically as well as technologically. He was equally eager to bring Hansen, whom he greatly respected as a physicist, back to Stanford. The Stanford Board of Trustees responded to the urgings of Tresidder and Terman, who had returned from Harvard to become dean of engineering, and in 1945 approved the creation of a microwave research laboratory as part of Stanford's School of Physical Sciences, with close ties to the School of Engineering. Hansen, already back at Stanford since 1944, and Ginzton, still at Sperry, were appointed as director and assistant director of the new laboratory.

1946 TO 1949: LOADED WAVEGUIDES AND MEGAWATT KLYSTRONS

By March 1946 Hansen had returned to his faculty position in physics, and Ginzton to a new junior faculty position in the same department. Because of concerns by some over his purely engineering background, Ginzton was appointed assistant professor of applied physics rather than physics, with a parallel appointment in electrical engineering. He was promoted the following year to associate professor of applied physics. Marvin Chodorow, who had become a colleague at Sperry, also joined them as a physics faculty member.

As part of his initial teaching and research activities Ginzton developed a comprehensive family of microwave measurement tools, "making our laboratory the best of its kind in the world," while Chodorow developed course and research activities in microwave electronics. But most of all, Ginzton, Hansen, and Chodorow were seeking to accelerate electrons using those microwave tubes. The Stanford Physics Department's interest in X rays and in generating energetic particles to explore nuclear physics dated back to the 1920s and early 1930s. Hansen's invention of the microwave cavity resonator in 1936 had been partly motivated by a desire to find a cheap method of obtaining high-energy electrons. This motivation remained strong and was shared by Ginzton following World War II. Others around the world had similar goals, though many of these groups thought the more interesting results in nuclear physics would come from accelerating heavier particles, such as protons or ions.

The Stanford group was focused, however, on the acceleration of electrons using "loaded waveguide" linear accelerator structures with a diameter of a few inches, down which microwaves and electrons could travel in perfect synchronism at just infinitesimally less than the velocity of light.

The electrons, surfing on the crests of the microwave cycles and continually pushed forward by the microwave fields, thus gained energy, and converted this energy into mass as they traveled. Linear accelerators offered two great advantages over other schemes for accelerating particles: Since the electrons traveled in straight lines rather than curved paths, they did not continually lose energy to synchrotron radiation; and the accelerator itself could be built in the form of individual modules perhaps 10 feet in length, which could then be cascaded to almost unlimited lengths and energies. Such a linear accelerator, perhaps 200 feet in length and driven by sufficient microwave power, could be made to accelerate electrons to a billion electron volts (1 GeV), and these electrons could then be used as probes to study the still largely unknown interior structures of the nuclei of atoms.

They already had the waveguide structure they needed: a cylindrical copper pipe 3.5 inches in diameter containing transverse copper disks 1/4 inch thick and 1 inch apart with a 1-inch hole in the center of each disk for the electron beam to pass through. The microwave fields in this structure were analyzed by E. L. Chu and Hansen in early 1947, and later that year Ginzton, Hansen, and W. R. Kennedy prepared a remarkably prescient 20-page paper describing exactly how a few hundred feet of this pipe, driven by several hundred megawatts of microwave power at 3 GHz, could be used to accelerate electrons up to 1 GeV. There would be no problem in steering the electrons through thousands of such holes in succession, they found; to the relativistic electrons the whole pipe would appear to be only a few inches long. Their paper, submitted in November 1947 and published under the title "A Linear Electron Accelerator" in the February 1948 issue of the *Review of Scientific Instruments*, laid the foundation for several generations of electron linear accelerators that continue in operation today and that have provided the primary tools for at least half a dozen Nobel Prizes.

Still unsolved at this point, however, was the problem of how to generate the hundreds of megawatts of microwave power required to drive such a linear accelerator. As a result of intensive development during the war years, pulsed magnetrons that could generate peak pulse powers of at least a few megawatts were widely available by the end of the war. Distributed injection at multiple points along a lengthy accelerator pipe required, however, that the injected microwave signals originate from a single weak but very stable master oscillator and then be amplified to multimegawatt levels by individual microwave amplifiers at each injection point. This was something that magnetrons could not do. They could function very well as small, powerful, and highly efficient pulsed oscillators—just the thing, it was later realized, for microwave ovens—but not as clean and stable amplifiers.

It was Ginzton who realized early on that the klystron could potentially provide the needed megawatt power levels. As of 1946 most klystrons produced power outputs from a few tens of milliwatts to a few tens of watts, although during the war years Ginzton had seen in England a few klystron amplifiers with pulsed power outputs of 20 kilowatts. Ginzton had the bold vision that klystron amplifiers could be made to deliver not just tens of kilowatts but tens of megawatts from a single tube—and moreover that he could make the required leap of 1,000 times or more in power output in a single step, rather than a lengthy sequence of many smaller steps. Success in achieving this goal would very likely make possible Hansen's linac (linear accelerator) and its goal of GeV electrons. None of the necessary components for Ginzton's klystrons existed at the time, however, much less the klystrons themselves, and many believed that they could not be made.

"WE HAVE ACCELERATED ELECTRONS"

Pulsed magnetrons, although fundamentally unsuitable for longer linacs, could be and soon were used for the first tests of the group's linac concept. By the time the linear accelerator analysis was completed in 1947, Hansen working with three students, had already assembled a single 10-foot section of his pipe, later known as the Mark I linac. Using a single 750-kilowatt magnetron he and his assistants generated 4.5-MeV electrons. His subsequent contract report to his Office of Naval Research sponsors is said to have contained just four words: "We have accelerated electrons."

Based on these results, together with Ginzton's initial designs for megawatt klystrons, in March 1948 the group submitted a detailed proposal to the Office of Naval Research for what would eventually became the Stanford Mark III accelerator: a building some 220 feet long containing an accelerator 160 feet long, with one of Ginzton's still nonexistent 20-MW klystrons every 10 feet along its length, all to be completed in two and a half years by a staff of five key people and for a budget of $951,000. Their audacious proposal was accepted, and active development of the accelerator and the klystrons went into full swing later the same year.

Megawatt power outputs required not only klystron tubes scaled up by factors of between one thousand and one million times but also power supplies that could deliver several hundred kilovolt volt pulses with peak currents of tens to hundreds of amperes, and associated structures that could stand these voltages and currents—components that simply did not exist at the time. Ginzton's team of students and technicians failed on their first two tries: Vacuum windows failed and insulators were unable to stand up to the megawatt peak powers. In March 1949, however, on their third try one of their klystrons operated as planned, delivering 14 MW. Succeeding tubes delivered steadily higher powers and more reliability, and by October 1949 one of Ginzton's klystrons was used to operate a Mark II prototype linac that delivered 40-MeV electrons from a 14-foot section of the loaded waveguide.

Just as success for the Mark III project came in sight, however, Hansen died very suddenly and unexpectedly, in May 1949, only a few weeks after learning of his election to the National Academy of Sciences. Hansen had struggled with serious lung disease for many years, and had worked throughout the preceding few years with an oxygen tank at his side and wearing an oxygen mask that he had built himself. It suddenly became Ginzton's responsibility to complete the entire project, taking over as director of the Microwave Laboratory with full responsibility for both the accelerator and the klystron aspects of the work.

Under Ginzton's direction the first few sections of the Mark III operated in the accelerator's new building a year and a half later, delivering 75-MeV electrons from 30 feet of waveguide driven by three klystrons, each operating at 8 MW. By April 1951 as more klystrons were produced, the accelerator had been extended in successive 10-foot steps to 80 feet and was delivering 180-MeV electrons—enough to begin serious research.

1948: FOUNDING OF VARIAN ASSOCIATES

During this same period, as Sigurd Varian became the last of the Stanford klystron group to return to the West from Sperry, Ginzton joined with the Varian brothers and others from the Stanford and Sperry groups in founding their long-planned enterprise. Varian Associates was established with $22,000 of capital and six full-time employees, and its first board meeting was held in April 1948. Besides Ginzton and the Varian brothers, the initial board members were William W. Hansen, Paul Hunter, Richard Leonard, Stanford physics faculty member Leonard Schiff, H. Myrl Stearns, and Russell Varian's wife, Dorothy; other associates were Marvin Chodorow as a consultant and employees Don Snow and Fred Salisbury. The group chose the name Varian Associates because Russell Varian was well known in the scientific community as the inventor of the klystron, adding the term associates to indicate that the group wanted to create a science-based company, managed by scientists, where the decisions would be made by the scientists and engineers who carried out the work.

In addition to his membership on the new venture's board of directors, a position he would retain until 1993, Ginzton worked to establish the objectives and help guide the activities of the new company. Five years later, in 1953, Varian moved from its initial leased facilities in San Carlos to a new building on Stanford land, becoming the first tenant in what would later become the Stanford Industrial Park. A decade later Ginzton was to take over full direction of the company's growth and development.

During this same period, Ginzton also developed one of the first graduate courses in the art of microwave measurements, teaching it full time from 1946 through at least 1953 and intermittently thereafter, and publishing his widely recognized text on *Microwave Measurements* through McGraw-Hill in 1957. Harried faculty members from any era may appreciate a quote from Ginzton's memoirs, recalling for his children and grandchildren those first years as a new faculty member:

I was teaching my first class in microwaves. I was building a house. I was consulting with Hewlett-Packard. I was supervising about a dozen graduate students. Supervising the construction of a building [for] the Microwave Laboratory, the new building. Seeking money from the government and from industry to continue the work. Teaching Litton and Varian and Eimac and GE and RCA how to build microwave tubes.

THE EARLY 1950'S: THE MARK III LEADS TO A NOBEL PRIZE

In the early 1950s as the success of the Mark III linac began to appear more certain, attention turned to the physics that could be done with Hansen and Ginzton's machine. In 1950 Robert Hofstadter joined the Stanford physics faculty then led by noted theoretician Leonard Schiff, and the two physicists began a serious examination of how they could study atomic nuclei by observing the scattering of the high-energy electrons that would be generated by the evolving accelerator. In 1951 the University of California became embroiled in a faculty-administration confrontation when the university's Board of Regents yielded to the state's conservative legislature and insisted that an oath of loyalty be required of all university staff. The promising young particle physicist and skilled experimenter W. K. H. Panofsky, who had at that point spent several years at Berkeley helping Luis Alvarez build huge proton accelerators out of surplus radar transmitters, was willing to sign the oath himself. Panofsky insisted, however, that rights of nonsigners must be respected, and viewed the dismissal of nonsigners as a violation "of all that is true about academic freedom and tradition in the European sense." When this nonetheless happened, Panofsky felt he could no longer stay at Berkeley, and elected to try his hand at the rival university across the bay.

Not long after Panofsky arrived, the basic structure of the Mark III accelerator was completed, and in November

1953 the Mark III generated 400 MeV electrons along its full length, with 14 out of 21 potential klystrons operating. Hofstadter was by then already carrying out pioneering measurements of nuclear scattering, using 150 to 200 MeV electrons that were siphoned out of the accelerator at a midstation halfway along the pipe. By the end of 1953 Panofsky and Ginzton had also worked out new organizational and physical solutions to the Mark III's needs and difficulties. Together they created an umbrella W. W. Hansen Laboratories of Physics within which Ginzton continued as director of the Microwave Laboratory, concentrating on klystron development and on his emerging interests in high-power traveling wave tubes, while Panofsky, as director of a new High Energy Physics Laboratory, or HEPL, assumed leadership of accelerator development efforts and plans for particle physics research using the Mark III. The original goal of 1 GeV was ultimately achieved by the Mark III in 1960 and extended to 1.2 GeV in 1964. The most notable contribution of Ginzton and Panofsky's Mark III was its use by Professor Robert Hofstadter to measure the size and charge distribution of the proton, the neutron and several heavier nuclei, fundamental results for which Hofstadter was awarded the 1961 Nobel Prize in physics. Within less than a decade the Mark III, as pioneering and productive as it was, became only the infant from which ultimately grew the very much larger and even more productive 2-mile Stanford Linear Accelerator Center.

LINEAR ACCELERATORS FOR CANCER THERAPY

In addition to his leadership of the Mark III project and the related klystron developments, Ginzton also supervised the construction of some 10 other microwave linear accelerators during this period. As early as 1953 Ginzton had recognized the potential application of linear electron accelerators for radiation therapy, and had joined with physician Dr. Henry Kaplan of the Stanford Medical School to explore the use of energetic electrons for the treatment of cancer. In 1954 as the Mark III accelerator became increasingly dedicated to physics experiments, Ginzton began construction of a 20-foot, 80-MeV Mark IV accelerator intended for experiments on improved accelerator components. Studies of beta-ray cancer therapy using this machine were immediately successful, and the first clinical medical linac, a 6-foot, 5-MeV accelerator, went into regular use soon afterward in the university's hospital in San Francisco.

During the mid-1950s Ginzton and his colleagues also constructed a 10-foot, 35-MeV accelerator for cancer therapy at Michael Reese Hospital in Chicago, a 20-foot, 60-MeV linac for cancer research at Argonne National Laboratory, and several research accelerators including a 6-foot, 5-MeV linac for medial research at General Electric. After taking over the leadership at Varian six years later, Ginzton continued to crusade for the use of small accelerators in cancer treatment and steadfastly supported many years of related but unprofitable development work, which ultimately led to a line of small electron linacs called Clinacs. By the time of Ginzton's death, some 4,000 of these had been installed in hospitals around the world and were treating over 1 million patients annually. These machines were a source of great satisfaction to Ginzton because his father had died of cancer.

1953: AN UNPLEASANT ENCOUNTER WITH SECURITY CONCERNS

In 1951 Ginzton along with a dozen others at Stanford received a Secret clearance for his government-sponsored work on high-power microwave tubes. In September 1953—some eight months after Joseph McCarthy opened his campaign alleging massive Communist subversion in the U.S.

government with a fiery speech in Wheeling, West Virginia—Ginzton applied for a similar clearance for his expanding responsibilities in the same area at Varian. Five days later the Western Industrial Personnel Security Board denied Ginzton's application, citing his alleged unreliability in handling classified documents, his alleged omission of names of relatives residing in the Soviet Union, and his "close associations with [unnamed] individuals identified with Communistic or related movements." Two weeks later the board revoked Ginzton's existing Stanford clearances as well, based on the same charges.

The first of these charges apparently stemmed from a series of confused events in the handling of Hansen's belongings and his personal and classified papers by Hansen's family and the Stanford Physics Department following Hansen's sudden death four years earlier. With regard to the second charge, Ginzton had in fact listed his only relative—an elderly aunt—known to be still alive in Russia. As regards the third, since no specific individuals were identified by the security board, Ginzton could only reply that the only individuals known to him that could possibly be characterized in this fashion were Frank Oppenheimer, an instructor with whom Ginzton had shared an assigned office as a graduate student in 1939, but whom he had not encountered since; a Stanford physics M.S. student and anti-Korean-War activist with whom Ginzton had had a few brief interactions in his faculty role during 1946-1949; and the noted San Francisco artist Emmy Lou Packard, also known as an early acquaintance of Diego Rivera and Frida Kahlo and as a social activist. The Ginztons had met Packard on a few occasions through their long-standing interest in modern art, and had purchased some of her paintings.

These actions greatly hampered Ginzton's ability to carry out his responsibilities both at Stanford and at Varian. To protect Varian's interests Ginzton immediately resigned from its board of directors, along with Schiff and Terman, who faced similar accusations. To assist Ginzton in his appeals against the clearance denials, Stanford brought in its noted attorney and alumnus Robert Minge Brown, who would later become president of its Board of Trustees, and Fred Glover, the widely respected administrative aide to Stanford's president, Wallace Sterling. Under Brown's guidance Ginzton and Glover testified at a daylong appeals board hearing in San Francisco on November 19. During Glover's testimony it emerged that Glover himself was a commander in the Naval Reserve with five years active experience in naval intelligence, including three years in counter-intelligence in San Francisco and two years as director of naval intelligence in Europe. Four days later the hearing board, possibly outgunned, responded: "The Appeal Division has determined that . . . the granting of clearance to you for access to classified security information is clearly consistent with the interests of national security."

THE MID 1950'S: PROJECT M

By the middle of the 1950s with the Mark III's high-energy physics program well underway, Hofstadter, Panofsky, Ginzton, and a number of their colleagues began to consider the possibility of building a very much larger electron linear accelerator at Stanford. The concept of such a machine apparently originated in 1954 when Robert Hofstadter, sitting in Leonard Schiff's living room with Felix Bloch, Ginzton, and Schiff, proposed building a multi-GeV linac to provide electrons of shorter wavelengths, which could probe still deeper into the nucleus. Panofsky and Ginzton quickly took leading roles in exploring the feasibility of building a 2-mile-long multi-GeV electron accelerator—20 to 30 times larger than the Mark III in both size and energy output, and initially referred to as "the Monster"—on the Stanford campus. These numbers were determined in part by their assessment of the energies needed to do significant nuclear studies, along with the capabilities of the linac technology, but as Ginzton later recalled, "The length was 2 miles simply because that was the longest straight path we could identify on the map of the Stanford lands." Ironically, as the administrative and organizational problems associated with building and operating such a facility in an academic setting began to emerge, Hofstadter elected to divorce himself both from the project itself and from making use of the resulting accelerator for scientific work, and eventually redirected his own efforts into other areas of physics.

The formal birth of Project M took place in April 1956 when a group of some dozen Stanford physicists and staff members gathered at Panofsky's home to discuss the possibilities and implications of such a project. Ginzton was named as director of the new project, a position he retained from 1956 through 1960, with Panofsky as deputy director, and Schiff and Hofstadter as consultants. An augmented and largely volunteer group of Stanford physicists and engineers then began to study the practicality, usefulness, and costs of an accelerator 2 miles in length. During subsequent months Ginzton led the efforts to lay out the design of the accelerator, while Panofsky formulated the research program it would be intended to accomplish. In April 1957 these efforts led to a formal $100 million proposal by Stanford to the Atomic Energy Commission, the National Science Foundation, and the Department of Defense for the construction of the proposed accelerator on Stanford land.

With the proposal off to Washington, Ginzton was able to spend a long-delayed sabbatical year in Geneva in 1957-1958, during which he visited many electronics laboratories in Europe to give lectures on microwave technology and on accelerator design. Working in an office at CERN, Ginzton was also able to complete his McGraw-Hill book on *Microwave Measurements*. Also of importance were visits to hospitals in London to discuss the treatment of cancer with electrons and X rays. In 1958 advisory panels convened by the National Science Foundation and the Atomic Energy Commission recommended that the project be funded, the first step in an approval process that was eventually to take almost four years. Ginzton and Panofsky then testified in several rounds of congressional hearings in 1959 and 1960, attempting to obtain congressional approval for construction on the Stanford campus of the world's highest-energy electron accelerator. After Ginzton stepped down as project director in 1960 to assume full-time leadership of Varian Associates, Panofsky took over as director of the renamed Stanford Linear Accelerator Center, or SLAC. Following lengthy and sometimes contentious debates both in Washington and on the Stanford campus, the proposal to establish SLAC and build the accelerator under Panofsky's direction was approved by Congress in September 1961. Once the accelerator itself was completed, essentially on time and within budget, in February 1966, Ginzton and Panofsky's machine became the source of many new discoveries in high-energy physics, and by the time Panofsky retired as its director in 1984, SLAC could log two Nobel Prizes earned "on his watch," with several others to come in later years.

THE 1960'S: TRANSFER TO THE LEADERSHIP OF VARIAN

Throughout the 1950s even while carrying heavy responsibilities at Stanford, Ginzton had continued as a member of the Board of Directors and of the Executive Committee of Varian Associates, and had become increasingly active in its management. In 1959 Russell Varian, who was chairman of the Varian board, died suddenly, and Ginzton was immediately elected to take his place as chairman and chief executive officer. Ginzton, who at the time was the director of both the Microwave Laboratory and the emerging Project M, felt a deep commitment to both of these projects, especially the completion of the proposed SLAC machine. His colleague "Pief" Panofsky, however, possessed highly regarded capabilities both in guiding the construction of large research facilities and in the basic physics to be done using them. After much soul searching, Ginzton made the choice to resign from his Stanford positions and accept the responsibilities at Varian, rather than continuing to lead Project M at Stanford. He continued on, however, as a special consultant to the president of Stanford on the construction of SLAC from 1960 until 1966, and until 1968 as a professor of applied physics on leave.

Following his transfer to Varian, Ginzton became active both in management and in developing longer-range objectives for the company, holding the title of president from 1964 to 1968 and of chief executive officer until 1972. Under Ginzton's leadership, Varian continued its success in the commercial development of new areas of basic physics, notably in analytical products, such as mass spectrometers, atomic absorption instruments, gas and liquid chromatographs, and visible and ultraviolet spectrometers. It also expanded its partnership role with overseas companies and explored new areas in medicine and in solar energy. Ginzton retired as chairman of the Varian board in 1984, but remained as a member and as chair of its Executive and Nominating committees until 1993.

Ginzton particularly supported the continuing development of nuclear magnetic resonance technology, or NMR, at Varian. NMR was another fundamental new area of physics that had germinated during Ginzton's early years at Stanford. The first experimental observations of NMR were independently carried out during the immediate postwar period by Felix Bloch's group at Stanford and Edward M. Purcell's group at Harvard, leading to a shared Nobel Prize in physics for Bloch and Purcell in 1952. In their earliest observation of this phenomenon as published in 1946, Bloch and Hansen in fact apparently, although quite unwittingly at the time, became the first researchers ever to observe a man-made population inversion and the associated phenomenon of stimulated emission—the physical phenomena underlying all subsequent developments in masers and lasers.

Even before the founding of Varian Associates in 1948, Russell Varian, occupying a cramped desk as an unpaid research associate in the Stanford Physics Department, had foreseen the possibilities of nuclear induction and NMR both for chemical analysis and for the precision measurement of geomagnetic fields, and had persuaded the Stanford researchers to obtain patents on their new technology. He also acquired commercial rights in these patents and in additional improvements that he himself made. Varian Associates even in its first year of operation had begun to develop nuclear magnetic resonance spectroscopy and nuclear induction magnetrometry as future commercial products, thus laying the foundation of what eventually became the Varian Instrument Division. Under Ginzton's leadership this early venture into NMR eventually became one of Varian's most profitable divisions and made Varian the leading manufacturer of NMR instruments worldwide.

LATER YEARS: CIVIC AND COMMUNITY LEADERSHIP

In addition to his leadership roles in technical and business affairs, Ginzton had strong interests in bettering his community and played a major role as a leader in championing fair housing and clean air before they became fashionable. He was founder and cochair with David Packard from 1968 to 1972 of the Stanford Mid-Peninsula Urban Coalition, an organization that helped launch minority-owned small businesses, and continued as a member of its Executive Committee until 1974. As a member of the Board of Directors of the Mid-Peninsula Housing Development Corporation beginning in 1970, Ginzton worked on community education and health issues and supported efforts to meet the need for affordable housing.

Ginzton was supported in these efforts by his wife, Artemas, who was active in her own community and conservation efforts, especially on behalf of trails, hostels, and the preservation of unrecognized architectural masterpieces. With an appreciation for the land, an eye for the unusual, and an unconventional sense of opportunities, she worked on projects including the Santa Clara County master plan for trails, a system of bicycle trails along California aqueducts, and the conversion of abandoned Pacific coast lighthouses into hostels.

Ginzton also served as a director of the locally founded Stanford Bank from 1967 to 1971, a member of the Advisory Board of the Mid-Peninsula Region of the Union Bank from 1971 to 1973, and a member of Northern California Advisory Board of the Union Bank from 1973 to 1981.

In his later years Ginzton also responded to both of the universities at which he had studied, serving on the Advisory Committee for the School of Business Administration at the University of California from 1968 to 1974 and the

Lawrence Berkeley Laboratory Scientific and Educational Advisory Committee from 1972 to 1980. At Stanford he served as chair of the Advisory Board for the School of Engineering from 1968 to 1970; as a member of the Board of Directors of the Stanford University Hospital from 1975 to 1980; a member of the board of the university's National Bureau of Economic Research from 1983 to 1987; and a member of the Stanford Synchrotron Radiation Laboratory's Science Policy Board from 1985 to 1990. He also served for two terms as a member of the university's Board of Trustees from 1977 to 1985.

HONORS AND AWARDS

Ginzton had been a member of the Institute of Radio Engineers, or IRE (now the Institute of Electrical and Electronics Engineers, or IEEE) since his student days in 1936 and was elected a fellow of the IRE in 1951. He received the Morris Liebmann Memorial Prize from the IRE in 1957 for his contributions in the development of megawatt-level klystrons, and the IEEE Medal of Honor in 1969 for his overall accomplishments in the development of microwaves. He subsequently served as a member of its Board of Directors from 1971 to 1973, and chaired its Awards Board from 1970 to 1972 and its Long Range Planning Committee in 1973 and 1974. He was also a member of the U.S. National Committee of the International Union of Radio Science (URSI) from 1958 to 1968.

Ginzton was elected to the National Academy of Engineering in 1965, the National Academy of Sciences the following year, and the American Academy of Arts and Sciences in 1971, and subsequently gave extensive service to all of these groups. This included serving as a member of the NAE Council from 1974 to 1980, and chairing, from 1971 to 1972, the NAS Committee on Motor Vehicle

Emissions, a group created to advise Congress on the technological feasibility of the Clean Air Act of 1970. From 1973 to late 1974 he served on the Coordinating Committee for Air Quality Studies of the NAS, and in 1975 was a member of an NAS committee to advise the U.S. Environmental Research and Development Agency, or ERDA, on the creation of the Solar Energy Research Institute. Later that year he became cochair with Harvey Brooks of the Committee on Nuclear and Alternative Energy Systems, charged with recommending to ERDA plans and strategies for the energy future of the United States.

Ginzton also traveled with National Academy of Sciences delegations to Hungary in 1966, Bulgaria in 1972, and the U.S.S.R. in 1973 and 1975, and served on NAS Committees on International Relations from 1977 to 1980, on Scientific Communications and National Security from 1982 to 1984, and on the Use of Laboratory Animals in Biomedical and Behavioral Research from 1985 to 1988.

THE EDWARD L. GINZTON LABORATORY

By the mid-1970s Stanford's Microwave Laboratory, the direct descendent of Hansen and Ginzton's initial efforts, had become well established in many new areas of applied physics under the direction of Marvin Chodorow, with widely recognized accomplishments in quantum electronics, lasers and nonlinear optics, acoustic and scanning microscopy, fiber optics, and superconducting materials. In 1976 the laboratory was formally renamed the Edward L. Ginzton Laboratory in recognition of Ginzton's many contributions to its earlier history and to the developments at Stanford that his accomplishments had made possible. Two decades later its sister laboratory, the High Energy Physics Laboratory, or HEPL, was renamed the Hansen Experimental Physics Laboratory.

AVOCATIONS

During his active years Ginzton devoted his leisure time to outdoor activities, including skiing, sailing, and hiking, all of which he shared with his children, and to avocations that included a deep and lifelong personal interest in photography and the restoration of vintage automobiles. At the time of the Carolyn Caddes portrait mentioned below, he had three Model A Fords in his garage awaiting restoration, although his ultimate pride and joy was a 1929 Packard Phaeton sedan, a car of the same vintage as his own arrival in California.

With various members of his family he also traveled widely, including flying over Africa in a hot-air balloon and attending a banquet in the Saudi Arabian desert. Other round-the-world journeys took them to Machu Picchu, the Great Pyramids and the Sphinx, the Great Wall of China, and down the Glen Canyon of the Colorado River.

His interest in photography began in childhood when he prepared his own chemicals and even coated his own photographic printing paper. These early interests were strongly reinforced during the 1940s, when his wife, Artemas, presented him with a course of studies at the Museum of Modern Art in New York under Ansel Adams. This led to a longtime friendship with Adams and an extensive collection of Adams prints. As a photographer, he was a classicist, preferring black and white to color and large-scale-view cameras to 35 mm. He continued to maintain a personal darkroom in his home in retirement, spending many hours on printing to produce the effect that he originally thought would be most satisfying for each image.

PHILOSOPHY AND BELIEFS

Ginzton's continuing accomplishments during his lifetime clearly stemmed not only from his outstanding technical abilities and his devotion to his work but also from his ability to attract others to join with him in important enterprises, his remarkable foresight and vision, and his social concerns. In the interview that accompanied his retirement portrait Ginzton told Carolyn Caddes, "Grow and become educated, but do not equate professional training with education. Try to learn how to think. Attempt to do what you want to do. Making a living is not enough."

Ginzton's commitment to cooperation with others might be symbolized by the second word in the original name of Varian Associates. Throughout Ginzton's career his associates first at Sperry, then at Stanford, and finally at Varian spoke of his collegial management style, which encouraged and stimulated those around him to work at a high level of accomplishment. His vision and his technical foresight are exemplified by a brief but remarkably comprehensive summary of the future applications of microwaves in both basic science and technology that Ginzton wrote in 1956, in which he noted:

> Many of us are so immersed in the ever-narrowing branches of electrical engineering that it is difficult to take stock of the accomplishments in the field as a whole or to visualize the possibilities and limitations of future developments. For those of us engaged in teaching and research . . . such an assessment is necessary if we are to guide our students properly and anticipate the probable roles of our own specialties . . .
>
> It is evident that the applications of present microwave knowledge will continue to grow, both in number and diversity; but despite the daily invention of novel applications, we must not become complacent. Every field of research has a finite half-life . . .

Keeping the importance of basic research in mind, those of us who have specialized in this field must anticipate either more prosaic engineering applications or a change to some other branch of science. Many will remain to explore and exploit the possibilities for which the foundation is now laid; but some will think of exploring the higher regions of frequency lying beyond the microwaves.

The study and generation of still shorter wavelengths appears as fascinating and promising today, as the microwave region appeared in 1936. Now, as then, there are many practical difficulties, challenging to the imagination and ingenuity of human skill but which offer, for the scientific adventurer, unknown rewards.

Those of us who have had the good fortune to participate in the opening up of 22 infrared and optical regions "beyond the microwaves" made possible by the invention of the laser—an invention that occurred only four years after these words were written—can only admire their wisdom.

At time of his death on August 13, 1998, Ginzton was survived by his wife of 59 years, Artemas McCann Ginzton, and his children, Leonard of La Canada, California; David of Sandpoint, Idaho; Nancy of Los Altos Hills, California; and Anne (Cottrell) of Berkeley, California. It seems appropriate to close this memorial with the same words as in Edward Barlow's National Academy of Engineering memorial tribute to Ginzton: "He was truly a man of broad interests and large and persistent vision, who enjoyed life to the fullest and cared about his family, his associates, and his community."

The results of that vision and that caring persist today, in major institutions and in smaller personal memories, across the Silicon Valley landscape and around the world.

NOTES AND REFERENCES

Much of the biographical information in this memoir (and inevitably some of the wording as well) has been taken from various press releases and biographies in the files of the National Academies and Varian Inc.; from an IEEE Legacy profile of Ginzton in a booklet distributed at the IEEE Annual Banquet in 1969, when Ginzton was awarded the IEEE's Medal of Honor; from Ginzton's 1984 interview with the Oral History project of the IEEE and a brief autobiographical sketch prepared by Ginzton himself in April 1989; from an obituary in the January 1999 issue of *Physics Today* prepared by Ginzton's longtime colleague Karl L. Brown of SLAC; and from the NAE memorial tribute for Ginzton prepared by Edward J. Barlow and published in *Memorial Tributes: National Academy of Engineering* (vol. 10, pp: 100-105, National Academy Press, 2002). A brief but rewarding biographical note accompanying a notable black-and-white portrait of Ginzton in retirement can also be found in Carolyn Caddes's *Portraits of Success: Impressions of Silicon Valley Pioneers* (Palo Alto, Calif.: Tioga Publishing, 1986).

A large amount of archival material related to Ginzton and his career can be located (though not directly accessed) through the *Online Archives of California* (OAC) at www.oac.cdlib.org/, including links to material in the Stanford University Archives and the Special Collections of the Stanford University Library and the Varian Associates Records in the Bancroft Library of the University of California at Berkeley. Carolyn Caddes's interview notes and negatives for her volume are also stored in the Stanford University archives. Reminiscences of Ginzton in interviews by several of his professional colleagues, including Marvin Chodorow, Bill Rambo, and Mike Villard, along with the IEEE Legacy and Ginzton's own 1984 interview mentioned above, can be accessed online by searching on "Ginzton" at the IEEE Web portal (www.ieee.org/portal/index.jsp).

More detailed information on Ginzton's career at Stanford, especially the postwar developments that brought Hansen and Ginzton back to Stanford, the subsequent founding of SLAC, and the controversies over academic and science policies that ensued, can be found in C. Stewart Gillmor's definitive history *Fred Terman at Stanford: Building a Discipline, a University, and Silicon Valley* (Stanford University Press, 2004) and to a lesser extent in Rebecca S. Lowen's very inaptly titled *Creating the Cold War University: The Transformation of Stanford* (University of California Press, 1997). Much of the information relating to Ginzton's collaboration with W. K. H. Panofsky comes from a tribute to Panofsky presented by Sidney Drell at the 26th Annual Awards Dinner of the San Francisco Exploratorium held on April 30, 2003, the full text of which is available on the Exploratorium website. More detailed accounts of how the original Mark III linac evolved into Project M and then SLAC can be found in a May 1966 Technical Report "The Story of Stanford's Two-Mile-Long Linear Accelerator" by Douglas Dupen and in a 1983 contribution by Ginzton himself, both available on the SLAC website (www.slac.stanford.edu/history/.)

Preeminent over all of these, however, are Ginzton's own very personal reminiscences as recorded in his 1995 volume *Times to Remember: The Life of Edward L. Ginzton*, edited by his daughter Anne Ginzton Cottrell and Leonard Slater Cottrell and published by the Blackberry Creek Press in Berkeley, California.

SELECTED BIBLIOGRAPHY

1938

Application of feedback at radio frequencies, (engineer dissertation, Department of Electrical Engineering, Stanford University). A balanced feedback amplifier. *Proc. IRE* 26:1367-1379.

1939

Theory and applications of stabilized negative impedances. Ph.D. dissertation, Department of Electrical Engineering, Stanford University.

1945

Theory and application of stabilized negative impedance. Parts I, II, and III. *Electronics* (Jul., Aug., and Sept.):140-150, 138-148, 140-144.

1946

With A. E. Harrison. Reflex-klystron oscillators. *Proc. IRE* 34:97-113.

1948

With W. W. Hansen and W. R. Kennedy. A linear electron accelerator. *Rev. Sci. Instrum.* 19:89-108.

With W. R. Hewlett, J. H. Jasberg, and J. D. Noe. Distributed amplification. *Proc. IRE* 36:956-969.

1949.

With M. Chodorow and F. Kane. A microwave impedance bridge. *Proc. IRE* 37:634-639.

1950

With M. Chodorow, I. Neilsen, and S. Sonkin. Development of 10-cm high-power pulsed klystron. *Proc. IRE* 38:208.

1951

With M. Chodorow. Velocity modulated tubes. *Adv. Electron.* 3:43-83.

1953

With W. C. Barber and A. L. Eldredge. Possible medical and industrial application of linear electron accelerators. *Proc. IRE* 41:422.

With M. Chodorow, I. R. Neilsen, and S. Sonkin. Design and performance of a high-power pulsed klystron. *Proc. IRE* 41:1584-1602.

1954

The klystron. *Sci. Am.* 190:84-89.

1955

With M. Chodorow, W. W. Hansen, R. L. Kyhl, R. B. Neal, and W. K. H. Panofsky. Stanford high-energy linear electron accelerator (Mark-III). *Rev. Sci. Instrum.* 26:134-204.

1956

Microwaves—present and future. *IRE Trans. M. T. T.* MTT-4:136.

1957

Microwave Measurements. New York: McGraw-Hill. Translated into Russian in 1960 and Polish in 1961.

With K. B. Mallory and H. S. Kaplan, M.D. The Stanford medical linear accelerator: Design and development. *Stanford Med. Bull.* 15(Aug.).

1958

Microwaves. *Science* 127:841-851.

1959

With M. Chodorow, J. Jasberg, J. V. Lebacqz, and H. J. Shaw. Development of high-power pulsed klystrons for practical applications. *Proc. IRE* 47:20-29.

1961

With W. Kirk. The two-mile long electron accelerator. *Sci. Am.* 205:49-51.

1975

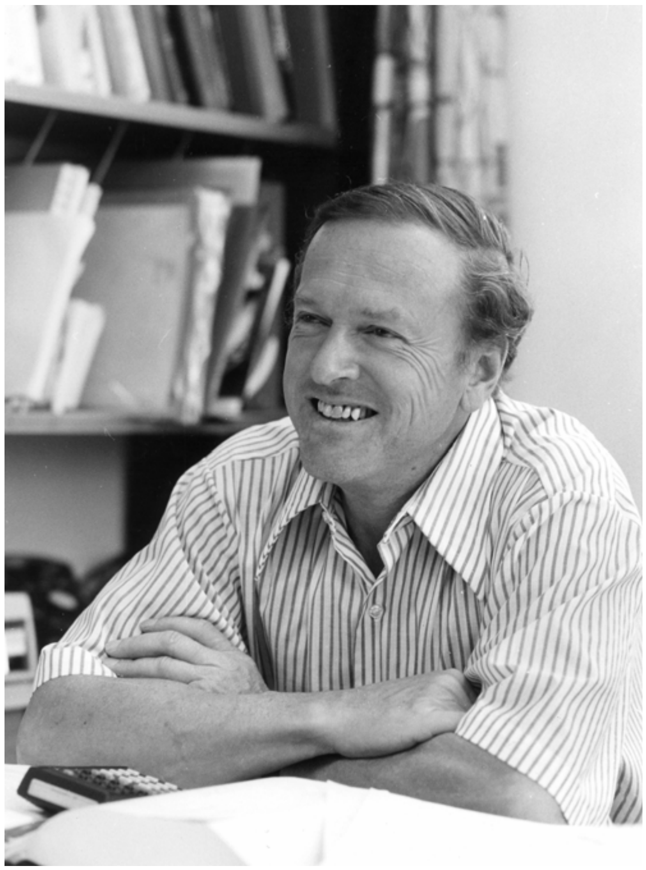
The $100 idea: How Russell and Sigurd Varian, with the help of William Hansen and a $100 appropriation, invented the klystron. *IEEE Spectrum* (Feb.). Reprinted in *IEEE Trans. Electron Dev.* ED-23:714-723 (1976).

1977

With H. Brooks. National Academy energy study. *Science* 196:372.

1995

A. G. Cottrell and L. S. Cottrell, eds. *Times to Remember: The Life of Edward L. Ginzton*. Berkeley, Calif.: Blackberry Creek Press.



THOMAS GOLD

May 22, 1920–June 22, 2004

BY GEOFFREY BURBIDGE AND MARGARET BURBIDGE

THOMAS GOLD WAS BORN in Vienna, Austria, on May 22, 1920, and he died in Ithaca, New York, on June 22, 2004. His father was a director of a large mining and metal fabrication company in Austria, and his mother had a theatrical background and had been a child actress.

In 1930 they moved to Berlin, where his father became director of another large company. By 1933 when Hitler came to power, it was clear that since his father was Jewish, they would have to leave, though they were not immediately persuaded since they were Austrians. But Tommy was sent to a boarding school in Zuoz in the Engadine in Switzerland, and by the late 1930s his parents had moved to England. At his school in Zuoz he soon found that he was high up in every class, and was very clever. He was also good at sports, and he became an excellent skier. In sports he was always aggressive. He had to win—he had to be the best. If he was not, for example at chess, he refused to play at all.

Tommy left Zuoz in 1937. In 1938, at the *Anschluss*, Germany invaded Austria and occupied it, and Gold and his family went to England with stateless papers. Gold entered Cambridge University and began to study in mechanical sciences. In September 1939 war was declared, and the following May the British government rounded up and interned many men who were technically enemy aliens. Gold was picked up in Cambridge and a few weeks later he was shipped with some 800 others to Canada. Within a year, however, he was shipped back to England and released. It was during this internment that he met his close friend Hermann Bondi.

Gold went back to Cambridge University in 1942, having lost about a year, and began to realize that he much preferred physics to mechanical sciences. Bondi had already graduated from Cambridge, taking part III in mathematics in April 1942, joined the naval research establishment, and gone over to the radar establishment at the Admiralty, where he worked in the theory group headed by Fred Hoyle. Gold graduated with a pass degree in June 1942. Because Gold had not done very well at Cambridge it took several months before Hoyle, assisted by Bondi, could arrange for Gold to join them. In that period Gold worked as an agricultural laborer in the north of England, cutting pit props for the mines. This was work he enjoyed; he became a lumberjack and was proud that he could cut down more trees with an axe that anyone else in the camp.

By the end of 1942 he had joined Bondi and Hoyle in the radar research group and in a few months was put in charge of new radar devices, designing and building new kinds of radar systems. It was in this period that the three of them began to talk about and work on problems in astronomy. Soon after the end of the war in 1945, Hoyle and Bondi returned to Cambridge, but Gold was not able to get back to Cambridge until 1947, when he started first to work on magnetron design.

He ultimately got involved with R. J. Pumphrey, a zoologist in the Zoology Department, whom he had originally met when Pumphrey was deputy head of the radar establishment. Thus he began to work in the zoology laboratory on the problem of hearing. This led to his first really original discovery. His studies of the cochlea showed that a passive cochlea where elements are brought into mechanical oscillation solely by the incident sound would not work. The degree of resonance of the elements of the cochlea can be measured, and the results are not compatible with the very heavy damping that must arise from the viscosity of the liquid. He proposed a regeneration hypothesis in which electromechanical action takes place whereby a supply of electrical energy is employed to counteract the damping. This feedback mechanism by the microphotonic potential forms an important link in the chemical events. This revolutionary idea was published by Gold (1948) and Pumphrey and Gold (1948), and Gold was awarded a prize fellowship at Trinity College Cambridge for his thesis on this topic. Unfortunately, the hearing specialists of that period with no physicists involved could not believe that the cochlea must incorporate such a mechanical feedback system. It took more that 40 years for this proposal to be understood and accepted. Gold's discouragement at this response led him to move into other fields, in particular, into astrophysics. On the basis of his radar work and a Trinity fellowship, Gold finally obtained a junior lectureship (a demonstratorship) in the Cavendish Laboratory.

STEADY STATE COSMOLOGY

Gold often discussed a wide range of problems in astrophysics with Hermann Bondi and Fred Hoyle. A major puzzle was Hubble's discovery of the redshift, the apparent magnitude relation that is interpreted as a clear indication that the universe is expanding. Gold pointed out in his

1978 interview with Spencer Weart[1] that Hoyle was talking endlessly about Hubble's result. As Gold put it,

> Everyone [else] had supposed that matter was created at one moment in the past because it was the obvious thing to say. There had been the Gamow discussion, but frankly it wasn't an awfully tight discussion. One didn't take it very seriously. But it was the obvious thing to say that if you see things flying apart you can work out when it [they] were together. People say it was all done at once. I don't see why you shouldn't think that it's done all the time and then none of the problems about fleeting moments arise. It can be just in a steady state with the expansion taking things apart as fast as new matter comes into being and condenses into new galaxies.

This was the basis of the idea that led Bondi and Gold to publish their paper on the steady state cosmology derived from what they called the "perfect cosmological principle," and Hoyle to publish separately on the steady state, basing his analysis on a field theoretical approach. Both papers appeared in 1948 and generated tremendous interest and a good deal of controversy.

For the next 10 to 15 years there was much debate about the steady state universe model. Most astronomers were opposed to it, some because of what they thought was good observational evidence against it, but nearly all of which turned out to be flawed. Others objected to it on religious grounds, though this was never admitted openly. It was Fred Hoyle and his student, and later collaborator, Jayant Narlikar, who devoted much of their energy making the case for the steady state model. They were strongly opposed by the Cavendish Laboratory's Martin Ryle and his radio astronomy group, who claimed that the distribution of distant radio sources that had been found showed that the universe is evolving and that the steady state theory could not be correct. Ryle began this crusade in 1955, and Hoyle and Narlikar tried not too successfully to deal with Ryle's observations; in the end it turned out that indeed

Ryle's initial data were flawed. From the beginning, the establishment and its followers believed Ryle. Ironically Ryle's whole argument was based on the belief that the radio sources were extragalactic, and at great distances—something that had only been established in 1952 but which Gold had suggested at a meeting in 1951. The circumstances were as follows.

For the first few years after the discovery of the radio sources, Ryle passionately believed that they were flare stars in our own Galaxy. At a meeting in London in 1951 it was Gold who first proposed that the sources might be very distant, and not very close by. When he said this (one of us, G.B., was present), he was harshly attacked by Ryle and several mathematical cosmologists who told him he didn't know what he was talking about. But by 1952 the first distant source had been identified, and at an International Astronomical Union meeting in Rome, Gold was able to get up and show that he was right, and that Ryle had been wrong.

By the early 1960s the apparent discovery[2] of the microwave background radiation, the black body radiation, led nearly everyone to believe that there must have been a beginning—a big bang. We suspect that Gold never believed this. Certainly Hoyle never did.

In 1952 Gold left Cambridge and became chief assistant to Astronomer Royal Sir Harold Spencer-Jones. He spent three productive years at the Royal Observatory. After Spencer-Jones retired and was replaced by Sir Richard Woolley, Gold resigned and left England in 1956. For the period 1957-1959 he was a professor of astronomy at Harvard University. In1959 he moved to Cornell University as chairman of the Department of Astronomy. He remained there as a professor and successively director of the Center for Radio Physics and Space Research (responsible for the Arecibo

Observatory in Puerto Rico) and assistant vice-president for research. He stayed at Cornell for the rest of his life.

In the period 1953-2004 he worked in many fields and demonstrated his amazing versatility in understanding and solving many geophysical and astrophysical problems. Some of his most notable achievements apart from those already mentioned are as follows.

While at the Royal Observatory, Gold became interested in the instability of the earth's axis of rotation (the wandering pole). He also wrote many papers on plasmas and magnetic fields in the solar system and the general problem of charged particles in the sun and the solar system. He invented the term "the earth's magnetosphere." With Fred Hoyle he developed a theory of solar flares.

In the 1950s and the 1960s he became active in many areas of space research and served on numerous U.S. national committees and as a consultant to the National Aeronautics and Space Administration. In the run-up to the manned space program and the lunar landing, there was much debate and confusion about the nature of the surface of the moon. Was it hard rock or was there a layer of fine dust? If the latter were true, the designers of the moon lander and the astronauts needed to know. By making use of the evidence from micro-impacts, craters, electrostatic fields, and other arguments, Gold predicted that the astronauts' boots would not sink in to more than about 3 centimeters. Within the range of possibilities this turned out to be close to the truth. But his popular approach to this problem had infuriated other experts; for example, he talked of "moon dust" instead of the "lunar regolite." Thus, he was unfairly attacked for being a centimeter or two wrong in his estimate.

Later on, Gold's relationship with NASA became very bad. This in large part was because Gold was outspokenly critical of NASA's programs. In the 1970s he was highly critical of the space shuttle program, arguing correctly that it would never achieve low-cost or 50 flights a year. He testified to congressional committees along these lines, although he was warned by NASA officials that if he did this, his own research proposals, which were well supported by NASA at the time, would be in peril. There is clear documentary evidence that the threat was carried out. In 1973 the deputy administrator for NASA wrote to James Fletcher, then the administrator, that he had explained to Gold that adverse comments about the space program had negated the possibility of Gold being funded for his current proposal or any other. He wrote, "Gold should realize that being funded by the Government and NASA is a privilege, and that it would make little sense for us to fund him as long as his views are what they are now." After this (in 1973), Gold received almost nothing from NASA, and had to give up research in planetary sciences.

PULSARS

In 1968 a number of pulsed radio sources with periods of 0.25 to 1.33 seconds were discovered by a graduate student in radio astronomy at Cambridge, Jocelyn Bell, and her supervisor, Anthony Hewish. The very short and precise periods meant that the sources must be very small and either pulsating or rotating. Gold immediately deduced that the only viable model was one in which the period is associated with the rate of rotation of a neutron star. This was a major conclusion reached by Gold, and it is now considered to be the correct explanation. Because of the strong magnetic field and the high rotation speed, relativistic velocities will be set up in any plasma in the surrounding magnetosphere, leading to radiation in the pattern of a rotating beacon. Gold's analysis was immediately accepted, and it has opened up many areas of research, such as those involving supernova studies, solid state physics (the composition of neutron stars), and galactic structure (using pulsars as probes of the galaxy).

GEOPHYSICS

In his last 20 years Gold returned to studies of the earth. He pointed out that some old deep gas bore holes, which theoretically should be exhausted, were still producing methane at a low but constant level. Isotope dating suggested that this was very old. He suggested that we might be seeing primeval methane trapped during the formation of the earth and continuously rising from the deep interior. If this were the case the amounts might be prodigious and of extreme importance. This gas might be trapped in fault structures and could both trigger and possibly enable us to predict the onset of earthquakes. Of course, such an hypothesis infuriated many petroleum geologists and others who believed in the conventional theory that the gas and oil have a very different origin. Small deep boreholes put down in the 1980s by the Swedish government to test Gold's hypothesis yielded only a small flow of methane, but it seemed to be ancient and to continue to flow.

Gold modified his original hypothesis to propose a "deep hot biosphere" of methane-producing organisms. The essential idea that he left with us is that hydrocarbons are not a by-product of prehistoric plant life but were present when the earth was created some 4.6×10^9 years ago. Geological pressure forced the hydrocarbons, accompanied by helium, up toward the surface from several hundred kilometers deep in the mantle. The hydrocarbons then formed the deposits of gas, oil, and coal. Micro-organisms that feed on hydrocarbons grew up in these deposits, and it is these that have given rise to life on Earth. Such processes could also have taken place on other planets.

PERSONAL

Gold was married twice. In 1947 he married Merle Tuberg, an American theoretical astrophysicist who had worked with S. Chandrasekhar in Chicago. They had three daughters, Linda, Lucy, and Tanya. Later, in 1972, he married Carvel Beyer, whom he had met at Cornell. They had one daughter, Lauren. He is survived by six grandchildren.

He received many honors, including the Gold Medal of the Royal Astronomical Society. He was elected to the Royal Society in 1964, the National Academy of Sciences in 1968, and the American Philosophical Society in 1972.

He was one of the outstanding physicists of his time. His versatility was unmatched. As Freeman Dyson has said, "His theories were always original, always important, usually controversial—and usually right."

NOTES

1. Interview. Thomas Gold by Spencer Weart, recorded April 1, 1978. American Institute of Physics Oral History Archives.
2. It is often claimed that the Penzias and Wilson discovery is proof of the big bang hypothesis. This is not correct. In fact, the radiation had been indirectly discovered by McKellar in Canada long before (in 1941) the development of the big bang theory by Gamow and others in the 1950s. The cosmologists were completely unaware of this.

SELECTED BIBLIOGRAPHY

1947

With R. J. Pumphrey. Transient reception and the degree of resonance of the human ear. *Nature* 160:124.

1948

Hearing. II. The physical basis of the action of the cochlea. *Proc. R. Soc. B* 135:492.

With R. J. Pumphrey. Phase memory of the ear: A proof of the resonance hypothesis. *Nature* 161:640.

With H. Bondi. The steady-state theory of the expanding universe. *Mon. Not. R. Astron. Soc.* 108:252.

1949

Rotation and terrestrial magnetism. *Nature* 163:513.

1951

The origin of cosmic radio noise. In *Proceedings of the Conference on Dynamics of Ionized Media.* London: University College Report.

1952

The alignment of galactic dust. *Mon. Not. R. Astron. Soc.* 112:215.

Polarization of starlight. *Nature* 169:322.

1954

Theories of interstellar polarization. *Mem. Soc. R. Sci. Liége* 15(4th ser.):591.

1955

With H. Bondi. On the damping of the free nutation of the earth. *Mon. Not. R. Astron. Soc.* 115:41.

Instability of the earth's axis of rotation. *Nature* 175:526,

The lunar surface. *Mon. Not. R. Astron. Soc.* 115:585.

1958

The arrow of time. In *Structure and Evolution of the Universe*, ed. R. Stoops. Brussels: Institut International de Physique Solvay. (Also in *Recent Developments in General Relativity*. London: Pergamon, 1962.)

1960

With F. Hoyle. On the origin of solar flares. *Mon. Not. R. Astron. Soc.* 120:89.

1961

The problem of the abundance of the hydrogen molecule. *Mem. Soc. R. Sci. Liége* 20:476.

1965

With S. Peale. The rotation of the planet Mercury. *Nature* 206:1240.

1968

Maser action in space. In *Proceedings of the Joint NRAO-Arecibo Symposium on HII Regions: Interstellar Ionized Hydrogen, Charlottesville, Va., Dec. 1967*, p. 747. New York: W. A. Benjamin.

Rotating neutron stars as the origin of the pulsating radio source. *Nature* 218:731.

1969

Rotating neutron stars and the nature of pulsars. *Nature* 221:25.

1970

Apollo 11 and 12 close-up photography. *Icarus* 12:360.

1972

The space shuttle program. Hearings before the Committee on Aeronautical and Space Sciences, U.S. Senate, part 2, p. 1112, April.

1977

With S. F. Dermott. The rings of Uranus. *Nature* 267(5612):590-593.

1979

Terrestrial sources of carbon and earthquake outgassing. *J. Petrol. Geol.* 1:3.

1980

With S. Soter. The deep earth gas hypothesis. *Sci. Am.* 242(6):155-161.

1982

With S. Soter. Abiogenic methane and the origin of petroleum. *Energ. Explor. Exploit.* 1(2):89-103.

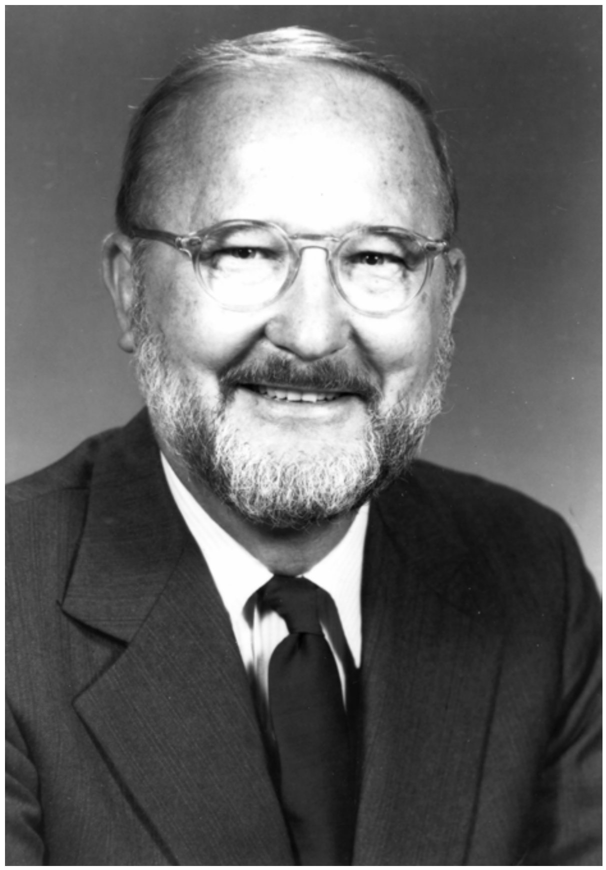
1985

The deep earth gas. In *1983 International Gas Research Conference Proceedings,* ed. L. Hirsch. Rockville, MD: Government Institutes.

The origin of natural gas and petroleum and the prognosis for future supplies. *Ann. Rev Energ.* 10:53-57.

1999

The Deep Hot Biosphere. New York: Springer-Verlag.



HERBERT SANDER GUTOWSKY

NOVEMBER 8, 1919–JANUARY 13, 2000

BY JIRI JONAS AND CHARLES P. SLICHTER

HERBERT SANDER GUTOWSKY and his students and collaborators made fundamental, pioneering discoveries in nuclear magnetic resonance (NMR). Their discoveries firmly established NMR as a major experimental tool in chemistry. Furthermore, Herb Gutowsky realized as early as 1950 that NMR would have a major impact in solving a wide range of structural and dynamics problems in chemistry, biochemistry, and material science. Indeed, many of the subfields of modern NMR spectroscopy can be traced to the original work by Gutowsky and coworkers.

Four areas of investigation of seminal importance for the development of NMR characterize Gutowsky's early work: (1) application of NMR to the study of the structure and motion in solids; (2) elucidation of the origin of chemical shifts; (3) observation of spin-spin couplings between nuclei in molecular liquids; and (4) use of NMR to study chemical exchange and molecular conformational changes.

In particular, the discovery of chemical shifts and spin-spin splitting, established the foundations of the vast field of high-resolution NMR spectroscopy, which developed into an indispensable tool for the determination of chemical, biochemical, and biological molecular structures. Early studies by Gutowsky showed that NMR spectra are modified when nuclei are involved in various types of chemical exchange. This provided chemists and biochemists with an important new technique to measure chemical exchange rates and to determine the nature of exchange processes.

PERSONAL HISTORY

Herbert Sander Gutowsky was born on November 8, 1919, in Bridgman, Michigan. He was one of seven children: six boys and one girl. They all lived on the family farm and helped with the farm work. Herb spent his summer "vacations" on their farm, working in the fields 10 hours a day, six to seven days a week. Evidently, farm life and work instilled a strong work ethic that influenced Herb's entire life. While on the farm, Herb attended a one-room elementary school. Later, after his mother died, during the tough years of the Great Depression, the family sold the farm and moved to Hammond, Indiana, where Herb attended Hammond High School.

As a 16-year-old, Herb got a job distributing newspapers to help support the family. In spite of the economic hardships Herb wanted to get a university education. He borrowed money from one of his older brothers and attended Indiana University, where he graduated Phi Beta Kappa with a degree in chemistry with concentration in mathematics, physics, and astronomy. In fact, at one point Herb considered becoming an astronomer. He joined the Army Reserve Officers Training Corps at Indiana University and particularly enjoyed being a member of the precision drill team.

After receiving his A.B. in chemistry, Herb entered graduate school at the University of California, Berkeley, where he spent one year before volunteering for active duty in the military. He joined the Army several months before Pearl Harbor and became an officer for materials procurement in the Chemical Warfare Service. At the end of his service Herb was discharged with the rank of captain and returned to Berkeley. While in the Army, Herb was diagnosed with diabetes, a condition that did not impede his scientific career but eventually led to his death. Herb received his M.S. degree working with Ken Pitzer and entered Harvard in the fall of 1945 to start Ph.D. graduate work with George Kistiakowsky.

In 1948, immediately after completion of his Ph.D., Herb joined the Department of Chemistry and Chemical Engineering at the University of Illinois at Urbana-Champaign planning to dedicate his scientific career to applications of nuclear magnetic resonance to chemistry. After his initial appointment as an instructor in 1948, he was rapidly promoted to assistant professor in 1951, associate professor in 1955, and professor of physical chemistry in 1956. In addition to his outstanding research work, Herb Gutowsky was involved in administration; he was head of the Physical Chemistry Division from 1956 to 1962, head of the Department of Chemistry and Chemical Engineering from 1967 to 1970, and director of the School of Chemical Sciences from 1970 to 1983. In 1983 he returned full-time to research and teaching as a research professor of chemistry and professor at the Center for Advanced Study, and remained active until his death in 2000.

Before the overview of Gutowsky's major scientific accomplishments, a few remarks about his personal life and hobbies are appropriate. Herb's intense work habits left some time for his hobbies of bird watching and cycling. For many years Herb and his sons participated in "century rides," bicycle rides of 100 miles in a single day. Herb was also a devoted fan of the Fighting Illini and a longtime holder of season tickets for basketball and football games at the University of Illinois.

In 1949 Herb Gutowsky married Barbara Stuart, with whom he had three sons, one of them now deceased. This marriage ended in divorce in 1981. Herb married Virginia Warner in 1982. Herb died in his Urbana home in January 2000 at the age of 80. He is survived by his first wife, Barbara Stuart Gutowsky, and their two sons, Robb E. Gutowsky and Christopher C. Gutowsky; his second wife, Virginia Warner Gutowsky; three grandchildren; and sister Esther R. Enyart.

RESEARCH HISTORY

What were the circumstances that introduced Gutowsky to NMR and determined his career path? He entered Harvard in the fall of 1945 and received his Ph.D. working with George Kistiakowsky. Most of Gutowsky's Ph.D. thesis was on infrared investigations. However, as a result of a lunch discussion Kistiakowsky had with the physicist Edward Purcell, whose student George Pake had just discovered structure on the proton nuclear magnetic resonance spectrum of water hydration in gypsum, Gutowsky and Kistiakowsky decided to add a section to Gutowsky's thesis employing NMR to determine the structure of the molecule diborane. Together with George Pake, who had finished his thesis research but not yet taken the final exam, Herb tackled this problem. The initial results proved inconclusive, but they motivated Gutowsky and Pake to work intensively during the winter and spring of 1948, using Pake's apparatus in the basement of the Lyman physics lab at Harvard. Their research produced a famous set of papers showing how nuclear magnetic resonance made possible the study of the structure and motion of molecules in solids. They studied the proton-proton distances of hydrogens bonded to carbons or nitrogens in a number of rigid lattices, inferring the bond angles in these molecular crystals. Gutowsky and

Pake demonstrated that nuclear magnetic resonance could reveal the existence of hindered rotation in solids of groups of atoms containing hydrogen. Then, as a brand new Ph.D. with virtually no knowledge of electronics, Gutowsky went to the University of Illinois as an instructor and decided to commit his scientific career to the applications of magnetic resonance to chemistry. The early discoveries by Gutowsky and his students led to the enthusiastic acceptance of NMR by chemists and to the later phenomenal growth of the NMR field.

The following section is an excerpt from an address given by Gutowsky[1] after the presentation in Bombay of the 1974 award of the International Society of Magnetic Resonance; it describes in his own words the excitement of his early work and discoveries.

The magnet was delivered in early October 1949 and on October 20 we saw our first proton resonance in a water sample. However, my original plans to continue to study the motional narrowing of the broad resonances in solids ran into two snags. Our electronics, especially the lock-in amplifier and preamplifier just after the RF bridge we were using, did not have the sensitivity needed. Moreover, my initial efforts at designing and building an all-metal cryostat were not very successful in spite of having a 2-inch gap in which to place it. Fortunately, Knight's report of resonance shifts in metals appeared at that time, and it included the observation of ^{31}P shifts in several compounds. Also, in early 1950 Proctor and Yu reported chemical shifts between the ^{14}N resonances in the NH_4^+, CN^-, and NO_3^- ions, and Dickinson found shifts for fluorine in several compounds. So we turned our interest to chemical shifts in liquids, which we could readily observe for ^{19}F.

At first we looked at whatever compounds we could lay our hands on. Then we began to see if we could resolve the fluorine resonances from structurally nonequivalent nuclei in the same molecule and on April 26, 1950, we looked at the fluorine resonance in a benzene derivative with a CF_3 group and three fluorines on the ring and we were able to resolve them. This encouraged us greatly and led me to think about how the shifts might be related to molecular structure. Chemists learn very early to look for periodicities in the chemical and physical properties of compounds, or they don't stay in chemistry very long. We deal with such a large number and wide variety of systems that we have to oversimplify their diversity to be able to remember them. Moreover, in my senior year at Indiana University I was exposed to Linus Pauling's book "The Nature of the Chemical Bond," which is a masterpiece of such oversimplifications. In any case, it seemed to me that the chemical shift, as an electronic phenomenon should be related in some way to the nature of the chemical bonds. This in turn depends upon the nature of the atoms bonded together, so I chose to study the simple binary fluorides, which was a very happy choice.

I was encouraged in this approach because at about that time, I acquired my first graduate student, Charles Hoffman, who had a strong interest in inorganic chemistry. He undertook to synthesize the fluorides needed for the study, many of which were very difficult, and measured their fluorine shifts. At this juncture, in May of 1950, good fortune favored me again, I found exceptionally able help with our electronics problems from R. E. McClure, a senior in Electrical Engineering, who worked part-time with my group while he finished his bachelor's and master's degrees.

Bob was an immediate help, not only in maintaining, designing and constructing our apparatus, but in running many of the early experiments. With his able help, we set out to see if we could find shifts in proton resonances. We convinced ourselves of them in June 30, 1950, when Bob found small but reproducible shifts, several time larger than experimental error, between the protons in benzene, mineral oil, and aqueous solutions of strong acids. At this stage, we started a major effort on improving the homogeneity of the magnetic field by plotting it and hand polishing the pole tips in those areas where the magnetic field was high. And later, we broadened the shift study to include the simple hydrides. The shifts for the fluorides showed a strong correlation with electronegativity of the atom bonded to fluorine, which subsequently led Appolo Saika and Charlie Slichter to their elegant paper attributing it to the second-order paramagnetic term of Ramsay's theory and its dependence upon the p-electron bonding orbital of fluorine.

The discovery and characterization of the multiplets, or the indirect, scalar internuclear coupling as the phenomenon is often called, is a very interesting and often exciting story. In my lab, the first observation of the effect was by Charlie Hoffman on September 8, 1950. He observed a double line in the fluorine resonance of the small sample of PF_3, which he had synthesized as part of his fluorine shift study. However, in making the sample he fluorinated PCl_3, so we interpreted the double line as due to $PClF_2$ and/or PCl_2F impurities in his sample. It's an excellent example of something new not being recognized as real and different, but being attributed to a conventional, more plausible cause. Nonetheless, Hoffman repurified the sample, and the second line would not go away. Another sample was synthesized by a different route, and the same double resonance obtained. PF_5 was synthesized and the fluorine resonance in it was found to be a double line, this time attributed to a chemical shift between the three equatorial and two apex fluorines in the trigonal pyramid structure. It was not until March 1951 that it became completely clear that we were really seeing something entirely new. At that time, I encouraged a beginning graduate student, Dave McCall, to look at phosphorus shifts in several fluoro-chloro phosphorus compounds of known purity. When the ^{31}P resonance in the $POCl_2F$ compound exhibited a 1-1 doublet structure, it finally dawned on me that we had been observing a new type of internuclear interaction related to the spin states of the two nuclei. It was a great thrill to Dave and me when shortly thereafter, we first saw a ^{31}P resonance such as that for CH_3OPF_2, a 1-2-1 triplet as I had predicted.

In our subsequent characterization during 1951 of this scalar or indirect coupling of nuclear spins, we were able to show that the "slow beats" observed by Erwin Hahn in proton spin echoes originate from the multiplets we had found in steady-state fluorine spectra. Furthermore, we turned up some anomalies which clued us in on the importance of chemical exchange in determining the observability of such splittings. The best case was that of aqueous HBF_4, in which the proton and fluorine resonances were instrumentally determined, at about 0.01 gauss, while that of ^{11}B may have been somewhat larger. ^{11}B has a large g-value, so we expected to resolve a splitting of the fluorine resonance as we found in aqueous HPF_6. After a great deal of debate, we attributed the absence to chemical exchange of fluorines among the BF_4^- ions, which led C. P. Slichter to develop a mathematical treatment of the effect based upon Bloch equations.

There is an interesting quote from David McCall (personal communication, May, 2001) about his experiences as a graduate student at Illinois:

> Illinois was a great place to be in the early '50s and Gutowsky's group got to participate in very exiting projects. Herb was with us round the clock and always supportive. He let us think that we had some of the best ideas, but on reflection we knew where they came from. The interaction with physics was great. We profited enormously from the enthusiasm and approachability of Charlie Slichter and his students. We also enjoyed occasional meetings with George Pake's group at Washington University, just down the road in St. Louis.

In view of the extensive scope of Gutowsky's subsequent work (total of 298 publications), we have been quite selective in our discussion of his accomplishments, and have decided to mention only several key studies.

In their paper on multiplets Gutowsky, McCall, and Slichter worked out the mathematical theory of a resonance multiplet arising from a nucleus that could precess at either of two rates and whose precession frequency could jump from one to the other as a result, or example, of chemical exchange or some relaxation process. They showed how the multiplet collapsed as the exchange or relaxation became more rapid. Their formulation based on Bloch equations, described the case of spin-spin splittings in liquids and other situations in which the two lines were themselves narrow, a case that had not been worked out by Bloembergen, Purcell, and Pound.

Gutowsky and Saika recognized that these rate equations could be used as the basis for the investigation of chemical exchange. In their classic paper Gutowsky and Saika investigated the association and exchange of protons in aqueous solutions of electrolytes. They extended the theory working out the process with exchange among more than two sites and calculated and displayed in figures the details of the multiplet structure collapse, as the rate of exchange increased. While they predicted the collapse of the multiplet structure, none of the cases they dealt with had an exchange rate that passed through the region in which the collapse occurs.

Gutowsky and Holm, simultaneously with Arnold, succeeded in following the details of multiplet collapse. Gutowsky and Holm studied several amides and determined the barriers to internal rotation among several conformations. This technique enabled them to observe the existence of energy barriers even in the liquid state and opened a whole new area of chemical study. In the Science Citation Index 13 years later this paper was still being cited over 50 times in a year. Gutowsky's group continued to work in the area of chemical exchange, utilizing the spin- echo technique to extend greatly the range of rates that could be studied by NMR.

Herb Gutowsky also realized very early that NMR would have many applications in studies of complex biochemical and biological systems. His collaborative work with Eric Oldfield in the area of protein-lipid interactions in membranes and with Govindjee on photosynthetic systems represents pioneering applications of NMR in these fields.

In 1983 Herb left administration to devote full-time to research and teaching. At this time there was a major change in his research interests. He began the study of weakly bound (van der Waals) clusters of atoms and molecules, using high-resolution Fourier transform microwave spectrometry. Gutowsky and his group continued the development of the Flygare-Balle pulsed-beam Fourier transform spectrometer by significantly improving the sensitivity and capability of the spectrometer. This allowed Gutowsky and his collaborators to extend the study from dimers to more complex systems, such as trimers, tetramers, small clusters of atoms, and polyatomic molecules. It is impressive that within a few years Gutowsky and his group were able to make major contributions to our understanding of weakly bonded clusters. Even in this field Gutowsky's research was prolific and resulted in about 50 papers devoted to a wide range of problems on weakly bonded clusters. Most of the theoretical interpretations were performed in collaboration with Clifford Dykstra of Indiana University-Purdue University, Indianapolis.

Herb Gutowsky received many honors and awards for his seminal work in nuclear magnetic resonance. He was elected to the National Academy of Sciences in 1960, American Academy of Arts and Sciences in 1969, and the American Philosophical Society in 1982. Herb also held various offices in the American Chemical Society, American Physical Society, National Academy of Sciences, American Academy of Arts and Sciences, and the National Science Foundation.

WE WOULD LIKE TO acknowledge Robb E. Gutowsky for his comments on the personal life of Herb Gutowsky and Virginia Warner Gutowsky for the photograph. We also thank Clifford E. Dykstra for his helpful comments.

PRINCIPAL AWARDS AND HONORS

1954 Guggenheim fellow
1960 Member of the National Academy of Science
1964 Krug Lecturer, University of Illinois
1966 Fellow of the American Academy of Arts and Sciences
Irving Langmuir Award in Chemical Physics, American Chemical Society
1974 Award of the International Society of Magnetic Resonance
1975 Peter Debye Award in Physical Chemistry, American Chemical Society
1976 G. N. Lewis memorial lecturer, University of California, Berkeley

1977 National Medal of Science
1978 Honorary member, Phi Lambda Upsilon
1979 Honorary member, Society for Applied Spectroscopy
1980 G. B. Kistiakowsky Lectureship, Harvard University
1981 D.Sc. Indiana University
1982 Member of the American Philosophical Society
1984 Wolff Prize in Chemistry
1991 Chemical Pioneer Award, American Institute of Chemists
1992 Pittsburgh Spectroscopy Award, Spectroscopy Society of Pittsburgh

NOTES

1. Speech at the Fifth International Symposium on Magnetic Resonance, Bombay, India, January 1974, quoted in J. Jonas and H. S. Gutowsky. NMR in chemistry--an Evergreen. *Ann. Rev. Phys. Chem.* 31(1980):1-27.

SELECTED BIBLIOGRAPHY

1949

With G. B. Kistiakowsky, G. E. Pake, and E. M. Purcell. Structural investigations by means of nuclear magnetism. I. Rigid crystal lattices. *J. Chem. Phys.* 17:972-981.

1950

With G. E. Pake. Structural investigations by means of nuclear magnetism. II. Hindered rotation in solids. *J. Chem. Phys.* 18:162-170.

1951

With C. J. Hoffman. Nuclear magnetic shielding in fluorine and hydrogen compounds. *J. Chem. Phys.* 19:1259-1267.

1953

With D. W. McCall and C. P. Slichter. Nuclear magnetic resonance multiplets in liquids. *J. Chem. Phys.* 21:279-292.

With A. Saika. The proton magnetic resonance in aqueous electrolytes. *J. Chem. Phys.* 21:1688-1694.

1954

With D. W. McCall. Electron distribution in molecules. IV. Phosphorus magnetic resonance shifts. *J. Chem. Phys.* 22:162-164.

1956

With C. H. Holm. Rate processes and nuclear magnetic resonance spectra. II. Hindered internal rotation of amides. *J. Chem. Phys.* 25:1228-1234.

1957

With C. H. Holm, A. Saika, and G. A. Williams. Electron coupling of nuclear spins. I. Proton and fluorine magnetic resonance spectra of some substituted benzenes. *J. Am. Chem. Soc.* 79:4596-4605.

1959

With M. Karplus and D. M. Grant. Angular dependence of electron-coupled interactions in CH2 Groups. *J. Chem. Phys.* 31:1278-1289.

1961

With I. J. Lawrenson and K. Shimomura. Nuclear magnetic spin-lattice relaxation by spin-rotational interactions. *Phys. Rev. Lett.* 6:349-351.

1962

With G. G. Belford and P. E. McMahon. NMR studies of conformational equilibria in substituted ethanes. *J. Chem. Phys.* 36:3353-3368.

1963

With R. J. C. Brown and K. Shimomura. Nuclear spin relaxation in liquid $CHFCl_2$. *J. Chem. Phys.* 38:76-86.

1964

With A. Allerhand. Spin-echo NMR studies of chemical exchange. I. Some general aspects. *J. Chem. Phys.* 41:2115-2126.

1966

With A. Allerhand, J. Jonas, and R. A. Meinzer. Nuclear magnetic resonance methods for determining chemical-exchange rates. *J. Am. Chem. Soc.* 88:3185-3194.

1969

With C. J. Jameson. Systematic trends in the coupling constants of directly bonded nuclei. *J. Chem. Phys.* 51:2790-2803.

1976

With T. Wydrzynski, N. Zumbulaidis, P. G. Schmidt, and Govindjee. Proton relaxation and charge accumulation during oxygen evolution in photosynthesis. *Proc. Natl. Acad. Sci. U. S. A.* 73:1196-1198.

1978

With E. Oldfield, R. Gilmore, M. Glaser, J. C. Hsung, S. Y. Kang, T. E. King, M. Meadows, and D. Rice. Deuterium nuclear magnetic resonance investigation of the effects of proteins and polypeptides on hydrocarbon chain order in model membrane systems. *Proc. Natl. Acad. Sci. U. S. A.* 75:4657-4660.

1979

With S. Y. Kang, J. C. Hsung, R. Jacobs, T. E. King, D. Rice, and E. Oldfield. NMR investigation of the cytochrome oxidase-phospholipid interaction: A new model for boundary lipid. *Biochemistry* 18:3257-3267.

1985

With C. Chuang, J. D. Keen, T. D. Klots, and T. Emilsson. Microwave rotational spectra, hyperfine interactions, and structure of hydrogen fluoride dimers. *J. Chem. Phys.* 83:2070-2077.

1987

With T. D. Klots, C. Chuang, J. D. Keen, C. A. Schmuttemaer, and T. Emilsson. Rotational spectra and structures of small clusters Ar_3-HF and Ar_3-DF. *J. Am. Chem. Soc.* 109:5633-5638.

With C. J. Coleman and Govindjee. ^{35}Cl-NMR measurement of chloride binding to the oxygen-evolving complex of spinach photosystem. II. *Biochim. Biophys. Acta* 894:443-452.

1990

With T. D. Klots and C. E. Dykstra. Rotational spectrum and potential surface for Ar_2-HCN: A T-shaped cluster with internal rotation. *J. Chem. Phys.* 93:6216-6224.

1992

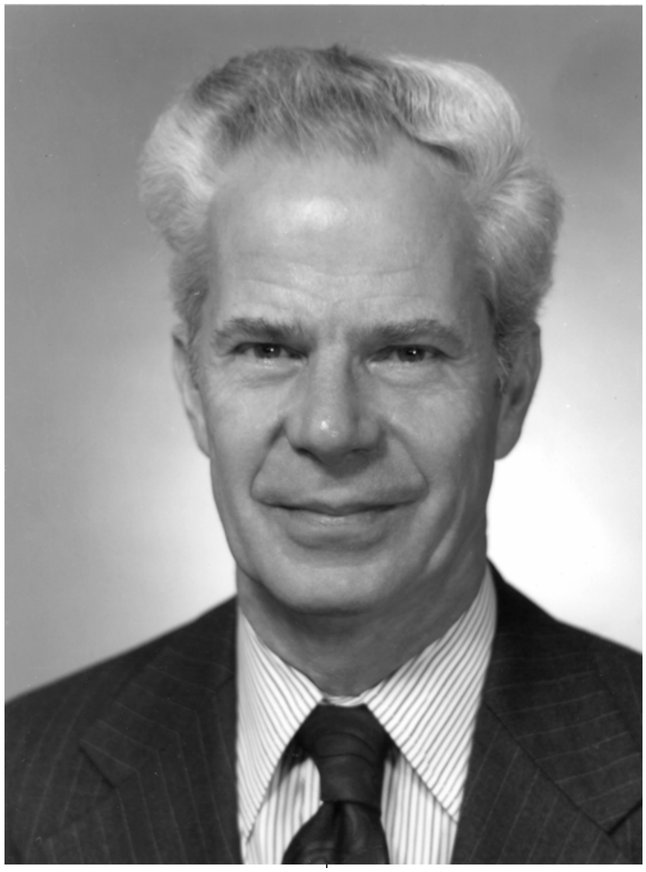
With T. D. Klots and T. Emilsson. Rotational spectra, structure, Kr-83 nuclear quadrupole coupling constant, and the dipole moment of the Kr-benzene dimer. *J. Chem. Phys.* 97:5335-5340.

1993

With J. D. Keen, T. C. Germann, T. Emilsson, J. D. Augspurger, and C. E. Dykstra. Rotational spectrum and structure of the Ne-HCN dimer. *J. Chem. Phys.* 98:6801-6809.

1996

With E. Arunan, T. Emilsson, S. L. Tschopp, and C. E. Dykstra. Rotational spectra, structures, and dynamics of small Ar_m-$(H_2O)_n$ clusters: The Ar_2-H_2O trimer. *J. Chem. Phys.*105:8495-8501.



VLADIMIR HAENSEL

September 1, 1914–December 15, 2002

BY STANLEY GEMBICKI

VLADIMIR ("VAL") HAENSEL WAS born to Nina Von Tugenhold and Paul Haensel (Pavel Petrovich Genzel) on September 1, 1914 (one month after the outbreak of World War I) in Freiburg, Germany. He spent much of his youth in Moscow, where his father was a respected professor of economics from 1903 to 1928. Shocked by the outbreak of the Bolshevik Revolution, his family fled Moscow, but was captured and returned to meet an uncertain fate. Officially rehabilitated, his father resumed his professorship and was made director of the financial section of the Institute of Economic Research in Moscow from 1921 to 1928; there he authored Lenin's first five-year plan. After escaping the U.S.S.R. in 1928, the Haensel family lived briefly in Germany, France, and Austria. They came to the United States in 1930 when Haensel's father accepted a teaching position at Northwestern University.

Haensel entered Northwestern University in 1931 and received a bachelor of science degree in general engineering in 1935. He received his master's degree in chemical engineering from the Massachusetts Institute of Technology in 1937.

Haensel joined the Universal Oil Products Company in 1937 as a research chemist. From 1939 to 1946, while still working for the company, he was assigned to the Ipatieff High Pressure Laboratory at Northwestern University as an assistant to the famous catalysis researcher Prof. V. N. Ipatieff (NAS, 1939), who was affiliated with both Northwestern University and UOP. (Some years earlier Ipatieff had defected from Russia, where he had been a general in the Imperial Army and a professor of chemistry at St. Petersburg University. Haensel told how Ipatieff's catalysis research grew out of his having been an artillery officer in the Imperial Army, intrigued with the high-pressure, high-temperature combustion chemistry occurring in cannons.) While at the laboratory, Haensel continued his education and earned his Ph.D. in chemistry from Northwestern University in 1941. In the same year Haensel was assigned as the coordinator of the "cracking" research division of UOP. During this time, Haensel's work focused on the use of catalysts other than platinum in the general reactions. Among his early successes was the development of a catalytic method of selective demethylation to make triptane, the hydrocarbon with the greatest antiknock properties of any compound. In 1951 he was appointed the director of refining research and in 1960 became the director of process research. In 1969 Haensel became vice-president and director of research. In 1972 he was appointed vice-president for science and technology, a position he held until 1979.

In the early 1950s it was established that the deadly photochemical smog frequently experienced in locales such as the Los Angeles basin was produced when nitrogen oxides and unburned or partially burned fuel hydrocarbons in auto exhaust reacted in bright sunlight. Haensel, at United Oil Products from 1956 to 1974, played a key role in establishing research and development programs that eventually culminated in the automotive catalytic converters that were first used on almost all U.S. autos in the 1975 model year, and today are virtually ubiquitous in most of the developed and developing nations on the five nonpolar continents.

After 1979 Haensel was a consultant at UOP and a professor of chemical engineering at the University of Massachusetts, Amherst. He was a member of the National Academy of Sciences and the National Academy of Engineering and was recognized with awards that included the National Medal of Science, the Perkin Medal, the first National Academy of Sciences Award for Chemistry in Service to Society, the Professional Progress Award (American Institute of Chemical Engineers), the Draper Prize, and the Chancellor's Outstanding Teacher Award (University of Massachusetts).

Haensel was a multifaceted person, deeply interested in the world around him and the process by which it advanced. He was a patron of the arts, enjoying plays and music, particularly when in the company of friends. His interest even extended to the writing of short stories, usually illustrating a lesson he had derived from his experience and study of the reactions of people to advancing knowledge. Perhaps best known of these is his whimsical *Lucky Alva* short story in which he brought forth his view of important lessons from the life of America's most famous inventor and entrepreneur (1967).

During the later years of his career in industry and his tenure at the University of Massachusetts, he felt an obligation to use his life experience to foster and mold the career of young scientists. It was a joy for him to see young scientists develop into accomplished researchers who would make a real difference in the world. In his obituary he was quoted from a 1995 interview: "Work to produce something important. Do something new. Do something interesting, something that makes you want to shout out loud when you've got it. Life is too darn amazing—and too short—for anything less."

Of him it can be said: He lived life to the fullest and left his mark as a researcher and as a person. We are all the better for having known him and for having benefited from his work.

THE DEVELOPMENT OF THE PLATFORMING™ PROCESS

Haensel's most important invention is without doubt the Platforming process. By 1940 the octane number of gasoline could be improved by the Houdry process, using clay catalysts, or by adding octane boosters. One octane booster was iso-octane, prepared by the Pines-Ipatieff process using strong liquid acids as the catalyst to alkylate olefins with branched paraffins. Another octane booster was tetra-ethyl lead, which was used heavily until the 1970s, when it was phased out of gasoline because of the toxicity of the lead compounds in auto exhaust. It was not generally understood that the clay catalysts used by Eugène Houdry were in reality solid acids and that the chemistry of catalytic isomerization and catalytic cracking of straight hydrocarbons was thus akin to that of the liquid-acid catalyzed alkylation invented by Ipatieff and Pines.

In 1947 Haensel began exploring the possibility of using platinum catalysts for upgrading petroleum. When he started working on what came to be called the Platforming process, there was no economically practical way to upgrade gasoline to high-octane numbers by reforming processes. Thousands of catalysts had been tried, but none was able to generate the necessary results. The existing 65-octane fuel was inefficient and caused knocking in the compression cycle of engines. Consequently, it prevented the development of high-compression engines and their promise of higher efficiency. In looking for a better way Haensel took a different approach and proposed something that at the time was considered both impractical and uneconomical.

He suggested the use of the precious metal platinum in the refining process, supported on alumina as a bifunctional catalyst.

The marvelous catalytic properties of platinum had been described by J. W. Doebereiner and Humphry Davy around the year 1800. Doebereiner had even experimented with platinum supported on clay. Haensel thought that the miracle catalyst platinum might also be good for upgrading gasoline.

To most of his contemporaries this sounded crazy. Gasoline costs were between 8¢ and 10¢ per gallon, and platinum had never been considered a viable catalyst because it was too expensive—more expensive than gold—and could only be obtained in significant quantities in Russia and South Africa. However, Haensel understood that a catalyst that had a long life and could be regenerated and reused in situ would, in fact, be more economically efficient in the long run than a "cheap" catalyst with a short life. After his initial tests confirmed the high stability and good activity of platinum on alumina, he tried to minimize the amount of platinum. He knew that only surface atoms are used in heterogeneous catalysis, so he directed his efforts to prepare extremely high dispersed supported platinum. In1947 he showed that a catalyst with 0.01 percent platinum on alumina was both active and stable. Clearly, the platinum particles of this catalyst must be extremely small. Hydrogen adsorption indicates that more than 50 percent of the platinum atoms are surface atoms.

Even more important was his proposal that platinum on alumina was a dual-functional catalyst, ideally suited to the catalytic reforming chemistry. Platinum is an excellent hydrogenation and dehydrogenation catalyst, but acid-base chemistry is required to go from saturated alkane chains to aromatic rings. That was evident from the work of Houdry, Ipatieff, and Pines. Haensel's insight was that alumina, a Lewis acid, not only could physically support dispersed platinum but the unsaturated hydrocarbons formed by the platinum could also be isomerized to rings on the acidic alumina. He established this key synergism by testing both the supported catalyst and physical mixtures of platinum and alumina, where the contact with intermediates was not intimate.

Another major advantage of Haensel's process was that it generated large amounts of hydrogen. In addition to the economic value of the hydrogen, its production helped to remove much of the sulfur and other contaminants found in petroleum. Hydrogen generation, therefore, is an important step in making the Platforming process a much more environmentally friendly process than any previous refining technique. Gasoline produced by the Platforming process also has a higher octane value than gasoline produced using older methods. Higher-octane fuels burn much more cleanly and efficiently, reduce knocking, and improve mileage and engine performance.

In addition to cleaner, cheaper fuel this process generated a higher yield of aromatic hydrocarbons—the raw materials used in the manufacture of plastics. This created the base for the modern plastics industry, which previously relied on the processing of coal tar, a very environmentally unfriendly process. Through catalytic reforming chemistry, more than 200 billion pounds of aromatic hydrocarbons are produced each year.

It could be concluded that Haensel was the inventor of catalysis with supported nanoparticles of platinum, although that word was coined much later. It is now one of the buzzwords in material science; few people realize that in heterogeneous catalysis, nanoparticles have routinely been used for six decades thanks to the work of Vladimir Haensel.

THE ACHIEVEMENT'S WORLDWIDE IMPACTS

Each of us benefits daily from the fruits of Vladimir Haensel's work. The engineering breakthrough of the Platforming process has helped shape our economy in many ways, from the inexpensive processing of high-grade fuels to the production of plastics in a more environmentally sound way. These advancements have directly and indirectly contributed to many of the world's industries. We can easily take for granted the abundance of low-cost, high-efficiency fuels without realizing that the ability to economically transport food, medicine, industrial supplies, and even our mail is very much dependent on Haensel's invention.

Indeed, the Platforming process has reduced the United States' reliance on foreign oil, has broadened the long-term energy outlook for the world, and has saved billions of dollars in transportation costs. At the time of this writing the United States had over 190 million cars, trucks, and buses that consumed nearly 132.9 billion gallons of gasoline each year. They serve the bulk of the nation's transportation needs, bring families together, and deliver food and medicine. A gallon of reformed high-octane gasoline produced through the Platforming process can provide 35 percent more mileage than previous methods as well as much higher performance. The savings in natural resources and costs to the consumer are tremendous.

As I write this memoir, the United States spent, on average, $123 billion per year to buy gasoline. The estimated cost of operating an automobile in the United States was about 46¢ per mile; of that, the cost of gasoline and oil was only 6¢. World consumption of oil was 66 million barrels per day, of which the United States accounts for roughly one-fourth.

VLADIMIR HAENSEL RETURNS TO TEACHING

After serving as vice-president for science and technology at UOP, Haensel joined the faculty at the University of Massachusetts, Amherst, in 1980 as professor of chemical engineering. He continued to teach at the university and also served as a consultant to UOP up until his passing.

Known across campus as "Val," he was an influential figure at UMass Amherst, both as a teacher and as an adviser to students, faculty, deans, and chancellors. He took particular pride in two elective courses he taught to mixtures of undergraduate and graduate students: Catalysis and Energy Conversion Processes, and Industrial Chemistry. His style was Socratic, often aided and abetted by his wife, Hertha Skala Haensel, former director of physical chemistry and surface science at UOP. Following preparative study, the students launched into spirited discussion, punctuated by anecdotes, stories, and occasional apples from the teacher in recognition of new insights.

He also cherished the chance to work with undergraduate and graduate students in the lab, exploring new science with them and sharing his experience and research philosophy. The company contributed directly to this activity by creating a Vladimir Haensel/UOP research scholarship fund, which sponsors research by undergraduates.

Haensel has served as a board member of the Petroleum Research Fund, 1979-1982; chairman of the U.S.-U.S.S.R. Technology Exchange in Chemical Catalysis, 1976-1979; U.S. State Department Representative to the International Scientific Forum in Hamburg, Germany, 1980; chairman of the advisory committee, Industrial Science and Technology Innovation of the National Science Foundation, 1982-1985; and a member of the Board of Directors of Heico Corporation.

Haensel authored more than 120 scientific and technical papers, and was granted over 145 U.S. patents and 450 foreign patents. He was elected to the National Academy of Sciences in 1971 and the National Academy of Engineering in 1974. Among his many awards and honors was the National Medal of Science from President Nixon on October 10, 1973. He was also the first recipient of the National Academy of Sciences' Award for Chemistry in Service to Society in 1991. In 1994 Haensel was awarded the Chancellor's Outstanding Teacher Award from the University of Massachusetts, Amherst. In 1997 Haensel was selected by the National Academy of Engineering to receive the Charles Stark Draper Prize.

Haensel is survived by his wife, Hertha Skala Haensel, who lives in Amherst. His daughter, Kathee Webster, lives in Virginia Beach, Virginia. Before his passing, Haensel was investigating the use of hydrogen as a fuel.

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SELECTED BIBLIOGRAPHY

1943

With V. Ipatieff. The triptane process. *Science* 98:495.

1946

With V. Ipatieff. Selective demethylation of paraffin hydrocarbons. *J. Am. Chem. Soc.* 68:345.

1947

With V. Ipatieff. Selective demethylation of paraffin hydrocarbons, preparation of triptane and neopentane. *Ind. Eng. Chem.* 39:853.

1949

Alumina-platinum-halogen catalyst and preparation thereof. August 16. U.S. Patent No. 2,479, 109.

Process of reforming a gasoline with an alumina-platinum-halogen catalyst. August 16. U.S. Patent No. 2,479,110.

1951

With C. V. Berger. Production of aromatics by the Platforming process. *Petrol. Processing* 6(3):264-267

With G. R. Donaldson. Platforming of pure hydrocarbons. *Ind. Eng. Chem.* 43:2102.

1952

With K. J. Sterba. Pyrolyric and catalytic decomposition of hydrocarbons. *Ind. Eng. Chem.* 44:2073.

1955

With G. R. Donaldson and L. F. Pasik. Dehydrocyclization in Platforming. *J. Ind. Eng. Chem.* 47:731-735.

1958

With C. V. Berger. Catalytic reforming. In *Advances in Petroleum Chemistry and Refining,* vol. 1, eds. K. A. Kobe and J. J. McKetta Jr., pp. 386-427. New York: Interscience Publishers.

1964

With H. S. Bloch. Duofunctional platinum catalysts in the petroleum industry. *Platinum Met. Rev.* 8(1):2-8.

With G. R. Donaldson and F. J. Riedl. Mechanisms of cyclohexane conversion over platinum-alumina catalysts. In *Proceedings of the Third International Congress on Catalysis, Amsterdam, July 20-25*. pp. 294-307. Amsterdam: Interscience Publishers.

1967

Lucky Alva. *Res. Manage.* 10:135-139.

1970

With E. L. Pollitzer and J. C. Hayes. The chemistry of aromatics production via catalytic reforming. In *Refining Petroleum for Chemicals,* pp. 20-37. Washington, D.C.: American Chemical Society.

1971

With R. L. Burwell Jr. Catalysis. *Sci. Am.* 225(6):46-58

1973

Performance and poisoning of dual functional catalysts. In *Abstracts of Papers, American Chemical Society 165th National Meeting, Dallas, TX,* p. COLL-25. Washington, D.C.: American Chemical Society.

1976

Concerns about energy. *Ind. Eng. Chem.* 15:89.

1978

Catalysis of chemical reactions. In *1978 Yearbook of Science and the Future,* pp. 236-249. Chicago: Encyclopedia Britannica.

1988

With D. M. Rebhan. A kinetic and mechanistic study of cyclohexene disproportionation: An example of irreversible hydrogen transfer. *J. Catal.* 111:397-408.

1993

Transportation costs and the national debt. Letter to the editor. *Science* 262(5131):163.

1994

Creativity: Is anyone listening? *CHEMTECH* 74(9):10.

Photograph by Earl Colter

James D. Hardy

JAMES DANIEL HARDY

August 11, 1904–September 6, 1985

BY ARTHUR B. DUBOIS

Hardy came from a family that was interested in teaching. Born in Georgetown, Texas, in 1904, he attended a Mississippi military school for boys, and from this beginning worked his way up from astrophysicist to admiral. During his career, he swept the underwater mines from Anzio Beach to Normandy, taught astronauts how to fly the space capsule for reentry into Earth's atmosphere, and he calculated the heat load of sunlight on the bright side or of chill of blackness on the dark side of the moon. To reel in bluefish three at a time on Long Island Sound, he used an umbrella rig with a square frame and four lures, one on each corner. His major contributions to science were in finding out how humans regulate their body temperature.

My first encounter with Hardy was as a child watching him play vigorous squash with Harold G. Wolff on the 18th floor of New York Hospital. Each intended to raise the other's core temperature higher than his own. Hardy and Wolff measured pain threshold on each other, and they evaluated the intensity of pain by using my classmates at Cornell Medical School as guinea pigs. The intensity of pain was expressed as a 10-step rating scale, the Dol scale, in which 10 is maximum pain and is experienced by women in labor.

Hardy's life's work seems to have sprung from a physics textbook (Kimball, 1929) that was once on his bookshelf and is now on mine. That textbook formed the basis of his scientific career in that it was like an acorn that would grow into an oak tree. Pin oaks were Hardy's favorite trees, which he planted on his lawn in Woodbridge, Connecticut, and in front of the John B. Pierce Laboratory in New Haven. Those trees would grow straight up as did Hardy's career.

Hardy's grandfather, James Malcolm Daniel, went from upstate New York to Indiana, where he met and married Laura Leonard, and then moved to Overton in East Texas. In 1901 they moved to Georgetown, Texas, where the grandfather became a railroad station agent.

Hardy's father, James Chappel Hardy, had moved from East Texas to Georgetown (the home of Southwestern University) to train for the ministry. There he met and married a student, Lulu Daniel, on November 11, 1903. Hardy's father founded a military academy for boys at Columbia, Tennessee, and subsequently founded the Gulf Coast Military Academy at Gulfport, Mississippi, in 1913.

Jim had a brother, Leonard, and sisters, Jessie, Verona, and Laura. When Jim went to Gulfport High School, his summer job was heavy work loading gravel. He became a football quarterback and was on the track team and debating society. Turning 17, Jim attended Southwestern University, a small Methodist college in Georgetown, Texas, for his freshman and sophomore years. Becoming interested in physics and math, he transferred to the University of Mississippi, where astronomy and physics were his favorite subjects and W. L. Kennon and Vice-Chancellor Alfred Hume were his favorite professors. Since, as he wrote in his autobiographical note for the National Academy of Sciences, he was too small and not strong enough to become competitive in sports, he turned instead to studies, the debating society, and girls. He was given a job as instructor in physics, and at graduation in 1924 was awarded "special distinction" on his degree, awarded magna cum laude. Here his own notes end.

Hardy continued his training in physics, astronomy, and infrared spectroscopy at the University of Mississippi, where he obtained an M.A. degree in 1925 and was an assistant professor from 1925 to 1927. In 1928 he married Augusta Ewing Haugh, of Atlanta, Georgia. He received scholarships at Johns Hopkins University, where he was awarded membership in the Phi Beta Kappa Society and a Ph.D. degree in 1930. His thesis (1930) was "A Theoretical and Experimental Study of the Resonance Radiometer," whose principle he ascribed to A. H. Pfund (1929). In his thesis Hardy used a pendulum to periodically interrupt a light beam that was focused on a thermocouple so that the voltage from the thermocouple would contribute to and amplify the swing of the galvanometer set to have a frequency of oscillation equal to the frequency of the pendulum. This boosted the amplitude 25 times, the way a small periodic push could make a person go higher on a swing. A second light source, bounced off the mirror of the first galvanometer, passed through a grating with bars two mm wide and gaps also two mm in width. The striped beam was focused on two adjacent gratings, separated by a bar's width to make them out of phase with each other, and because the oscillation period of the second galvanometer equaled that of the first galvanometer, the current from the thermocouples periodically kicked the second galvanometer into higher swings up to 2000 times as great as the displacement that would have resulted from the current generated by the first thermocouple. Weak signals of light or infrared from the primary source became strong light and electrical signals from the secondary source thrown periodically onto the second pair of thermocouples and their galvanometer. This maximized the optical signal yet minimized the random noise caused by jiggling of molecules and table tops.

The Pfund principle of frequency-dependent, optical-electrical-coupling amplification used by Hardy allowed him to overcome the dimness produced as he spread out the infrared spectrum with a prism to separate closely spaced absorption bands of crystalline versus fused quartz, of helium, of neon, of ammonia, or of hydrogen isotopes. He did these experiments between 1930 and 1932 while he was appointed as a National Research Council fellow at the University of Michigan.

At the time that Hardy was ready to look for a job as an astrophysicist, there was an economic depression, and no such jobs were available. However, on June 7, 1932, he was offered a position as a physicist at the Russell Sage Institute of Pathology under the directorship of Graham Lusk. The letter was signed by my father (Eugene F. DuBois), who was conducting research on body heat production using the Russell Sage calorimeter. In 1948 in his autobiographical note prepared for the National Academy of Sciences, Eugene DuBois wrote (to Hardy's pleasant surprise): "Perhaps my most important service was to bring James D. Hardy, a physicist, into the field of physiology where he could apply his basic training in radiation."

On October 1 of that year, Hardy began the study of the radiation of heat from the human body in the new building of New York Hospital at York Avenue and 68th Street. A series of papers from 1934-1936 on this subject began with: "I. An instrument for measuring the radiation and surface temperature of skin" (1934). He used a portable radiometer to measure radiant energy flux, which he expressed in calories per second per square centimeter of body surface. From this, Hardy used the Stefan-Boltzmann formula to calculate the skin temperature. The formula he used is S =

$S_0(T^4 - T_0^4)$, where S is the radiation emitted in calories per square cm of skin surface per second, S_0 is the Stefan-Boltzmann constant 1.37 x 10^{-12} cal/sec/cm^2, T the absolute temperature of the skin surface (273+ degrees C), and T_0 the absolute temperature of the reference thermocouple at room temperature. At a skin temperature of 27°C the wave length of infrared, calculated from quantum theory using Planck's constant, would be in the range of 5 to 10 or perhaps 5 to 20 microns.

Although shiny surfaces neither absorb nor radiate visible light and infrared light effectively, Hardy found that white skin absorbs and radiates long wavelength infrared just as well as blackened skin. Using a rock-salt prism, he found the absorption and emission wavelength of skin to be 5 to 10 microns, similar to that of a black body, or equal to a Leslie cube held at skin temperature. But the skin had some prominent bands in the absorption spectrum, bands that he attributed to organic compounds in the skin. A Leslie cube is a copper water bath that has a hollow cone, painted flat black inside, inserted into one side with its base facing outward and apex inward. For temperature calibration the spectrometer "looks" at the temperature radiated outward from inside the blackened cone.

When the skin is exposed to visible light, or to infrared of 1-2 microns wavelength, part of the incident energy is reflected by the skin, but part is absorbed and can be sensed as heat. With exposure to wavelengths of 2-10 microns, all the infrared energy is absorbed and may be felt. If the skin is painted with india ink, all the visible and infrared wavelengths are absorbed completely at the skin surface whence the heat may diffuse deeper into the skin. Hardy and Oppel (1937,2) measured the radiant heat from the skin immediately after such an exposure to radiant energy to determine the skin temperature responsible for the threshold for sensation of heat. Later, Hardy would measure the thresholds for warmth, cold, and pain sensations. Between 1937 and 1938 his attention turned to radiant heat loss as compared with convective and evaporative heat loss from the human body at a variety of environmental exposure conditions with or without clothing of subjects while they were enclosed in the Russell Sage whole-body calorimeter. The subject inside would aim the radiometer at designated points on the skin surface while Hardy, standing outside the calorimeter, took galvanometer readings of skin temperature to calculate radiant heat loss (1937,1).

To apply infrared methods in thermal physiology to medicine, Hardy had to understand preclinical and clinical sciences that he had not encountered in college, graduate school, or postgraduate training. Therefore, he took medical school courses in biochemistry, physiology, pharmacology, and neuroanatomy. Hardy maintained an up-to-date reprint file, as well as the latest textbooks.

From 1939 to 1942 Hardy, Wolff, and Goodell published papers on the sensation of warmth and pain resulting from radiant energy focused on blackened human skin. They used a light bulb as the source of heat, and a radiometer as a means of testing the incident rate of heat flux at which the sensation was felt. Sensation (warmth or pain) was dependent on the wavelength of radiation (visible, near infrared = 1-2 microns, or further infrared = 3-10 microns), and on the intensity of radiation (calories per cm^2 per second), the duration of exposure (seconds), skin pigmentation (white or black), skin area exposed (cm^2), and skin location (forehead or forearm). These factors were sorted out by Hardy and Oppel for warmth (1937,2) and cold (1938).

Based on the exploratory findings, Jim Hardy (physicist turned physiologist), Harold G. Wolff (medical neurologist), and Helen Goodell (research fellow in medicine) published a method to determine the pain threshold. They then began to use it to find out how analgesics changed the amount of radiant energy required to reach that pain threshold (1940). The voltage supplied to a 1000 watt lamp was controlled using a rheostat. The light from the lamp was focused by a lens and passed through a shutter that was set to open for exactly three seconds. The light then went through an aperture set to expose 3.5 cm^2 of skin that had been blackened with india ink, for example, on the forehead or forearm. The light intensity was initially set low, but was increased by steps until the subject reported pricking pain, which is distinct from the sensations of warmth or heat. The head or arm was removed from the aperture and replaced by a calibrated thermocouple to measure the radiant heat flux that had been responsible for heating the blackened skin. The heat flux producing threshold pricking pain was found to be 0.23 $cal/cm^2/sec$. Pain reached maximum when the heat flux was more than doubled and the skin blistered.

Various analgesics raised the amount of heat flux required to reach the threshold for pricking pain by about 35 percent above the control threshold (stated previously as 0.23 $cal/cm^2/sec$). These analgesics included acetylsalicylic acid, acetanilide, acetophenetidine, alcohol, barbiturate, and caffeine (1941). The investigators had found that morphine raised the pain threshold 70 percent or even 100 percent, the latter resulting in blistered skin. Codeine raised the threshold about 50 percent, or halfway toward the blistering intensity (1940). In comparison, ethyl alcohol equal to one or two drinks rapidly raised the pain threshold 45 percent for a short time (1942,1).

The reader should note that the concept of pain threshold was based on the radiant heat flux aimed at the blackened skin. At this point the skin temperature at which pain was felt was not being measured. That would have to wait until after World War II.

As Europe was erupting into World War II, Hardy went to the Navy and volunteered his services as a physiologist. The Navy said that it did not need a physiologist, but did need a physicist in mine sweeping, and that Hardy seemed to be suitable. They sent him to England to learn how to defuse unexploded bombs and sweep underwater mines in preparation for an invasion of Europe. As a result, he became the officer in charge of a minesweeping group at the invasion of Tunisia, Sicily, Salerno, Anzio, Normandy, and southern France. He was the mine officer for the Eighth Fleet in 1944 and minesweeping operations officer of the chief of naval operations. He was awarded the Purple Heart and Legion of Merit medals. He returned to inactive duty in February 1946, but continued to train a naval reserve unit and so became commander in June 1948, captain in 1952, and rear admiral in the naval reserves in 1961.

"Isn't it dangerous to try to defuse an unexploded bomb?" I asked.

"Well," Hardy replied, "You have to read the latest bulletins on the newer types of German bombs. Otherwise, when you try to defuse them, they can blow up."

Hardy's helpful nature is revealed by the following anecdote. While under orders to travel from one site to another, he had to wait for a few days until transportation arrived. He reported to the local commander of the post, and rather than do nothing, inquired in what way he might be of some use. The commander suggested that they needed someone to open and censor outgoing letters to make sure that the movement of ships or other operations would not be revealed in the correspondence. Hardy undertook the task until his transportation to the next duty post arrived. He always volunteered his services as a participant in events.

Hardy was bold but cautious, proud but modest, honest about people but always polite concerning them. He loved the South, but preferred to live in the North. These differences coexisted in the same person and put his blood pressure up and down like a yo-yo, also producing a duodenal ulcer that he hardly mentioned while he was directing minesweeping operations in the Atlantic during the war but which later required subtotal gastrectomy.

In 1946 Hardy returned to Cornell University Medical College where he resumed his studies on pain sensation in collaboration with Wolff and Goodell. Their work is explained in their book *Pain Sensations and Reactions* (1952,1). The first big step leading up to that was the invention of the Dol scale (1947).

Reasoning that pain was a sensation, the authors thought that a just noticeable difference (jnd) in intensity (I = intensity of pain) could be expressed using the Weber law, delta I / I, which Fechner expressed as K, the cumulative function, which is that the sensation is proportional to the natural logarithm of the intensity. Hardy measured the intensities of incident heat flux producing just noticeable differences in pain sensation ranging from threshold to maximum. There were 21 such steps of jnd over the range from threshold heat flux (0.22 cal/ cm^2/sec) to heat flux producing maximum pain (0.68 cal/cm^2/sec). Because it was not usually easy to distinguish less than two jnd steps, the authors decided on a 10-step scale, called the “Dol scale.” They made a graph of the number of Dols (Y axis) versus the stimulus intensity (X axis). The intensity of the stimulus was plotted as increasing linearly rather than logarithmically. The next year they compared the jnd pain scale to estimates of Dols produced by heat as fractions of the heat that had produced 8 Dols. The Dol scale produced by fractionations of 8 Dols equaled the Dol scale built up by summation of jnds. We see Dol scales on the wall of a patient's postoperative recovery room. The patient points to the scale to indicate the intensity of his or her pain as a guide for the doctor to prescribe an analgesic to relieve the pain. Hardy and Javert (1949) used this method to estimate the pain of childbirth, which they found equal to 10 Dols, unless relieved by analgesics.

Hardy, Goodell, and Wolff made a major discovery when they found that the pain threshold depended primarily on skin temperature and only secondarily on the amount of heat flux (1951). Starting in a room at 26°C they measured skin temperature and pain threshold, then moved to a room at 8°C to measure skin temperature and pain threshold on cold skin, then heated the skin to a skin temperature of 43°, each time measuring the heat flux required to produce pricking pain. In each case, pain occurred when the skin had reached 44.9°C, whether initially cold or warm. Reversible tissue damage occurred at that temperature of 44.9°. They concluded: "Thus it is the actual skin temperature level that is critical as regards noxious stimulation of the skin," suggesting that the rate of protein destruction exceeded the rate of protein repair, causing pain.

Alice Stoll and Jim Hardy (1952,2) were influenced by C.-E. A. Winslow and L. P. Herrington (1949), who had examined the heat exchange between a person and the surrounding environment. Stoll and Hardy adapted the thermal radiometer to enable them to measure indoor and outdoor temperatures as a way of examining the way the environment influenced radiant heating or cooling of a person. They could locate hot or cold spots in a wall, or find how Earth radiates to a clear sky at night.

Day and Hardy (1942,2) had constructed a gradient layer calorimeter to measure heat production and heat loss from the body of a premature infant. They placed the infant inside the calorimeter, which consisted of a copper box that had thermocouples inside and outside the box to measure the temperature gradient across the walls (air inside minus air outside). Generation of a measured rate of heat production in the box allowed conversion of thermal gradient to heat flux. The principle behind this early gradient layer calorimeter was used by Lawton, Prouty, and Hardy (1954,1) to construct a gradient layer calorimeter to measure heat loss and heat production in laboratory animals. Air temperature and humidity entering and leaving the chamber were measured at the same time as heat loss through the gradient layer surrounding the animal. Thermal and metabolic responses were complete within six minutes. Hardy needed this apparatus to make measurements on *Cebus* monkeys, cats, and dogs for comparison with earlier measurements on man (1954,2). Surprisingly, temperature regulation in dogs, not monkeys, was closest to that of humans. Hardy used a computer diagram to illustrate how the hypothalamic center seemed to regulate bodily heat loss. Later, he would test this diagram by measurements on the hypothalamus of dogs.

At Johnsville, Lipkin and Hardy gave birth to the first "computer diagnosis" of disease, in this case hematology disorders (1958,1). The computer consisted of punched cards in a shoe box. Diagnostic criteria had been obtained from a hematology textbook and were wedge-punched at the edge of each of 26 cards to match the symptoms and laboratory findings of the 26 blood disorders. Knitting needles were run through the holes that corresponded to the symptoms and laboratory findings of each of 80 patients, matching those to the diagnostic criteria wedge-punched into the edges of the set of 26 hematology cards. Shaking the box made the card whose criteria matched those of the patient drop out of the shoe box to show the diagnosis printed on the hematology card.

During Hardy's appointment in 1953 as research director of the Aviation Medical Acceleration Laboratory, U.S. Naval Air Development Center, in Johnsville, Pennsylvania, Hardy was in charge of the human centrifuge. He was also professor of physiology at the University of Pennsylvania, where he was in charge of the Ph.D. program for students in physiology, under John Brobeck, chairman of physiology.

At Johnsville, Hardy tested G tolerance of astronauts for the space program and tested equipment that would be flown in the Gemini and Apollo space flight missions of the National Aeronautics and Space Administration. Since astronauts would have to be protected against thermal stress of solar radiation, or dark sky, in orbit or on the Moon, rats and other animals as well as humans were used to assess the effect of thermal radiation on the skin. Re-entry from space into Earth's atmosphere would produce seven to ten G force on the body as the space capsule decelerated. Protection against these G forces could be tested using the human centrifuge (1959,1). To simulate various phases of space flight, the person riding in a capsule mounted on the arm of the centrifuge could control the roll, pitch, and yaw of the capsule, enabling astronauts to practice in advance how to control the rocket during its exit from and reentry into the atmosphere (1959,2). This way, Hardy taught the astronauts how to fly the space capsule.

A major step in understanding pain threshold came when Hendler, Crosbie, and Hardy (1958,2) devised a method to alternate between exposure of the skin to infrared radiation and the measurement of the resultant skin temperature. The trick was to use fan blades separated by open sectors that exposed the skin first to the heat source and then to the radiometer, which measured the skin temperature as the fan rotated at 12 revolutions per second. The method would be used to relate temperature sensation to skin temperature.

After studying the heat production and heat loss in the dog (1958,3), Hammel, Hardy, and Wyndam surgically implanted thermode tubes and thermocouples into the hypothalamus of the dog brain. The tubes would conduct hot or cold water deep into the brain substance to locate neural receptors and control centers sensitive to heat or cold (1960; 1961,1). Hardy's thinking on thermoregulation is best summarized in "The Physiology of Temperature Regulation" in *Physiological Reviews* (1961,2).

In 1961, at the age of almost 59, Hardy, who was under the stress of two simultaneous full-time jobs, or three if you include his being in charge of a naval reserve training unit, welcomed an offer to become director of the John B. Pierce Foundation Laboratory, in New Haven, so named for the benefactor who had endowed the New York Foundation in his will of 1916 to do research and teaching for the benefit of human comfort and hygiene (i.e., health) in the fields of heating, ventilating, and sanitation. The trustees of the Pierce Foundation of New York had established a laboratory building in New Haven in 1934 to provide Charles-Edward Amory Winslow, who was a Yale professor of public health, with a place where he could oversee research in indoor air quality and climate, which were among his interests. The initial staff under Winslow had been Lovic P. Herrington and A. Pharo Gagge. The trustees had also established a facility in New Jersey to design and test prefabricated houses suitable for families, and to test modular buildings for farms or factories. Hardy persuaded the trustees to focus the resources of the trust fund on physiology rather than on architecture. Hardy was appointed professor of physiology at Yale Medical School and later professor of public health. He negotiated an affiliation agreement with Yale to work jointly on teaching and research in areas of common interest to the foundation and Yale. This agreement attracted capable staff members to the laboratory, many of whom received academic appointments at Yale, where they taught without pay. Hardy began to supplement the annual budget with research grants from outside the foundation, such as those from the National Institutes of Health. This way he set aside enough endowment money received from the trustees to enable further construction of new laboratory space.

Scientists are promoted to administrative positions because of their achievements in science and often lack training in administration. But Hardy had learned administration first by working in several departments (medicine at Cornell Medical School, physiology at the University of Pennsylvania, the Navy aboard ship, and then in the Civil Service at the Naval Medical Acceleration Research Laboratory, in Johnsville). As civilian director of the Johnsville facility, Hardy supervised employees by using Civil Service guidelines. When he came to the Pierce Laboratory, Hardy had these systems of administration in his background, first academic, second military, and third Civil Service. His dealings with Yale and with the trustees, and his management of the laboratory were based on this experience.

Talented scientists joined or visited the lab between 1961 and 1965, for example, Jan A. J. Stolwijk, A. Pharo Gagge, H. Ted Hammel, T. Nakayama, Harold T. Andersen, Joseph Eisenman, R .F. Hellon, and Don C. Jackson. George Rapp and Robert Rawson and later Michel Cabanac took part. Women scientists initially included Dorothy Murgatroyd, Kerstin Southerland, and Dorothy Cunningham. Hans

Graichen built much of the equipment, and James Casby wrote the computer programs to collect and analyze laboratory data. Linc Dotlo managed the lab, and Gloria Trapkauskas was the secretary.

Hardy's interests during this time continued to focus on the internal mechanisms for regulation of body temperature not only in man (1966,1,2) but also in monkeys (1971), dogs (1960), rats (1974), and even such animals as lizards (1967), which although generally considered cold blooded, sought warm spots if they were infected with bacteria. Their hyperthermia seemed to be a substitute for the fever in mammals. John T. Stitt would explore the febrile response to endotoxin in rats (1974).

Hardy now had an opportunity to examine how man responded to changes in the thermal environment. He built specialized chambers in which the environment could be controlled and changed quickly. Partitional calorimetry was carried out within an all-weather chamber using different indoor climates where the temperature, humidity, wind velocity, and radiant heat load could be varied (1965). The respiratory oxygen uptake, skin temperature, and sweat rate would be monitored with a computer, and the comfort vote and thermal sensation would be recorded. People at rest or exercising on a bicycle ergometer would rate the comfort of the conditions while their core temperature, skin temperature, and sweat rate were measured. The effect of clothing on comfort was measured. Such measurements of subjects at rest or during exercise, with summer or winter clothing, were of use in defining the comfort zone.

From all this, Hardy and Stolwijk (1966) sketched out a heat transfer model of the passive temperature control system, and then added to this the body's temperature control system. A. P. Gagge translated these into comfort zones delineating acceptable conditions for people at rest or working in built environments, such as office buildings, factories, or vehicles. Such data are used by architects and heating and ventilating engineers as criteria to be met in designing buildings. The thermal physiology field was summarized in a collection of articles (1970,1) resulting from an international symposium held in New Haven in 1968.

As director, and even as director emeritus at age 70, Hardy kept up certain traditions. One was lunch club. At noon, Hardy would sit down at the head of the table, with his top senior on his right, next senior on his left, and so forth, alternating sides until reaching the junior person near the end. A local person, Rose, came in and prepared soup and sandwiches. Conversation usually included experiences and events, with war stories sprinkled among them. Lunch kept the group cohesive and informed.

Another tradition was the annual bluefishing expedition. From the dock at New London a select crew consisting of the head of technical services, two janitors, the business administrator, and Hardy would board the party boat to go fishing. Out in "the Race" the rods would be rigged and the lures deployed in the tide rip over a shoal. There may be some parallel between catching fish, getting research grants, and writing manuscripts. Hardy was good at all of these.

There was the tradition of the annual Christmas party. Mrs. Hardy (Augusta) would invite most of the laboratory members and their spouses to dinner at the Hardy house in Woodbridge, Connecticut. Someone would play the piano and lead the Christmas carols. People would sit around and chat. After dinner everyone would help clean up. Several days later Augusta would load the ironing board into the trunk of the Cadillac and the Hardys would head for their condominium in Winter Park, Florida, near Jim's brother, Leonard, who was a magistrate judge in Orlando.

There was the annual summer outing at a country site for hire. All the scientific and maintenance staff would attend with wives and children. The picnic grounds had wide lawns, tennis, volley ball, grills for hot dogs and hamburgers, beer, ice cream, and a lawn for playing bocce ball. Hardy and the janitors would roll the bocce balls to see who could get closest to the white ball used as a target. The janitors usually won at this game. Younger athletic scientists competed at volley ball. The children swam in the pool while the wives watched them for safety.

Hardy added other specialists to the staff in the fields of assessment of air quality, and of psychophysics to assess how the human senses respond to environmental changes. Eleanor R. Adair would extend Hardy's earlier studies on microwave absorption to include thermal responses to microwaves in monkeys trained to control their environment by adding cool or warm air as they sensed a need (Adair et al., 1970, cited in Hardy and Stitt [1971]). There were two conclusions. One was that microwaves could heat up body tissues while bypassing the skin's sensors, posing a thermal threat; the other was that behavior of trained animals could be used to judge their feeling of temperature. Hardy's lab became a center for visiting scientists from Great Britain, Europe, Canada, and Japan.

Hardy reached the age of 70 in 1974 and became Professor Emeritus at Yale and Director Emeritus of the John B. Pierce Laboratory. I, who had known and respected him since 1932 at Cornell Medical School and then while at the University of Pennsylvania, became director. Hardy continued for 10 more years as a consultant whose advice was of the utmost value in continuing the traditions of the Pierce laboratory.

Hardy received many awards for his work. Among these were the Eric Liljancrantz Medal of the Aerospace Medical

Association in 1960; Meritorious Civilian Service Award by the U.S. Navy in 1961; honorary doctor of science, Kansas City College of Osteopathy and Surgery, 1966; honorary doctor of science, Southwestern University, 1967; doctor honoris causa of the Faculty of Medicine and Pharmacy, University of Lyon, France, 1970; fellow of the American Academy of Arts and Sciences, 1970; member of the National Academy of Sciences, 1970; William F. Peterson Medal for Human Biometeorology in 1972; and Distinguished Alumnus Award, University of Mississippi, 1976. He was an invited speaker at many international conferences, traveled abroad with his wife, played golf, and as he passed 70 in 1974 continued to do all these things and advise the younger members when they asked him. Despite hypertension and some angina, which were controlled with pills, he continued to be active and greatly admired by everyone in the thermal field.

Hardy retired from his Navy career as line officer with the rank of rear admiral. On Navy Day, he would appear at the lab resplendent in full uniform with the gold stripe on his sleeve. Despite coronary arteriosclerosis, he lived to the age of 81. Highly respected, well liked, he and his wife were buried in Arlington National Cemetery with an 18-gun salute. He was survived by a sister, Laura Crites of Annandale, Virginia; a brother, Leonard, of Orlando, Florida; son James Daniel Hardy Jr., of Baton Rouge, Louisiana; and son George Frederick Hardy, of Pelham Manor, New York.

NOTE

A literature search for publications of James Daniel Hardy yielded a second author with the same name. Both authors are honored graduates of Ole Miss. Ours was a physiologist who lived in Woodbridge, Connecticut, whose publications originated in institutions on the East

Coast, whereas the other was a cardiovascular surgeon who lived and published in Jackson, Mississippi. They wrote about different subjects.

REFERENCES

Kimball, A. L. 1929. *College Physics* (4th ed. revised by A. L. Kimball Jr.) New York: Henry Holt.

Pfund, A. H. 1929. Resonance radiometry. *Science* 69:71-72.

Winslow, C.-E. A., and L. P. Herrington. 1949. *Temperature and Human Life*. Princeton, N.J.: Princeton University Press.

(Obituary information can be found in Adair, E. R. 1985. *Bioelectromag. Soc. Newsl.* 62; LaMotte, R. H. 1986. *Pain* 27:127-130; and Stitt, J. T. 1986. *Yale J. Biol. Med.* 59:83-84.)

SELECTED BIBLIOGRAPHY

1930

A theoretical and experimental study of the resonance radiometer. Dissertation. *Rev. Sci. Instrum.* 1:429-448.

1934

The radiation of heat from the human body. I. An instrument for measuring the radiation and surface temperature of skin. *J. Clin. Invest.* 13:593-604.

1937

With E. F. DuBois. Regulation of heat loss from the human body. *Proc. Natl. Acad. Sci. U. S. A.* 23:624-631.

With T. W. Oppel. Studies in temperature sensation. III. The sensitivity of the body to heat and the spatial summation of the end organ responses. *J. Clin. Invest.* 16:533-540.

1938

With T. W. Oppel. Studies in temperature sensation. IV. Stimulation of cold sensation by radiation. *J. Clin. Invest.* 17:771-778.

1940

With H. G. Wolff and H. Goodell. Studies on pain. Measurement of the effect of morphine, codeine and other opiates on the pain threshold and analysis of their relation to the pain experience. *J. Clin. Invest.* 19:659-680.

1941

With H. G. Wolff and H. Goodell. Measurement of the effect on the pain threshold of acetylsalicylic acid, acetanilid, acetophenetidin, aminopyrine, ethyl alcohol, trichlorethylene, a barbiturate, quinine, ergotamine tartrate and caffeine: An analysis of their relation to the pain experience. *J. Clin. Invest.* 20:63-80.

1942

With H. G. Wolff and H. Goodell. Studies on pain. Measurement of the effect of ethyl alcohol on the pain threshold and the "alarm" reaction. *J. Pharmacol. Exp. Ther.* 75:38-49.

With R. L. Day. Respiratory metabolism in infancy and childhood. *Am. J. Dis. Child.* 63:1086-1095.

1947

With H. G. Wolff and H. Goodell. Studies on pain: Discrimination of differences in intensity of a pain stimulus as a basis of a scale of pain intensity. *J. Clin. Invest.* 26:1152-1158.

1949

With C. T Javert. Studies on pain: Measurement of pain intensity in childbirth. *J. Clin. Invest.* 28:153-162.

1951

With H. Goodell and H. G. Wolff. The influence of skin temperature upon the pain threshold as evoked by thermal radiation. *Science* 114:149-150

1952

With H. G. Wolff and H. Goodell. *Pain Sensations and Reactions.* Baltimore: Williams and Wilkins.

With A. M. Stoll. A method for measuring radiant temperatures of the environment. *J. Appl. Physiol.* 5:117.

1954

With R. W. Lawton and L. R. Prouty. A calorimeter for rapid determination of heat loss and heat production in laboratory animals. *Rev. Sci. Instrum.* 25:370-377.

Summary review of heat loss and heat production in physiologic temperature regulation. Report No. NADC-MA-5413, 1-46, Oct. 14. Johnsville, Pa.: Bureau of Medicine and Bureau of Surgery.

1958

With M. Lipkin. Mechanical correlation of data in differential diagnosis of hemotological diseases. *J. Am. Med. Assoc.*166:113-125.

With E. Hendler and R. Crosbie. Measurement of heating of the skin during exposure to infrared radiation. *J. Appl. Physiol.* 12:177-185.

With H. T. Hammel and C. H. Wyndam. Heat production and heat loss in the dog at 8-36ºC environmental temperature. *Am. J. Physiol.* 194:99-108.

1959

Acceleration problems in space flight. In *Proceedings of the XXI International Congress of Physiological Sciences*. Symposia 1-12, Buenos Aires, August 9-15, Buenos Aires: Congreso International de Ciencias Fisiológicas.

With C. C. Clark. The development of dynamic flight simulation. *Aerospace Eng.* 18:48-52.

1960

With H. T. Hammel and M. Fusco. Thermoregulatory responses to hypothalamic cooling in unanesthetized dogs. *Am. J. Physiol.* 198:481-486.

1961

With T. Nakayama and J. S. Eisenman. Single unit activity of anterior hypothalamus during local heating. *Science.* 134:560-561.

The physiology of temperature regulation. *Physiol. Rev.* 41(3):521-605.

1965

With A. P. Gagge and J. A. J. Stolwijk. A novel approach to measurement of man's heat exchange with a complex radiant environment. *Aerospace Med.* 36:431-435.

1966

With J. A. J. Stolwijk. Partitional calorimetric studies of man during exposures to thermal transients. *J. Appl. Physiol.* 21:1799-1806.

With J. A. J. Stolwijk. Temperature regulation in man—a theoretical study. *Pfluger's Arch.* 291:129-162.

1967

With M. Cabanac and T. Hammel. *Tiliqua Scincoides*: Temperature sensitive units in lizard brain. *Science* 158:1050-1051.

1970

With A. P. Gagge and J. A. J. Stolwijk, eds. *Physiological and Behavioral Temperature Regulation.* Springfield, Ill.: Charles C. Thomas.

With J. D. Guieu. Effects of preoptic and spinal cord temperature in control of thermal polypnea. *J. Appl. Physiol.* 28:540-542.

1971

With J. T. Stitt. Thermoregulation in the squirrel monkey (*Saimiri sciureus*). *J. Appl. Physiol.* 31:48-54, citing E. R. Adair, J. U. Casby, and J. A. J. Stolwijk. Behavioral temperature regulation in the squirrel monkey: Changes induced by shifts in hypothalamic temperature. *J. Comp. Physiol. Psychol.* 72(1970):17-27.

1972

With W. Wunnenberg. Response of single units of the posterior hypothalamus to thermal stimulation. *J. Appl. Physiol.* 33:547-552.

1974

With J. T. Stitt and J. A. J. Stolwijk. PGE_1 fever: Its effect on thermoregulation at different low ambient temperatures. *Am. J. Physiol.* 227:622-629.

1975

With R. L. Day, L. M. Kitahata, F. F. Kao, and E. K. Motoyama. Evaluation of acupuncture anesthesia: A psychophysical study. *Anesthesiology* 43:507-517.

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Glen King

CHARLES GLEN KING

October 22, 1896–January 23, 1988

BY JOHN E. HALVER AND NEVIN S. SCRIMSHAW

CHARLES GLEN KING WAS A brilliant research biochemist, well known in the world for his isolation of vitamin C. Glen, as he was known by his friends, was an active pioneer researcher in the young science of nutrition. Through a series of meticulously designed experiments over a period of 10 years, he pursued and finally isolated crystalline ascorbic acid and proved it was the antiscorbutic factor for guinea pigs, and subsequently for humans. In 1942 after winning fame for his biochemical contributions to nutrition, he became the first director of the Nutrition Foundation. In this capacity he had a unique ability to identify young scientists at the beginning of their career and to provide them support through the Nutrition Foundation for their early research. As president of the International Union of Nutrition Sciences from 1950 to 1953, he introduced a system of commissions and committees that permanently converted it from a passive to a proactive organization. He was uniquely effective nationally and internationally with leaders in both the private and public sector because of his dedication and absolute integrity. King held professorships at the University of Pittsburgh and Columbia University. He received honorary degrees from the University of Pittsburgh, Washington State University, Drexel Institute of Technology, Denison

University, and the University of Lignan. He was elected to the National Academy of Sciences in 1951, and received numerous other honors during a long and distinguished career in nutritional biochemistry.

THE EARLY YEARS

Charles Glen King was born on a homestead in Entiat, Washington, on October 22, 1896. He was raised on an apple farm along the Entiat River, a tributary of the upper Columbia River. He attended one-room schools in Entiat and in Ashland, Kansas, where he lived with an aunt for a number of years on a wheat and corn ranch. He returned to Entiat at age 11 and later went to college in Pullman at Washington State College (now a university). At first he majored in geology but in his junior year he changed his major to chemistry. He also had an active interest in religion. With the onset of World War I he interrupted his studies and volunteered to serve his country in the military in a heavy machine gun company. He was almost 22 years old before he received his bachelor of science in chemistry in 1918 from Washington State College (WSC). At WSC he was president of the Lambda Chi Alpha fraternity and excelled in his academic studies.

He married Hilda Bainton on September 11, 1919, after returning from his Army service with the 12th Infantry Machine Gun Company. They moved to Pittsburgh, Pennsylvania, where their three children, Dorothy King Hammel, Robert Bainton King, and Kendall Willard King were born. Hilda King and Marieta Loren, who was married to a close fraternity friend of Glen's, reared their first children near each other in Pittsburgh. Glen became the godfather of Jane Loren, my wife (J.E.H.). "Auntie Hilda" and Glen insisted that the babies receive fresh orange juice each morning even though vitamin C had not yet been isolated and crystallized. Glen knew it was an important nutrient for children in 1922 when he started his pursuit to isolate and identify vitamin C.

After receiving his B.S. degree, he moved to the University of Pittsburgh and received his master's degree in chemistry in 1920, and his Ph.D. in organic chemistry in 1923. He served as an instructor in the Department of Chemistry at the University of Pittsburgh from 1920 to 1926. During 1926-1927 he was a postdoctoral associate with W. Sherman at Columbia University in New York City, and he spent six months in 1929 as a postdoctoral associate with F. G. Hopkins at Cambridge, England. He returned to the University of Pittsburgh as assistant professor from 1927 to 1930 and a professor from 1930 to 1942. He received his first research grant from the Buhl Foundation and later formed a group of research faculty in chemistry, biology, and physics. In 1942 he moved to New York City to become director of the Nutrition Foundation. He also became a part-time visiting professor at Columbia University from 1942 to 1946 and a professor in the chemistry department from 1946 to 1962.

THE VITAMIN C STORY

In 1927 Albert von Szent-Györgyi reported a reducing substance similar to hexuronic acid isolated from the adrenal gland. In 1930 R. B. McKinnis and C. G. King published a positive suggestion that hexuronic acid could be vitamin C (*J. Biol. Chem.* 87:615). Glen King and his graduate students H. L. Sipple, O. Bessie, F. L. Smith, W. A. Waugh, and J. L. Svirbely were able to prepare vitamin C concentrates from lemon juice and studied the properties of vitamin C fractions from 1929 to 1931. Otto Bessie, from Montana, did not trust J. L. Svirbely, from Hungary, and on one occasion their disagreements ended in physical blows.

Finally a crystalline compound was isolated and the chemical nature of vitamin C was reported in *Science* on April 22, 1932. The early announcement on the chemical nature of vitamin C was followed by a more lengthy and descriptive report in the *Journal of Biological Chemistry* by Waugh and King in 1932.

Svirbely, who had been King's graduate student for only one year during the isolation of vitamin C, returned to Hungary and was hired by Szent-Györgyi in 1931 to isolate a reducing factor in the adrenal cortex and in cabbage. He used his experience in King's laboratory to isolate these extracts and fed these preparations to growing guinea pigs. No scurvy developed. Fifteen days after King and Waugh published their isolation and crystallization of vitamin C from lemon juice, Svirbely and Szent-Györgyi announced on May 7, 1932, that vitamin C is a single substance and identical to hexuronic acid. It has been suggested that Szent-Györgyi was the first to isolate vitamin C, however King and his students had isolated and crystallized this compound, and published their results, in advance of the Szent-Györgyi team announcement. In 1937 Szent-Györgyi received the Nobel Prize in physiology or medicine for "his discoveries in connection with the biological combustion processes, with special reference to vitamin C and the catalysis of fumaric acid." It was a lifelong disappointment to King that in Europe Szent-Györgyi was credited with the first identification of vitamin C. Subsequently, Glen King and his research team in the period 1932-1942 published over 50 papers on ascorbic acid characteristics, deficiencies, and enzyme activities in various animal tissues. This work came to fruition when Burns and King reported the synthesis of 1-C^{14}-L-ascorbic acid in *Science* in 1950. Glen continued to pursue the role and functions of ascorbic acid until his last publication dealing with metabolic products of L-ascorbic-acid in 1958.

OTHER FIELDS OF ENDEAVOR

In 1942 Glen King became the director of the Nutrition Foundation in New York City and made it the leading private supporter of nutrition research for the next 21 years. In leaving Pittsburgh for New York he shifted his focus from personal biochemical research and became focused on nutrition and public service. He was responsible for strengthening many nutrition departments in U.S. universities and successfully enlisted industry support in these efforts. He had a remarkable ability to inspire action and consensus in his relations with both industry and academia. As director of the Nutrition Foundation he was extraordinarily effective in obtaining funds from industry and in gaining scientific recognition for this fledgling science.

Glen had high standards for his own research and that sponsored by the Nutrition Foundation. He had a unique capacity to identify promising young nutrition researchers and took a direct personal interest in helping their careers. Several subsequent members of the National Academy of Sciences are indebted for this support. He established the Nutrition Foundation Journal, *Nutrition Reviews*, which was unique at the time and contributed importantly to the development of nutrition research in the United States.

In 1951 he was elected to the National Academy of Sciences and soon became active in the agricultural policy of the United States. He helped establish the U.S. Department of Agriculture's Plant, Soil, and Nutrition Laboratory, in Ithaca, New York. He joined the Advisory Council of the National Institutes of Health's Institute of Arthritis and Metabolic Diseases in 1955. After his retirement from teaching in 1962, he became the associate director of the Institute of

Nutrition Sciences at Columbia University. He also began serving as a consultant to the Rockefeller Foundation.

The International Union of Nutritional Sciences elected Glen King as its president in 1960. Glen built up an active network of international commissions and committees to deal with the manifold aspects of nutrition. In 1972 the U.S. National Committee for the International Union of Nutritional Sciences formally expressed its appreciation for his outstanding services over his many years of association with the IUNS and international nutrition programs.

Under his leadership the Nutrition Foundation provided the first research support to the newly established Institute of Nutrition of Central America and Panama (INCAP) and followed this with over 10 years of invaluable service on its Technical Advisory Committee (TAC). From 1951 to 1961, accompanied by Hilda, he went to Guatemala for at least a week every year to participate in the TAC review of the research progress of INCAP in the preceding year. Professional staff members had to present their research for evaluation by the committee. During the 1958 TAC meeting when the president of Guatemala called his name and that of two others at a large formal evening reception in the National Palace to come forward, there was no answer because he and another future National Academy of Sciences member, William Darby, whose name was also called, were still at INCAP finishing the committee report. They missed the reception and received the Order of Rodolfo Robles, Guatemala's highest honor for a scientist, rather unceremoniously the next day. After the TAC meetings, he often visited ministries of health in the other Central American countries in support of INCAP. In his exchanges with ministers and directors of health he inspired confidence and displayed the same effectiveness in promoting nutrition issues with them as he did with the members of his Nutrition Foundation Board.

In 1960 he persuaded the president of the Massachusetts Institute of Technology, who was serving as chairman of Glen's board, to establish a department of food science and technology and appoint the director of INCAP as its first department head(N.S.S.). King later served on the visiting committee of this department and was proud that it proved highly successful and internationally recognized. He also encouraged the development of the Department of Nutrition in the School of Public Health of Columbia University in New York and served on its faculty during his entire time in New York.

He was active on the Food and Nutrition Board of the National Research Council and the U.S. National Committee for the International Biological Program. His 20 years of service involved work on National Research Council activities, the Food and Nutrition Board, the U.S. Committee for the IUNS, and the International Biological Program.

His work, in addition to over 80 vitamin C publications, encompassed studies on fats and oils in human diets, and in microbiology of *Clostridium*. He even worked on electrical pasteurization of milk to minimize bacterial contamination and assure a healthy milk supply. His major scientific reviews covered not only recent advances in vitamin C research nationally and internationally but also many other factors involved in good human health practices. As the field of nutrition and health progressed Glen promoted programs in stabilizing food supplies for essential fats and good quality programs for the developing world. He was vitally interested in proper education of both basic and applied nutrition principles in the world populace and in the application of these in clinical nutrition. Food science and engineering were also on his agenda because he was convinced that nutritionally balanced food preparation was absolutely essential for determining population acceptance for better health and vigorous living.

Glen King served in many other leadership roles that focused on the impact of nutrition on sound health. He was past president of the American Institute of Nutrition, American Society of Biological Chemists, and the American Public Health Association. He was an advisor to the Williams Waterman Foundation, a member of the American Association for the Advancement of Science, the Chemist Club, the Masons, Sigma Xi, and Lambda Chi Alpha fraternity. His contributions to research and public service were recognized by the Pittsburgh Award and the Spenser Award of the American Chemical Society, John Scott Award, Bicentennial Award of the city of Pittsburgh, Conrad Elvehjem Award of the American Institute of Nutrition, Nicholas Appert Medal of the Institute of Food Technologists, Grocery Manufacturers of America Award, and the Gold Medal in Biological Sciences from the Czechoslovakian Academy of Sciences. He held honorary memberships in the American Dietetic Association and the British Royal Society of Health. He felt strongly that dieticians were the logical group of professionals to teach the general population about wholesome nutrition.

One of his most important contributions came during his term as president of the International Union of Nutritional Sciences (IUNS). Activities of this global union had been limited to an international congress every three years. His leadership established a network of commissions and committees that drew in scientists from many countries and made the IUNS an effective international contributor to the development of all aspects of nutrition sciences from the laboratory to the community and from plant and animal nutrition to clinical and public health nutrition. It continues to function through active committees and task forces. He also guided the incorporation of the IUNS on an equal basis with other major scientific unions in the International Council of Scientific Unions, a real breakthrough in the recognition of nutrition science.

Glen loved the outdoors and gardening. His rose garden at his home in Scarsdale was the envy of neighbors and his pride and joy. He had over 100 rose plants. The peach rose was his favorite.

Throughout his career in pioneering definitive scientific research, he maintained his interest and commitment to the church. He consistently attended services at the First Baptist Church of Pittsburgh and served on its Board of Directors. For 35 years he served as a member and deacon or trustee of the Riverside Church. After retirement in 1974, Glen and Hilda moved to a managed retirement community, Kondal at Longwood, at Kenatt Square, Pennsylvania. After three years of searching for a new church affiliation Glen and Hilda joined the Religious Society of Friends (the Quakers).

Charles Glen King was a brilliant research scientist with a generous and gentle heart and soul. He was always interested in the welfare and promotion of others. As a scientific mentor to many searching investigators, he always advised that "any good scientist has so many ideas he can never complete, that these should be shared or given to others to advance understanding in the field of nutritional biochemistry, physiology, and metabolism." He told one of us (J.E.H.) this in 1950 at his first meeting with Glen King after being hired to start the nutrition research and diet development program for the U.S. Fish and Wildlife Service.

His daughter Dorothy became an accomplished pianst, his son Bob became a physician and, following in his father's footsteps, his son Kendall became a well-known nutrition scientist in his own right. Kendall served as technical secretary of the Williams-Waterman Committee of the Research Foundation before joining the facultyr at Virginia Polytechnic Institute until his premature death. Glen and his wife, Hilda, were steadfast partners in their dedication to science and service. Glen was not tall and was always slender, but the force of his personality transcended his physical size. He always ate his orange every day to get his vitamin C and to the end was convinced that the U.S. Food and Nutrition Board recommendations for this vitamin were too low. He passed away on January 23, 1988, at the age of 91. His mentorship for humanity will be missed but remains a superb example for any young scientist.

SELECTED BIBLIOGRAPHY

1929

With E. J. Quinn and B. H. Dimitt. A study of the effects of certain diets upon the growth and form of albino rats. *J. Nutr.* 2:7.

1930

With D. R. P. Murray. The sterochemical specificity of esterases. I. The affinity of liver esterases for optically active alcohols. *J. Biochem.* 24:190.

1931

With J. L. Svirbely. The preparation of vitamin C concentrates from lemon juice. *J. Biol. Chem.* 94:483.

1932

With W. A. Waugh. Chemical nature of vitamin C. *Science* 75:357.

With W. A. Waugh. The isolation and identification of vitamin C. *J. Biol. Chem.* 97:325.

With W. A. Waugh The vitamin C activity of hexuronic acid from suprarenal glands. *Science* 76:630.

1935

With M. L. Menten. The influence of vitamin C level upon resistance to diphtheria toxin. I. Changes in body weight and duration of life. *J. Nutr.* 10:129.

1936

With I. Selleg. The vitamin C content of human milk and its variation with diet. *J. Nutr.* 11:599.

With A. Sigal. The relationship of vitamin C to glucose tolerance in the guinea pig. *J. Biol. Chem.* 116:489.

Vitamin C, ascorbic acid. *Physiol. Rev.* 16:238.

1938

With E. Silverblatt. Observation on the aerobic oxidation of vitamin C in plant juices. *Enzymologia* 4:222.

1939

With R. R. Musulin, R. H. Tully, and H. E. Longenecker. Vitamin C synthesis and excretion by the rat. *J. Biol. Chem.* 129:436.

With B. F. Daubert. A method for the preparation of alpha, beta-diglycerides of fatty acids. *J. Am. Chem. Soc.* 61:3328.

1941

With B. F. Daubert. Synthetic fatty acid glycerides of known constitution. *Chem. Rev.* 29:199.

With J. Harrer. Ascorbic acid deficiency and enzyme activity in guinea pig tissues. *J. Biol. Chem.* 138:111.

With H. W. Karn and R. A. Patton. Nutritional deficiency as a factor in the abnormal behavior of experimental animals. *Science* 94:186.

1949

With V. Allfrey, L. J. Teply, and C. Geffen. A fluorometric method for the determination of pteroylglutamic acid. *J. Biol. Chem.* 178:465.

1950

With J. J. Burns. Synthesis of 1-C^{14}-L-ascorbic acid. *Science* 111:257.

1951

With S. S. Jackel and E. H. Mosbach. Ion exchange separation of ascorbic acid and isolation of the 2, 4,-dinitrophenylosazone. *Arch. Biochem. Biophys.* 31:442.

With J. J. Burns and H. B. Burch. The metabolism of 1-C^{14}-L-ascorbic acid in guinea pigs. *J. Biol. Chem.* 191:501.

1952

Basic research and its application in the field of clinical nutrition. *J. Clin. Nutr.* 1:1-6.

1957

Fats in nutrition and health. *J. Am. Oil Chem. Soc.* 34:559.

1958

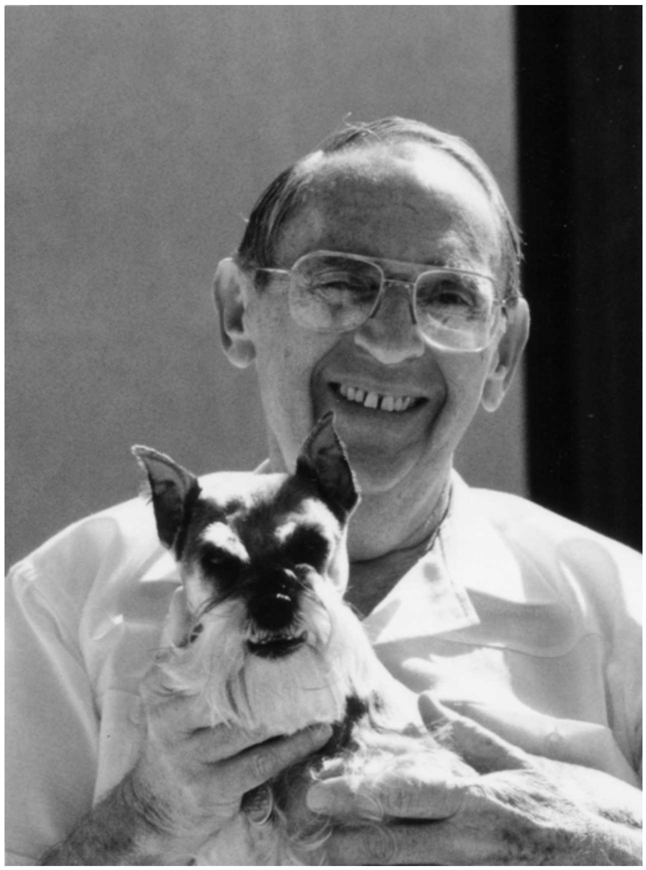
With P. C. Chan and R. R. Becker. Metabolic products of L-ascorbic acid. *J. Biol. Chem.* 231:231.

1958

The chemist and engineer in fifty years of food processing. *Ind. Eng. Chem.* 50:89A.

1959

Advances in nutrition. *J. Am. Diet. Assoc.* 35:109.



John I. Lacey

JOHN I. LACEY

April 11, 1915—June 27, 2004

BY J. RICHARD JENNINGS AND
MICHAEL G. H. COLES

FOR APPROXIMATELY 30 YEARS John I. Lacey defined the field of psychophysiology. His pioneering work relating physiological measures in humans to their psychological function subsequently influenced the fields of behavioral medicine and neuroscience. He died on June 27, 2004, at age 89. His wife and coworker, Beatrice C. Lacey, had died earlier, on November 9, 2000. They retired from academic roles and their collaborative research in the early 1980s. Both worked for over 30 years at Fels Research Institute in Yellow Springs, Ohio, and continued to live there prior to retiring to Rancho Mirage, California.

We first encountered John Lacey when he was in the middle of his career. At that time John and Bea's research was becoming a dominant force in psychophysiology, and we were both drawn to them because of their enthusiasm and because of the novelty of their ideas. In their early careers the Laceys adopted the contemporary view, embodied in general arousal theory, that the reticular activating system was the neural substrate of a central arousal system. Central arousal, as a regulator of both neural and psychological function, was then viewed as a key concept for both psychology and neuroscience. However, empirical data collected by the Laceys failed to demonstrate the generality of arousal across brain and bodily systems. Furthermore, their data suggested that homeostatic regulatory systems of the body appeared to influence the brain. Brain and body were engaged in a two-way communication that had implications for human performance.

The excitement of this period did not arise by chance but was a result of a combination of elements that ultimately defined the field of psychophysiology. The field was defined by an alliance of psychology, physiology, medicine, and engineering. The inclusion of medicine and engineering requires some explanation. The Laceys' work on individual and situational differences in autonomic response patterns opened applications to medicine. Why do some individuals become ill and others not when exposed to similar pathogens and situations? Why are some individuals more or less resilient to the effects of psychological stress? And why does the pattern of responsivity among different autonomic systems vary as a function of the situation? This work on what they called "individual response stereotypy" and "situational stereotypy" provided an alternative scientific basis for the medical field of psychosomatic medicine, which was becoming disillusioned with strictly psychoanalytic approaches. The assessment of human physiological responses also required engineering. Sensitive biological amplifiers that were resistant to electrical interference were not readily available. Instruments that transduced the biological signals of relevance were also not routinely manufactured. Investigators who wished to study sweating palms due to stress typically built their own electrodes and bridge circuits to derive the galvanic skin response measures.

John fit the field well with interests in all four members of the alliance. His biographers (*American Psychologist,* 1977, 1985) tell us of his initial study of engineering at Cornell and that his reading during recovery from a fencing team injury might have turned John away from a career in engineering and toward biology and psychology. His Cornell degree trained him thoroughly in experimental psychology, but World War II added further skills. He became familiar with research on individual differences and associated statistical techniques in the Army Air Force's Psychological Testing and Classification Program. All of these skills (many of which Bea shared) made John an ideal choice for research investigator at Fels. They built a laboratory at Fels using his engineering skills, and started a research program designed to examine individual differences and normal development using carefully designed experiments.

John's early work, which was continued during the initial years at the Fels Institute, focused on defining meaningful characteristics of individuals that would then predict subsequent health and performance. Conceptually many investigators at the time thought it likely that individuals would differ in peripheral physiological levels and responses; these differences, if consistent over time, would then define a psychophysiological personality type. Later commentators noted that this seemed to harken back to medieval classifications based on different bodily humors, such as the *sangwyn franklin* and the *colerik reve* in Chaucer's *Canterbury Tales.* John and Bea's approach was hardly medieval, however. Armed with their knowledge of statistics and methodology growing from their World War II experience, they set out to determine empirically whether individuals would show consistent levels of activity in physiological systems controlled by the autonomic nervous system and whether they would respond consistently in these systems to different stimuli. If reliable psychophysiological types could be identified, then the origins and development of these types could be studied, their sensitivity to life experiences assessed, and the relationship to both psychiatric and physical disease examined. Difficult problems had to be overcome first. Each of the physiological measures studied had different characteristics; heart rate was assessed as beats per minute, palm sweating as electrical resistance, brain waves as voltage changes. The Laceys (e.g., Lacey and Lacey, 1962) developed a method for a combining measure based on the mean and variability of the measure.

In the case of responses to stimuli the Laceys addressed another issue. The amplitude of the response to a stimulus frequently appeared to depend on the level of activity in the measure just prior to stimulation. This correlation between initial level and response amplitude had to be considered if responsivity was to be isolated. They introduced a regression approach to this problem, variants of which are still being employed. Using their methods, the Laceys were able to show that autonomic patterns were to some degree a consistent characteristic of an individual. Their work also showed them that the nature of the stimulus as well as individual characteristics determined the exact pattern of change across a set of autonomically controlled physiological responses. The stage was then set to see if these autonomic patterns characterizing individuals related to other individual characteristics, such as personality types, motivational styles, or proneness to disease. For example, if a person was characterized by strong blood pressure responses to a psychological challenge, would that person be more likely to develop hypertension than a person responding primarily with sweaty palms? The contemporary field of behavioral medicine is finding support for this last conjecture in that cardiovascular reactivity now appears to be a risk factor for hypertension and coronary heart disease (e.g., Schneiderman et al., 2005; Jennings et al., 2004). The notion of stereotyped responses to situations has also been incorporated into behavioral medicine. Using ambulatory physiological and behavioral recordings, investigators now attempt to determine whether individuals reactive to laboratory stressors do or do not encounter stressful situations that alter their physiology during their normal work day. Presumably, risk for cardiovascular disease will be greatest for those with the combination of an environment with situations that elicit cardiovascular reactivity and an individual tendency to show large cardiovascular responses.

Although their early work had substantial influence on the fields we now term behavioral medicine and psychosomatic medicine, the careers of the Laceys were taken in a somewhat different direction by their testing of the concept of general arousal. Physiological investigations, most particularly of the reticular activating system of the brain, had suggested that daily events and personal characteristics led to a level of activity in the brainstem that then had critical modulating influences on the rest of the brain and thus on behavior. These central effects were presumed to be mirrored rather directly in neural outflow within the autonomic nervous system, known also to have control centers within the brain stem. Levels of arousal assessed from autonomically controlled variables would presumably predict an individual's affective state and performance capabilities. The concept of general arousal suggested that a degree of activation would be observed to be consistent across output systems—creating a simpler theory of psychophysiology type than envisaged in the Laceys' earlier work. The Laceys set out to define general arousal given that their approach and measures were so suitable to this then-popular new concept. However, the concept of general arousal was quickly challenged by the Laceys' results. They developed an innovative scale in which different situations were used to elicit responses in a variety of autonomic systems measured concurrently. The results failed to show general arousal. There were dissociations among different autonomic measures such that different person-environment interactions induced different patterns of physiological response. One of the most striking findings was that heart rate decreased during active attention to environmental stimuli.

Their research questioning general arousal theory coincided with a number of related developments that enhanced the value of their empirical critique and the development of their alternative view. Questions about the necessity of the reticular activating system for cortical function were being raised in the physiological psychology and neuroscience communities. The general arousal concept did not fit neatly with the results from new research with such measures as the electroencephalogram and cortical evoked potential and with theoretical approaches to the physiology of cognition and emotion. More specifically related to the Laceys' work, Sokolov's (1958) view that a panoply of physiological responses were evoked while orienting to novel and significant events was becoming well known in the then-Western-World. Graham and Clifton (1966) published an influential article describing how heart rate slowed during orienting. We, in particular, were drawn to the idea that shifts in information processing from orienting or attention to internal processing could be detected in the direction of brief heart rate response to events (e.g., Coles and Duncan-Johnson, 1975; Jennings and Hall, 1980). In short, the Laceys led a change in thinking away from a solely arousal view to a view that both central and peripheral physiological responses could be meaningfully related to cognition as well as affect.

The Laceys moved forward by developing a bold neurophysiological theory for their results and creating precise experimental paradigms to study the patterning of autonomic responses during information processing. Their theory suggested that the activation of the baroreceptors reduced cortical integration of perceptual-motor events such that baroreceptor activation would interfere with performance, and deactivation (such as when heart rate slowed) would facilitate performance. Characteristically, this hypothesis was based on a combination of results from the neurophysiological literature and a detailed examination of a series of experiments from their Fels laboratory. Baroreceptors, which are buried in the wall of arteries in the carotid sinus and aortic arch, respond to the rate of change of pressure by sending signals with this information to the brain. As pressure rises in the arteries during the heartbeat, volleys of afferent information about pressure are sent to the brain. The neurophysiological literature suggested that enhanced signals from the baroreceptors reduced the amplitude of neural responses in the cortex. The results of the Laceys suggested that perceptual motor performance in humans was less efficient when the baroreceptors were most active during cardiac contraction. Their hypothesis met with variable empirical success; only some studies were able to replicate the influence of baroreceptor discharge on performance. On the positive side it engendered a hypothesis by others (e.g., Dworkin et al., 1979) that suggested that high blood pressure might be learned and maintained by the pain reduction reinforcement initiated by blood pressure increases (and consequent baroreceptor stimulation). The interrelationships of pain, blood pressure, hypertensions, and baroreceptor activation continues to be actively investigated. More importantly, by proposing an influence on sensory information processing by the body on the brain, the Laceys provoked a reconsideration of the importance of interoceptive information for our basic biology. Indeed, the vagal branch of the autonomic nervous system, a key component in the Laceys' thinking, is now being actively investigated because of its central role in carrying information about the body to the brain.

The Lacey hypothesis also brought a controversy that may not have been pleasant for the participants, but which nevertheless altered the field of psychophysiology. At a meeting in Denver in 1967 a former postdoctoral student of John Lacey, Paul Obrist, proposed what he considered an alternative formulation to explain why heart rate slowed during attention to environmental events. Based on animal conditioning experiments Obrist (1976) suggested that peripheral motor inhibition was the cause of the heart rate slowing, due to the intrinsic coupling between somatic and cardiovascular systems. He considered this a basic and simple biological explanation and initiated a heated discussion by suggesting that the Lacey hypothesis was less biological. Obrist elaborated on his ideas of the importance of cardiosomatic coupling and initiated a research program that ended up addressing the causes for hypertension and, as such, influencing the emerging field of behavioral medicine. The Laceys pursued their neurophysiological views, with both the Laceys and Obrist attracting adherents who ended up discussing the controversy more than the primary antagonists. The empirical data suggested that aspects of both their views were correct. A partial reconciliation of the individuals occurred at a Festschrift honoring the Laceys in 1982 (Coles et al., 1984).

John enjoyed meetings and interactions with his colleagues. He vacillated, however, from being totally engrossed and asking piercing questions during presentations to skipping sessions so he could tell stories to colleagues in the hallways. He reputedly advocated certain varieties of bass plugs—even to the extent once of having a number of colleagues pass one along under the table at a society banquet.

Over the course of his career John became increasingly involved in a number of scientific societies. He was active in the American Psychological Association, and equally active in societies related to psychophysiology, such as the American Psychosomatic Society and, of course, the Society for Psychophysiological Research. He played an important role in the formation of the now burgeoning Society for Neuroscience. He attempted to promote a continued interest in human as well as animal model work within the neuroscience community. Undoubtedly, he would be pleased by the reentry of human work into the Society for Neuroscience occasioned by the development of neuroimaging. He was also an active participant on several National Institute of Mental Health committees injecting a psychophysiological approach into their deliberations.

John was elected to the National Academy of Sciences in 1980. Prior to that, Beatrice and John together received the 1976 Distinguished Scientific Contribution Award from the American Psychological Association. A portion of the citation for that award is worth repeating:

> Arguing the inadequacy of traditional views of a unitary activational system, they have described a system with central feedback and dissociable subsystems. With superb technology and meticulous experiments, they have demonstrated that complex patterns of autonomic response are a measurable, characteristic of individuals, stable across years, and predictive of individual-environment transactions.

The interested reader is further referred to the thorough biography of Beatrice and John that follows this citation (*American Psychologist,* 1977). A similar (but at points, tongue in cheek) tribute to the Laceys also accompanied the earlier Distinguished Contribution to Psychophysiology Award from the Society of Psychophysiological Research (Stern, 1971). Beatrice and John also received the Psychological Science Gold Medal Award from the American Psychological Foundation in 1985. Again the citation and biography related to this award are informative (*American Psychologist,* 1985). Just prior to this, in 1982, Beatrice and John were honored at a Festschrift during which a dozen colleagues who had been strongly influenced by their work presented papers (Coles et al., 1984).

Despite an intention to retire, John was busy learning new techniques in his later career. In a visit to Yellow Springs around 1980 to talk to Bea and John about cardiac measures, we heard instead about John's new work with cortical evoked potentials. Few laboratories were using this measure, but John developed it in his laboratory at Fels based on information gained from visits in the laboratories of Grey W. Walter, Vahe E. Amassian, Karl H. Pribram, and Horace W. Magoun (during a Commonwealth Fund Fellowship). This work, published in 1980 together with Bea, described the concordance between cardiac and brain responses. Later during our visit, John entered his animal laboratory and demonstrated the success of an implanted device that stimulated the baroreceptors with varying acceleration to peak pressure. He planned to evaluate the precise sensitivity of the baroreceptors to this stimulation. At the end of the visit he did speak of retirement but only to say that computers had begun to fascinate him and that he wanted to build one from component parts.

Bea and John retired gracefully into private lives, attending to the families of their two children. They had visited the Palm Springs area of California at earlier times in their lives and became more attracted to it during retirement. John particularly was drawn to the warmth and the scenery of mountains and desert. John did woodworking and Bea gardened and they both enjoyed listening to jazz. Misfortune came soon, however. Two years after buying a home in Rancho Mirage, Bea became ill and was unable to continue to enjoy the home. After a period in special care, she died in 2000. Given the closeness of the couple, John's survival was a concern to all their friends and family. He proved resilient, however—even learning some Spanish to speak with a Guatemalan housekeeper that he employed. This resilience was challenged beyond measure, however, by the death of their daughter, Carolyn, in 2002 from leukemia. John also faced physical struggles with heart failure and then a broken foot. This occasioned a visit by his son, Robert, in 2004. The visit went well, but a few weeks later Robert died unexpectedly from a myocardial infarction. Four months later John passed on after tragically surviving his spouse and both children. He did leave behind, however, two successful families with grandchildren. Both his children's spouses, Karen Lacey and David Turner, spoke fondly of Bea and John and appreciated the chance to help create this memoir that would let their children appreciate the legacy of their grandparents.

The passing of John I. Lacey reminds us again of both how our science is built on conceptual and methodological developments and how these developments become such a part of scientific training and of the empirical corpus that their origins are forgotten. John (and Beatrice) established instruments, statistical techniques, integrative alliances, and basic concepts that can be identified today in the fields of psychophysiology, behavioral medicine, neuroscience, and psychology. The work truly turned us toward understanding the two-way communication between body and brain. This communication is now being emphasized in areas as seemingly discrepant as immunological theories of the cause of heart disease and the experience of different emotional qualities. We have lost one of our key integrative scientists, but his legacy continues to enrich us.

REFERENCES

American Psychologist. 1977. Distinguished scientific contribution awards for 1976. *Am. Psychol.* 32:54-59.

American Psychologist. 1985. American Psychological Foundation awards for 1985. *Am. Psychol.* 41:409-411.

Coles, M. G. H., and C. C. Duncan-Johnson. 1975. Cardiac activity and information processing: The effects of stimulus significance, and detection and response requirements. *J. Exp. Psychol. Human* 1(4):418-428.

Coles, M. G. H., J. R. Jennings, and J. A. Stern. 1984. *Psychophysiological Perspectives: Festschrift for Beatrice and John Lacey.* New York: Van Nostrand Reinhold.

Dworkin, B. R., R. J. Filewich, N. E. Miller, N. Craigmyle, and T. G. Pickering. 1979. Baroreceptor activation reduces reactivity to noxious stimulation: Implications for hypertension. *Science* 205:1299-1301.

Graham, F. K., and R. K. Clifton. 1966. Heart-rate change as a component of the orienting response. *Psychol. Bull.* 65:305-320.

Jennings, J. R., and S. W. Hall Jr. 1980. Recall, recognition, and rate: Memory and the heart. *Psychophysiology* 17:37-47.

Jennings J. R., T. W. Kamarck, S. A. Everson-Rose, G. A. Kaplan, S. B. Manuck, and J. T. Salonen. 2004. Exaggerated blood pressure responses during mental stress are prospectively related to enhanced carotid atherosclerosis in middle-aged Finnish men. *Circulation* 110(15):2198-2203.

Lacey, B. C., and J. I. Lacey. 1978. Two-way communication between the heart and the brain. Significance of time within the cardiac cycle. Am. Psychol. *33:99-113.*

Lacey, B. C., and J. I. Lacey. 1980. Cognitive modulation in time-dependent primary bradycardia. *Psychophysiology* 17:209-221.

Lacey, J. I., and B. C. Lacey. 1962. The law of initial value in the longitudinal study of autonomic constitution: Reproducibility of autonomic responses and response patterns over a four-year interval. *Ann. N. Y. Acad. Sci.* 98(4):1257-1290.

Obrist, P. A. 1976. The cardiovascular-behavioral interaction: As it appears today. *Psychophysiology* 13:95-107.

Schneiderman, N., G. Ironson, and S. D. Siegel. 2005. Stress and health: Psychological, behavioral, and biological determinants. Annu. Rev. Clin. Psychol. *1:607-628.*

Sokolov, E. N. 1958. *Perception and the Conditioned Reflex*. Oxford, U.K.: Publishing House, Moscow University.

Stern, J. A. 1971. Award presentation to J. I. and B. C. Lacey. *Psychophysiology* 8:241-242.

SELECTED BIBLIOGRAPHY

1939

With K. M. Dallenbach. Minor studies from the Psychological Laboratory of Cornell University. LXXXVIII. Acquisition by children of the cause-effect relationship. *Am. J. Psychol.* 52:103-110.

1941

Changes in cardiac and respiratory activity in states of frustration. *Psychol. Bull.* 38:581-582.

With B. C. Lacey and K. M. Dallenbach. Areal and temporal variations in pain sensitivity. *Am. J. Psychol.* 54:413-417.

1947

Sex differences in somatic reactions to stress. *Am. Psychol. 2:*343.

1948

Individual differences in somatic response patterns. *Am. Psychol.* 3:254-255.

1949

Consistency of patterns of somatic response to stress. *Am. Psychol.* 4:232-233.

1950

Individual differences in somatic response patterns. *J. Comp. Physiol. Psychol.* 43:338-350.

1952

With D. E. Bateman and R. Van Lehn. Autonomic response specificity and Rorschach color responses. *Psychosom. Med.* 14:256-260.

With R. Van Lehn. Differential emphasis in somatic response to stress. *Psychosom. Med.* 14:71-81.

1953

With D. E. Bateman and R. Van Lehn. Autonomic response specificity: An experimental study. *Psychosom. Med.* 15:71-82.

1954

With R. L. Smith. Conditioning and generalization of unconscious anxiety. *Science* 120:1045-1052.

1955

With R. L. Smith and A. Green. Use of conditioned autonomic responses in the study of anxiety. *Psychosom. Med.* 17:208-217.

Conditioned autonomic responses in the experimental study of anxiety. *Acta Psychol.* 11:137-138.

1958

With B. C. Lacey. The relationship of resting autonomic activity to motor impulsivity. *Res. Publ. Assoc. Res. N.* 36:144-209.

With B. C. Lacey. Verification and extension of the principle of autonomic response-stereotypy. *Am. J. Psychol.* 71:50-73.

1959

Psychophysiological approaches to the evaluation of psychotherapeutic process and outcome. In *Research in Psychotherapy,* eds. E. A. Rubinstein and M. B. Parloff, pp. 160-208. Washington, D.C.: American Psychological Association.

1962

With B. C. Lacey. The law of initial value in the longitudinal study of autonomic constitution: Reproducibility of autonomic responses and response patterns over a four-year interval. *Ann. N. Y. Acad. Sci.* 98(4):1257-1290, 1322-1326.

1969

Proceedings of the seventy-seventh annual meeting of the American Psychological Association, August 31-September 4, 1969, Washington, D.C. *Am. Psychol.* 24(12):1119-1172.

1974

With B. C. Lacey. On heart rate responses and behavior: A reply to Elliott. *J. Pers. Soc. Psychol.* 30(1):1-18.

1976

Psychophysiology of the autonomic nervous system. *Cat. Sel. Doc. Psychol.* 69:2.

1977

With B. C. Lacey. Change in heart period: A function of sensorimotor event timing within the cardiac cycle. *Physiol. Psychol.* 5(3):83-93.

1978

With B. C. Lacey. Two-way communication between the heart and the brain: Significance of time within the cardiac cycle. *Am. Psychol.* 33(2):99-113.

1979

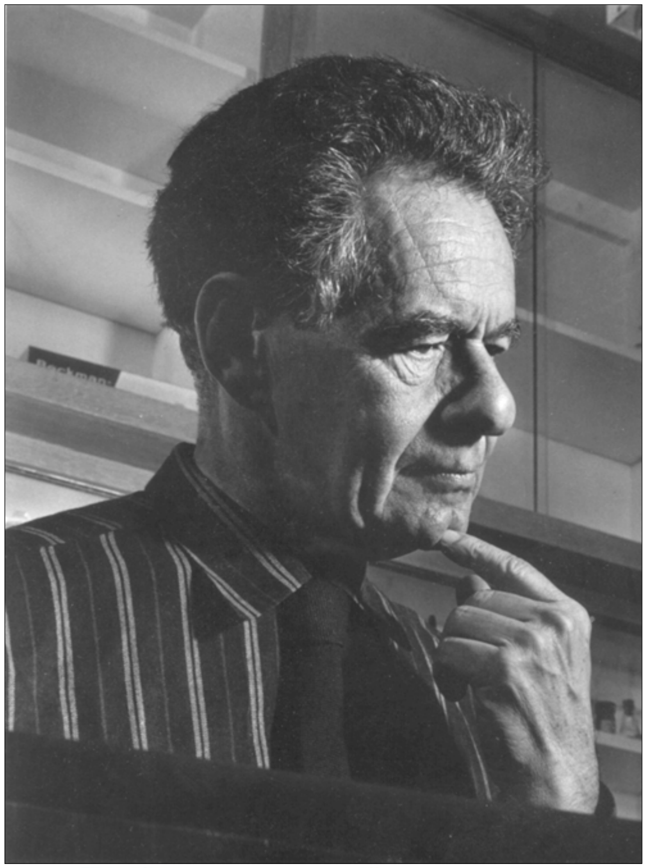
Somatopsychic effects of interoception. In *Research in the Psychobiology of Human Behavior,* eds. E. Meyer III and J. V. Brady, pp. 59-73. Baltimore: Johns Hopkins University Press.

1980

With B. C. Lacey. Cognitive modulation of time-dependent primary bradycardia. *Psychophysiology* 317:209-221.

1985

The visceral systems in psychology. In *A Century of Psychology as Science*, eds. S. Koch and D. E. Leary, pp. 721-736. Washington, D.C.: American Psychological Association.



Fritz Lipmann

FRITZ ALBERT LIPMANN

June 12, 1899–July 24, 1986

BY WILLIAM P. JENCKS AND
RICHARD V. WOLFENDEN

Fritz Lipmann was largely responsible for identifying and characterizing the connection between metabolism and the energetics of living systems that makes life possible.

EARLY LIFE AND EDUCATION

Lipmann was born at the end of the nineteenth century, on 12 June 1899, in Königsberg, which was then the capital of East Prussia. Königsberg was close to the Russian border, and the German government provided more support to the university than might have been expected for a city of its size. Königsberg later became part of the USSR and was renamed Kaliningrad. It was one of the Hanseatic cities and contained a harbour that was connected by a sound to the Baltic Sea. The Lipmann family regularly spent the summer there, close to the seashore. The taxis were droschkes, horse-drawn carriages. Lipmann remembered a visit of the Kaiser, who was driven through the town in a carriage with four horses and a coachman with a plumed helmet.

This memoir originally appeared in *Biographical Memoirs of the Royal Society* 46(2000):335-344 and is reprinted with permission.

Lipmann attended the Gymnasium in Königsberg, where he studied, among other subjects, both Latin and Greek—he preferred Latin. He was not an outstanding student at the Gymnasium, nor later at the university. His father was a lawyer, who told his son that he "was not enough of a crook to be an outstanding lawyer."

An uncle, who was one of Lipmann's heroes, died young from a ruptured appendix. This was one of the experiences that led him to study medicine in Munich in 1917. In 1918, he was called up into the army and assigned to the medical service. He had a short experience with the study of military medicine, close to the front lines, before the war ended in 1919. When he returned to Königsberg, he encountered the ravages of the influenza epidemic. In spite of months of contact with patients, he did not become infected.

After the war, in 1919, he joined his brother Heinz in Munich and studied medicine for a semester. His brother was interested in literature, the theatre and poetry. Fritz spent much of his time with the very active community of artists and writers at that time in Schwabing, the Greenwich Village of Germany. He was also close to Friedel Sebba, a painter who also had a strong interest in the theatre. During that period, he never ventured scientifically outside the confines of medicine, and later regretted not having gone to hear the great chemist Wilstätter.

His parents and friends suggested that he study pathology, to prepare for a career in the practice of medicine. He spent three months dissecting cadavers and examining slides. This led him to choose a different direction for his career. He enrolled in a remarkable three-month course in biochemistry that was taught by Peter Rona, who had worked with Leonor Michaelis. Rona taught three Nobel prizewinners: E.B. (later Sir Ernst) Chain, Hans (later Sir Hans) Krebs and Lipmann. This was an unusual step for a physician at that time, when biochemistry was considered a mere adjunct to physiology, but Lipmann was an unusual physician. He characterized this course as a kind of "biochemical marathon."

In 1923, inflation was raging in Germany, and the political situation was deteriorating. He accepted a fellowship to visit the pharmacology laboratory of Laqueur in Amsterdam. During this crucial four month period, he decided that he wanted to be a biochemist, and that it was essential to learn chemistry. Later, in 1924, he published a paper with Rona on colloid chemistry (1924), which was accepted as his thesis for the MD degree from the University of Berlin, and two papers with J. Planelles on the effects of injection of glucose, glycogen and starch on the levels of blood sugar in rabbits (1924[2], 1925).

To obtain a clearer understanding of chemistry, Lipmann attended a lecture course in chemistry given by Hans Meerwein, a leading organic chemist at the Kaiser-Wilhelm Institute for Biology in Berlin. This stimulated him to study chemistry with Meerwein, and he was awarded the doctorate three years later, in 1927.

However, his main interest was in intermediary metabolism and biochemistry so that, with support from his father, he spent three years in the laboratory of Otto Meyerhof in one of the Kaiser-Wilhelm institutes (now the Max-Planck institutes) in the Dahlem neighborhood of Berlin. The late 1920s were a remarkable time in Dahlem. Other investigators in Meyerhof's laboratory included Karl Lohmann, who discovered Adenosine Triphosphate (ATP), and Karl Meyer, Severo Ochoa , Dean Burk and David Nachmansohn. Erwin Negelein, Hans Gaffron, Walter Christian and Hans Krebs worked in the laboratory of Otto Warburg, who was on the top floor and seldom descended into the lower regions. Nevertheless, his influence permeated the institute. There was constant contact with Neuberg's biochemistry institute and with Otto Hahn and Liese Meitner in the chemistry institute where they discovered nuclear fission.

Hill and Meyerhof had demonstrated a quantitative relationship between muscle contraction and lactic acid formation from glycolysis. Lipmann showed that creatine phosphate was cleaved during muscle contraction, although he did not establish its role conclusively (1927). Meyerhof, in turn, had been greatly influenced by Otto Warburg. Lipmann never worked directly with Warburg, but was certainly influenced by him. François Chapeville had lunch with Warburg at a meeting in Paris and mentioned that he had worked with Lipmann. Warburg replied, "Ah, then you are my grandson." Lipmann became interested in the metabolic effects of the fluoride ion, which was known to inhibit muscle contraction. He confirmed that fluoride inhibits glycolysis and reacts with methaemoglobin to form a fluoromethaemoglobin. Fluoride also inhibits liver esterase (1930[1], 1929[2]). Three papers describing this work and the work in Meyerhof's laboratory resulted in the award of the PhD degree in 1929, under the sponsorship of Carl Neuberg (1929[2], 1930[2], 1941).

In the same year he met Freda Hall, whom he later married. Freda, or Elfried, was the daughter of Gertrud Hall. She was born in Defiance, Ohio, USA, on 19 December 1906 when her parents were in the USA; her father was a German businessman. The family returned to Europe and lived first in West Prussia and then in Berlin. She was talented in drawing and went to art school. Later, she worked as a fashion illustrator for newspapers. Lipmann met her in Berlin at the *Sozialistenball* in 1929.

Lipmann was involved in many activities outside of science. Berlin in the 1920s was a centre of artistic as well as scientific activities, and Lipmann had a strong interest in paintings and in the theatre. His brother had moved to Berlin and was the dramatist with Leopold Jessner, the director of the Staatstheater. Lipmann described the people involved with the theatre as a "clan, closed up and agitated by their problems and intrigues." He noted that they included "unusual characters and beautiful women, often astonishingly intelligent, who were not unsimilar to scientists." He spent considerable time with painter Friedel Sebba, who had made a portrait of Lipmann and his brother Heinz in 1926.

ENERGY

Hill and Meyerhof had shown that there is a close relationship between the production of heat and the formation of lactic acid from glycolysis when muscles contract in the absence of oxygen. That relationship was assumed to be causal, until Einar Lundsgaard, working in Copenhagen, showed that muscle can still contract in the presence of iodoacetate, which blocks glycolysis so that lactate is not formed. When he injected iodoacetate into rats, the rats went into rigor and died after a few minutes. Looking for a source of energy for that contraction, he now found a parallel with the breakdown of creatine phosphate, a relationship that had not been observed in the absence of iodoacetate. Meyerhof's laboratory had shown that the hydrolysis of creatine phosphate releases a large amount of heat, very similar to that released by the hydrolysis of ATP. Lundsgaard realized that when glycolysis was blocked by iodoacetate, the energy released by the hydrolysis of creatine phosphate was used to provide the energy for contraction of the muscle. In Lipmann's last year in Meyerhof's laboratory, Lundsgaard joined the group, and Lipmann became involved in that work. Using platinum electrodes sealed into manometric vessels to stimulate muscle preparations, he was able to demonstrate a quantitative relationship between creatine phosphate breakdown and contraction in intact muscle. It gradually became apparent that the creatine phosphate in muscle represented a reservoir of phosphoryl groups that could feed into ATP. It was found later that the energy released in glycolysis is used to form 1,3-diphosphoglyceric acid, phosphoenolpyruvate and creatine phosphate.

These compounds have very large and favourable Gibbs free energies of hydrolysis, comparable with that of ATP; they are "energy-rich" compounds that can provide the driving force for the synthesis of ATP, which in turn is used to provide the driving force for most of the energy-requiring reactions in all living systems.

Lipmann's two-year fellowship for his work with Meyerhof could not be renewed. After searching with difficulty for his first paying job, he moved in 1930 to Albert Fischer's laboratory in the Kaiser-Wilhelm Institute in Berlin to study cell growth in tissue culture. Here he was in close contact with K. Linderstrom-Lang, E. Lundsgaard, and H.M. Kalckar. The move to Berlin had the added advantage that he could more easily meet Freda Hall. In Fischer's laboratory he developed a technique to measure the growth of cells in tissue culture by the manometric measurement of oxygen uptake by fibroblasts in Warburg vessels. The accumulation of carbon dioxide was prevented by the presence of potassium hydroxide in a side vessel. He was interested in the large amount of glycolysis that occurs in normal embryonic fibroblasts in the presence of air, which he found to be similar to that in malignant cells (1929[2]).

At the end of the first year, Fischer was invited to set up a new laboratory in Copenhagen, funded by the Rockefeller and Carlsberg Foundations. This was fortunate, because Hitler's influence in Germany was increasing rapidly and would soon result in the Holocaust. During the following year, Lipmann took advantage of a Rockefeller Foundation Fellowship to visit P.A. Levene's laboratory at the Rockefeller Institute for Medical Research. Before he left for New York, he and Freda Hall were married on 23 June 1931.

Lipmann asked Levene if he could work on the phosphate link in phosphoproteins because he thought that there might be an energy-rich linkage to nitrogen as in creatine phosphate. Instead, he encountered O-esterified phosphate in vitellic acid, a protein purified from egg yolk. From this material, of which it constitutes roughly 10%, he was able to isolate pure serine phosphate, which, unlike other phosphoryl compounds that were then known, was quite stable to hydrolysis in strong acid. The common occurrence of serine-bound phosphate in tissue proteins, especially from brain, was to remain mysterious for many years, until E.R. Sutherland's much later discovery that cyclic adenosine monophosphate (AMP) acts as a "second messenger" for many hormones by stimulating enzymes that phosphorylate specific receptors. Lipmann later expressed some regret at having stumbled on this gold mine unwittingly, and having passed it by.

In the summer of 1932 he joined the many other scientists who traveled by boat to the Marine Biological Laboratory at Woods Hole on Cape Cod in Massachusetts, and worked in the rooms of Leonor Michaelis. There he made friends with Linderstrom-Lang before returning to the new institute in Copenhagen.

In Copenhagen, Lipmann began by working on the Pasteur effect, the first regulatory reaction in biochemistry to attract attention. Pasteur had shown that yeast cells, which can grow in the presence or absence of air, suppress the anaerobic production of alcohol when air is present. The same effect is observed in fibroblasts, muscle and brain, resulting in much more effective production of useful energy from glucose. He tried in vain to observe a direct effect of oxygen or of redox indicators on the glycolytic enzymes. Many years later it became apparent that the ATP:ADP(adenosine diphosphate) ratio, rather than oxygen itself, is the agent that regulates the activity of some glycolytic enzymes.

A new direction of his research developed in 1937 as he began to investigate the oxidation of pyruvate by a strain of *Lactobacillus delbrueckii.* Preparations from this organism were found to contain two cofactors: thiamine pyrophosphate and flavin adenine dinucleotide. A more important observation was that the reaction proceeded only in the presence of inorganic phosphate. Unlike the glycolytic enzymes of yeast, the components of this system resisted solublization, because its mitochondrial nature was unknown. However, when isotopically labeled phosphate and adenylic acid were added to this preparation, the oxidation of pyruvate led to the production of ATP from ADP and phosphate. This is now known as oxidative phosphorylation, the primary source of readily usable energy in aerobic organisms. This work was performed while Lipmann was still in Copenhagen, and was presented in 1939 at the 7th Cold Spring Harbor Symposium on Long Island, New York.

This work was conducted under the shadow of the rapid growth of fascism and anti-Semitism in Germany; Lipmann's Danish friends warned him of the danger of remaining in Denmark. For advice, he contacted Dean Burk, a colleague from Meyerhof's laboratory who had visited the Lipmanns in Denmark and shared an interest in the Pasteur effect. Burk was moving to Vincent du Vigneaud's laboratory in the Cornell Medical School at New York Hospital in Manhattan, and had two openings in his laboratory. Lipmann had been strongly recommended to du Vigneaud by Linderstrom-Lang. He invited Lipmann to join the laboratory, and the invitation was accepted. Du Vigneaud was awarded the Nobel Prize later, in 1955, for determining the structures of the peptide hormones oxytocin and vasopressin.

In New York, Lipmann encountered Rollin Hotchkiss, who was working with René Dubos at the adjacent Rockefeller Institute. Hotchkiss had isolated tyrocidin and gramicidin, the first bacterial antibiotics, from *Bacillus brevis;* Lipmann showed that these antibiotics contain several non-protein D-amino acids, by analysing their hydrolysates with D-amino acid oxidase.

THE APPEARANCE OF THE SQUIGGLE

In the spring of 1940, the Lipmanns vacationed in Vermont, where Fritz meditated on the significance of activated phosphate compounds such as ATP and acetyl phosphate. It was known that deuterium-labelled acetate was incorporated into fatty acids, steroids and amino acids, and Lipmann had shown that acetyl phosphate was formed from ATP and acetate in bacterial extracts.

It was at this time that he wrote his famous paper "The metabolic generation and utilization of phosphate bond energy" (1941) for *Advances in Enzymology,* introducing the squiggle (~P) for "energy-rich phosphate," which provides the driving force for many biochemical reactions and processes. These include physical processes such as the contraction of muscles, the transport of ions and other molecules across membranes, and chemical reactions for the biosynthesis of proteins, nucleic acids and other large molecules.

In spite of this impressive record of accomplishments, it was difficult for Lipmann to obtain a stable position in research or teaching. He attributed some of this difficulty to a lack of experience in preparing lectures. A particularly harrowing experience took place at a widely attended symposium at Madison, Wisconsin, in 1940 on intermediary metabolism. He had been asked to give a talk on the Pasteur effect, in which by then he had little interest. The lecture he had prepared was much too long, and he had time to deliver only half of what he had prepared before Carl Neuberg, the chairman of the session, was forced to stop him. The audience was uncomfortable, and word of this event contributed to his later difficulty in finding a decent position. Lipmann was finally awarded a Ciba Fellowship in the Department of Surgery at the Massachusetts General Hospital in Boston, which was chaired by Edward Churchill. His research was supported by a grant obtained by Oliver Cope, a surgeon who was interested in endocrinology and basic research. It is a credit to the flexibility of the medical and scientific community in the USA that it was possible to make such an appointment in a department of surgery.

The importance of his paper on activated phosphate compounds as a source of readily available energy was recognized quickly, and soon a number of biochemists came to work in his laboratory. He received financial support for his research from the Commonwealth Fund and was able to support a remarkably capable technician, Constance Tuttle, who worked with him for many years.

He isolated an enzyme from pigeon liver that permitted colorimetric determination of the acetylation of sulphonamides and other aromatic amides in the presence of ATP and acetate. He expected that ATP would react with acetate to give acetyl phosphate, which would then acetylate the sulphonamide. Indeed, the enzyme did catalyse the acetylation in the presence of ATP and acetate. However, acetyl phosphate, the presumed intermediate in the reaction, was inactive as an acetylating agent.

The crude liver extract that was active for acetylation by ATP and acetate lost its activity rapidly, but the addition of boiled liver extract gave full reactivation. This indicated that the extract contained a heat-stable cofactor that is required for acetylation. Lipmann, N.O. Kaplan and G.D. Novelli purified the liver extract and obtained a compound that contained a thiol group and adenylic acid.

Roger Williams, the discoverer of pantothenic acid, and Beverly Guinard hydrolysed the cofactor and showed that it contained â-alanine and adenylic acid in equivalent amounts. Lipmann's group then used alkaline phosphatase and an enzyme obtained from an extract of liver to cleave the compound and obtained pantothenic acid. At approximately the same time, D. Nachmansohn and John, and W.S. Feldberg and Mann, found a cofactor in an extract from brain that activated the acetylation of choline by ATP and acetate. Lipmann's group found the same activity in a dialysate of brain extracts and named it coenzyme A (now abbreviated to CoA); the A stood for the activation of acetate. It contained a thiol group, but the function of this group was not immediately apparent. E.E. Snell and his co-workers identified the compound as part of a cofactor required by *Lactobacillus bulgaricus* and showed that it was a peptide containing pantothenic acid and mercaptoethanol. F. Lynen and P. Reichart isolated acetyl-CoA and showed that the acetyl group is a thiol ester, and "energy-rich" compound containing a reactive acetyl group. J. Baddiley and his co-workers synthesized pantetheine 4'-phosphate and showed that it was converted to CoA by reaction with ATP. Kaplan showed that the third phosphate group of CoA is on the 3' position of the ribose with a phosphatase from rye grass that is specific for cleavage of a phosphate ester at that position. This confirmed the complete structure of CoA.

Further investigation in Lipmann's and other laboratories soon showed that acetyl-CoA and other thiol esters of CoA function as acyl donors in many synthetic and metabolic pathways. In the citric acid, or Krebs, cycle, acetyl-CoA adds an acetyl group to oxaloacetate to give citrate. The large sulfur atom of CoA has only a weak overlap with the carbonyl group, so that the acidity of the acetyl moiety of acetyl-CoA is similar to that of a ketone. The acetyl group can therefore lose a proton easily to give the enolate, which then adds to the ketonic carbonyl group of oxaloacetate to give citrate and free CoA. Although citrate itself is a symmetrical molecule, the reaction was shown to be stereospecific by labelling the acetyl-CoA.

In 1953 Lipmann summarized this and other important work on the structure and function of CoA and its acyl derivatives in a comprehensive review (1953) that included descriptions of the role of acyl-CoA thioesters in the synthesis of fatty acids and steroids, as well as the acylation of arylamines and of acetyl-CoA itself, in acetoacetate synthesis. David Novelli, Nathan Kaplan and Mary Ellen Jones played especially important roles in the laboratory, as senior investigators who provided advice and help to others.

In the same year, Lipmann became a Nobel laureate in Medicine and Physiology, with Hans Krebs. Acetyl-CoA provided the crucial link in the citric acid cycle that had been conceived by Krebs, foreshadowing a similar role for carbamyl phosphate in the urea cycle that Lipmann was to discover.

At the time that he received this recognition, Lipmann's work on CoA had reached what he considered the 'mopping up' stage. His work had been supported generously for some time by the National Institutes of Health, and the National Science Foundation offered him an additional five-year grant that allowed more latitude for exploration. He felt that there was broad scope for progress in the recognition that ~P was acting as a kind of energy quantum in biological systems, which could be used in many other areas of metabolic function and biosynthesis.

CARBAMATE AND SULPHATE ACTIVATION, AND PROTEIN SYNTHESIS

He next turned his attention to carbamyl transfer, by which urea derivatives were formed, presumably from a substituted carbamic acid. Microbial extracts had been shown to catalyse the breakdown of citrulline in the presence of phosphate, leading to the inference that carbamyl phosphate might serve as a precursor in the synthesis of citrulline. Seeing a possible resemblance between acetyl phosphate and carbamyl phosphate, he set out with Mary Ellen Jones to look for carbamyl phosphate in extracts of *Streptococcus faecalis* that catalysed the cleavage of citrulline in the presence of phosphate. They obtained a product that was more stable to acid than acetyl phosphate, whereas carbamyl phosphate had been expected to be less stable. However, Leonard Spector, then associated with Paul Zamecnik in a neighbouring laboratory, was able to perform the first effective synthesis of this material by analogy with the synthesis of acetyl phosphate by the reaction of inorganic phosphate with ketene. Carbamyl phosphate, generated by the reaction of potassium cyanate with inorganic phosphate, was thus identified as the material that Jones had isolated, and was then tested for activity in microbial and human preparations. In a short communication to the *Journal of the American Chemical Society,* Jones, Spector and Lipmann reported (1955) that ATP could be generated by the reaction of carbamyl phosphate with ADP, and was converted quantitatively to citrulline in the presence of bacterial extracts. They also described a somewhat slower reaction with aspartate to generate carbamyl aspartate, the first step in the biosynthesis of uridine and cytidine derivatives.

The combination of cyanate with inorganic phosphate attracted Lipmann's attention as one of several reactions by which the first biological molecules could have been formed, leading to the spontaneous formation of a form of ~P, interconvertible with the anhydride bonds in ATP. However, he regarded polymeric anhydrides of phosphoric acid as more likely to have served as the primary phosphoryl donors in early organisms. Lipmann once remarked that, in his experience, people's interest in the origin of life tended to increase with advancing age; he maintained a detached air of levity in discussing an area so inaccessible to experiment.

Sulphate esters occur widely in nature, particularly in the ground substance of cartilage, chondroitin sulphate, and in cerebrosides. The activation of sulphate was already known from R.H. DeMeio's work (DeMeio et al., 1955) to require ATP, but no anhydride of phosphoric and sulphuric acids had been described in the chemical literature. Helmuth Hilz observed formation of a phosphate-sulphate compound with release of pyrophosphate from ATP. Further experiments by Philip Robbins showed that the actual donor of sulphate in biosynthesis is not, as might be expected, a simple anhydride of sulphate and AMP: that compound is formed enzymically in preparations from rat liver, but was found to be inactive as a sulphate donor. Instead, a second molecule of ATP is required to generate adenosine-3'-phosphate-5'-phosphosulphate, which serves as the general donor of sulphate in enzyme reactions. The first reaction, in which a sulphate-phosphate anhydride bond is formed by the attack of sulphate on ATP, was shown to be quite unfavourable energetically. The second ATP-consuming reaction, however, is energetically favourable, so that the overall energy balance is nearly zero for the formation of 'active sulphate'. These two reactions are catalysed by distinct enzymes.

Protein synthesis presented a much greater challenge because of its complexity, but Lipmann had predicted in 1951 that it would proceed by activation of the carboxyl groups of amino acids. Mahlon Hoagland, working in Zamecnik's laboratory, demonstrated the existence of amino-acid-activating enzymes in rat liver, and showed that the action of these enzymes resulted in cleavage of ATP to pyrophosphate, which was consistent with formation of amino-acid-AMP anhydrides similar to the anhydrides that had been shown by Paul Berg to be formed during acetate activation. Lipmann's laboratory became involved when Earl Davie and William Koningsberger isolated the tryptophan-activating enzyme and showed that it was specific for that amino acid.

From the faithfulness with which amino acid sequences were known to be expressed, it was evident that a major part of the puzzle was missing. Francis Crick suggested that the principle of base-pairing would be found to be involved in protein synthesis, in which each amino acid might be equipped with a polynucleotide "adapter" carrying an anti-codon for that amino acid. Hoagland and Holley soon confirmed, independently, that a nucleic acid was indeed part of each amino-acid-activating system. Expecting at first that a phosphate-amino-acid anhydride might be involved, the Lipmann group showed by treatment with ribonuclease that an adenosine derivative containing the amino acid was released, indicating that it must be esterified at the 2' or 3'-terminal adenosine of tRNA. Efforts to determine the site of attachment revealed that the amino acid isomerizes between these positions with a half-time of a few milliseconds under physiological conditions. The generality of the adaptor hypothesis gained important support from experiments by von Ehrenstein in the Lipmann laboratory showing that tRNA from bacteria could be used in haemoglobin synthesis in reticulocytes, and from François Chapeville's demonstration that alanyl-tRNA, prepared from cysteinyl-tRNA by treatment with Raney nickel, incorporated alanine in place of cysteine in systems directed by poly(UG), which codes for cysteine.

Between 1961 and 1964, Daniel Nathans and Jorge Allende separated soluble factors from the supernatant fluid of disrupted *E. coli* that brought about the incorporation of many amino acids from tRNA into protein simultaneously in the presence of mRNA and ribosomes, using GTP as an energy source. The GTP requirement of these elongation factors seemed difficult to understand in view of the energy available in the amino acid ester linkage to tRNA. Through the use of puromycin, an antibiotic that resembles the terminal adenosine residue of tRNA and can participate in some of its reactions, Anne-Louise Haenni and Jean Lucas-Lennard showed that GTP hydrolysis leads to translocation of peptidyl-tRNA from an acceptor site to a donor site, where it stands ready to transfer its peptide to the next incoming amino acyl-tRNA. These experiments defined the chemical steps in protein synthesis as they are understood today. The role of GTP in mediating mechanical events in microtubules and other cellular processes was foreshadowed by these experiments on peptide elongation.

Lipmann maintained an active laboratory during his later years at the Rockefeller University. Providing a counterpoint to the machinery for protein synthesis, he showed that cyclic peptide antibiotics such as gramicidin and tyrocidine are synthesized by the sequential addition of amino acids on polyenzymes. He returned to another of his early interests in studying the phosphorylation and sulphation of tyrosine residues in proteins as a result of transformation, and showed that a phosphotyrosine residue produced by the action of an oncogene-encoded tyrosine kinase was energy-rich.

In an autobiographical collection of essays and scientific papers, entitled *Wanderings of a biochemist* (1971), Lipmann describes his early years of learning and wandering, and the process of following one's instinct without knowing exactly where it will lead. His instinct was remarkable, and few parts of biochemistry were not advanced by the results of his work. Lipmann died on 24 July 1986, at the age of eighty-seven, not long after having learned that his latest research grant application had been successful.

REFERENCE

DeMeio. R. H., M. Wiezerkaniuk, and J. Schreibman. 1955. *J. Biol. Chem.* 213:439.

SELECTED BIBLIOGRAPHY

1924

With P. Rona. Über die Wirkung der Verschiebung der Wasserstoffionenkonzentration auf den Flockungsvorgang beim positiven und negativen Eisenhydroxydsol. *Biochem. Z.* 147:163.

With J. Planelles. Blutzuckerkurven nach intravenöser Einspritzung von á-, â-, und á-â-Glucose beim Kaninchen. *Biochem. Z.* 151:98.

1925

With J. Planelles. Einfluss von intravenöser Glykogen- und Stärkeeinspritzung auf den Blutzucker beim Kaninchen. *Biochem. Z.* 163:406.

1927

Kann Milchsäure anaerob aus der Muskulatur verschwinden? *Biochem. Z.* 191:442.

1929

Weitere Versuche über den Mechanismus der Fluoridhemmung und die Dissoziationskurve des Fluor-Methämoglobins. *Biochem. Z.* 206:171.

Über den Mechanismus der Fluoridhemmung. *Verh. dt. Pharmakol. Ges.* 70

1930

Über den Tätigkeitsstoffwechsel des fluoridvergifteten Muskels. *Biochem. Z.* 227:110.

With K. Lohmann. Über die- Umwandlung der Harden-Youngschen Hexosediphosphorsäure und die Bildung von Kohlenhydratphosporsäureestern in Froschmuskelextract. *Biochem. Z.* 222:389.

1941

Metabolic generation and utilization of phosphate bond energy. *Adv. Enzymol.* 1:99.

1953

On chemistry and function of coenzyme A. *Bacteriol. Rev.* 17:1.

1955

With M. E. Jones and L. Spector. Carbamyl phosphate, the carbamyl donor in enzymatic citrulline synthesis. *J. Am. Chem. Soc.* 77:819.

1971

Wanderings of a Biochemist. New York: Wiley-Interscience.

FRANCIS DANIELS MOORE

August 17, 1913–November 24, 2001

BY JUDAH FOLKMAN

FRANCIS DANIELS MOORE, one of the world's great surgeon-scientists, died on November 24, 2001, at the age of 88. He was born in Evanston, Illinois, and graduated from the North Shore Country Day School. He graduated from Harvard College with the A.B. degree cum laude in anthropology. He entered Harvard Medical School and received the M.D. degree cum laude. Soon after he had completed his years of surgical training at the Massachusetts General Hospital in Boston and had become its chief resident in surgery in 1942, he began his pioneering work on the metabolic response to surgery. He became a postgraduate National Research Council fellow in isotope physics and its applications (under Joseph C. Aub). This new field would become his life's work. He would continue to pursue it at the Peter Bent Brigham Hospital, where he arrived in 1948 to become its Surgeon-in-Chief. At the age of 34 he was Harvard Medical School's Moseley Professor of Surgery, the youngest chairman of surgery in Harvard's history.

Francis Moore's studies, carried out between the physiology laboratory and the patient's bedside, culminated in two classic books: *Metabolic Response to Surgery* with M. Ball (1949) and *Metabolic Care of the Surgical Patient* (1959). These masterpieces changed the thinking of surgeons throughout the world and reduced suffering and mortality of their patients. Before Moore, surgeons concentrated on improving their craft to effect the local anatomic changes necessary to treat disease, but they remained perplexed by the body's physiologic response to the trauma of surgery. Surgeons of the day did not understand how to optimize the physiological status of their patients before surgery. A perfect anatomical operation could be followed by disastrous complications or death from a low level of circulating sodium chloride or magnesium, or a high level of potassium chloride, or an undetected loss of plasma or water.

Moore developed methods to quantify the concentration of ions in the blood by flame photometry. He was among the first clinicians to inject radioisotopes into laboratory animals, and then into volunteers and patients in order to measure total body water and body composition. These studies paved the way for the development of nuclear medicine. His publications codified the syndromes generated by altered physiologic states of electrolyte imbalance, or endocrine abnormalities, or body fluid deficits. By these studies Moore armed his fellow physicians and surgeons so that they could recognize potential trouble before or after surgery. Thousands of lives were saved every year. He introduced new methods to the hospital laboratory from his research laboratory so that these syndromes of metabolic imbalance could be quantified in all hospitals. Francis Moore's contributions to the improvement of the metabolic care of surgical patients, especially those with severe burns, provided a scientific basis for modern intensive care units and rank with the great leaps in the history of progress in surgery brought about by the introduction of anesthesia, the development of aseptic technique, and the transfusion of blood.

Francis Moore's early unshakable confidence that improvements in medicine could come from animal and human experimentation seems to have been set off by a searing experience on the night of November 28, 1942, when he was a 29-year-old surgical resident working in the emergency ward at the Massachusetts General Hospital. He was one of the few surgical residents left in Boston who had not been drafted to serve in World War II, because of his chronic asthma. That night more than 100 severely burned patients arrived on stretchers from a huge fire at the Cocoanut Grove night club in Boston. The conventional treatment for severe burns at that time was to soak the patient in tannic acid to toughen the skin in order to prevent the inevitable infection that would eventually kill the patient. It was a painful and toxic treatment and required several doctors and nurses for each patient. Oliver Cope, a senior staff surgeon, believed that tannic acid was more harmful than effective and decided to try an experiment. Cope insisted that the burned patients be wrapped in sterile gauze coated with petroleum jelly, because in his laboratory animals, blisters covered with this type of dressing did not appear to become infected. Moore followed Cope's orders. A month later none of the initial survivors at the Massachusetts General Hospital had died, in contrast with a 30 percent mortality among initial survivors treated with tannic acid at the Boston City Hospital. Over the next five years Dr. Moore began clinical research on burns, body fluids, and burn shock.

In 1948 when Dr. Moore arrived at the Peter Bent Brigham Hospital as the new Surgeon-in-Chief, Dr. George Thorn, the hospital's Physician-in-Chief, was just beginning a program that would eventually treat renal failure by dialysis, using an external artificial kidney donated by a Dutch physician, Willem Kolff. (Kidney transplants in dogs were being carried out in the laboratory by two young surgeons, Charles Hufnagel and David Hume). Moore and Thorn began a long and productive collaboration. They put together a team led by David Hume. By 1953, 10 patients with terminal renal failure had received a kidney transplant from a recently deceased individual, but these kidneys soon underwent immune rejection and all patients died within weeks except for one whose kidney worked for five months. Despite these early failures, Moore persevered. In 1953 he persuaded Joseph Murray to take over the kidney transplant program, after Hume was called to active duty in the Navy. Moore had appointed Murray to the surgical staff to develop plastic surgery at the Brigham. By December 1954 Dr. Murray had carried out the world's first successful kidney transplant between identical twins.

However, it would be eight more years before Murray was able to perform the world's first successful transplantation of an organ from an unrelated donor, in April 1962, because the immunosuppressive drug, azathioprine, had become available. As more effective immunosuppressive drugs were developed, kidney transplantation became a safer surgical therapy in major medical centers. In May 1963 Francis Moore was on the cover of *Time* magazine and in 1990 Joe Murray received the Nobel Prize. In his acceptance speech Dr. Murray credited Dr. Moore with providing the leadership, creativity, courage, and unselfishness that led to the success of transplantation. Moore continued to drive progress in organ transplantation, and in 1963 both he and Thomas Starzl, a surgeon at the University of Colorado, performed their first human liver transplants within months of each other in a handful of patients. But there were no long-term survivors. Both men called a temporary halt to the procedure. A year later only Starzl in the United States and Roy Calne in the United Kingdom resumed development of liver transplantation and continued for many years until it eventually became successful. Meanwhile, Dr. Moore had persuaded Dr. Dwight Harken to develop intracardiac surgery at the Brigham, beginning with surgery of the mitral valve, and provided encouragement during the early days of this endeavor.

Francis Moore was one of Harvard Medical School's most inspiring teachers, and its most eloquent public speaker. I entered Harvard Medical School in 1953 and I can still recall every one of his lectures to the medical students. He held Saturday morning "clinics" during which he presented a patient to a packed amphitheatre. We first-year students were concerned that perhaps the patient felt he or she was being exploited as a teaching case in front of all of these students. Dr. Moore immediately reassured us. He told us that after a successful operation, he had asked the patient if she could help him teach a class of Harvard Medical students how to make an accurate diagnosis of the type of illness from which she was recovering. Then he would pretend to whisper to the patient, "Now don't tell them all the answers right away. Let them figure things out." The patient would invariably smile, pleased to be playing the transient role of a member of the Harvard Medical faculty. As Dr. Moore then unfolded the patient's history before us, we entered the patient's life. In fact, we became the patient, almost as if we were in the play *Our Town.* Dr. Moore mesmerized not only the class but the patient as well. In fact, he was so charismatic that patients were elated to be presented by him to students or faculty, and they never forgot the experience. To us students the professor and his patient appeared to be two longtime friends. We learned by Dr. Moore's example that each patient must be accorded the highest respect and treated with the greatest compassion. His message came through to all of us: Take meticulous care of your patient, check on things yourself, and always make sure that the working diagnosis is still working.

He often employed a keen sense of humor to emphasize a teaching point. At a surgical grand rounds he was discussing a patient with gallstones. He had turned the wheelchair with the patient's back to the audience ostensibly so that the patient could see the X rays. It wasn't until the end of the discussion that the audience realized that the patient was Mrs. Moore.

Francis Moore's remarkable showmanship had already surfaced during his college years at Harvard College, where he was president of the *Harvard Lampoon* and of the Hasty Pudding Society. Nicholas Tilney, one of Dr. Moore's surgical residents at the Peter Bent Brigham Hospital and now professor of surgery there, describes Francis Moore's maturing expertise in writing prose and playing music while at Harvard.

> As President of the Harvard Lampoon in his junior year, Franny and his friends became famous (infamous) for purloining the Sacred Codfish from the State House, and as a sequel the following year, for removing the Yale Bulldog from New Haven to Cambridge. Indeed Handsome Dan II appeared on the cover of the March 1934 issue of the Lampoon, appropriately licking the foot of the statue of John Harvard. Franny's musical career burgeoned at the Hasty Pudding Club, to which he was also elected President. In 1934, he and a colleague wrote a famous show entitled "Hades! The Ladies!" After a successful local run, he and his co-author, Alistair Cooke, took the production on a prolonged road trip. Their adventures included tea in the East Room of the White House with Eleanor and Franklin Roosevelt, himself a member of a Hasty Pudding chorus.

Dr. Tilney wrote that during his years as Surgeon-in-Chief, "Franny Moore was a man of many interests." He was invited to numerous visiting professorships throughout his career and he traveled widely. Tilney was "one of three who accompanied Dr. Moore to lecture in China during the early 1980s, five years after the end of the Cultural Revolution. Franny's energy and enthusiasm for new sights, sounds, smells, and for everyone he met were all pervasive. It was like following Charlemagne. Dr. Moore was also an expert sailor who piloted his yawl, *Angelique,* in a Bermuda race and several Halifax races."

For many years Francis Moore was also active in Boston's locally famous Tavern Club. According to Dr. Tilney's brief memorial of Dr. Moore for the Tavern Club,

> Dr. Moore wrote and orchestrated several plays, including "Futures and Sutures," in 1968 and "A Wreath for a Wraith" in 1979. The lyrics for both were by the poet David McCord, who also provided the title *Give and Take* for Franny's 1964 book on the development of organ transplantation. In his final play, "Moonglow" in 1990, he was the writer, director, and with David Pickman the composer. With high-tech props, the moon even exploded. He drove his cast single-mindedly to a thespian triumph, but still felt short-changed that he could not act in the production as well.

After I had come to Boston Children's Hospital in 1967 as Surgeon-in-Chief, Dr. Moore was very helpful to me, offering sage advice about the art of being a department chairman. His surgical residents all rotated through our pediatric surgery service, and we could see their professor's strong influence in their clear thinking about a diagnostic dilemma or a problem at the operating table. Such is the long-lasting impact of a great clinical teacher. It continues on like the ripples from a pebble dropped in a pond. I recall asking Dr. Moore to be a discusser of a paper I was presenting to the American Surgical Association. His discussion was so insightful, and so thought provoking, and he had done so much homework on angiogenesis research that his discussion was better than my paper. I learned that many colleagues had this experience. One said, "If you really want to understand your own work, ask Franny to be your discussor."

Later in his career Dr. Moore became concerned about uneven access to surgical care in the United States. From 1970 to 1975 he chaired a joint committee formed by the American College of Surgeons and the American Surgical Association to do a national study of the distribution, educational needs, and economics of medical and surgical care in our country. This effort led to the 1975 publication of *Study of Surgical Services for the United States,* which provided guidelines for surgical care and for training of surgeons for the next several decades.

Dr. Moore also served on the Board of Regents of the Uniformed Services University of the Health Sciences from 1976 to 1983. He was for many years a consultant to the surgeon general of the U.S. Army, concerning care of the severely wounded. This continued his lifelong scholarly interest in the physiological response to trauma. Earlier in his career, during the Korean War, he had helped to solve the problem of potassium toxicity in wounded soldiers, which turned out to be a result of transfusions with outdated blood. Dr. Moore was also a consultant to the National Aeronautics and Space Administration and to the National Institutes of Health.

Even after Dr. Moore had retired as Surgeon-in-Chief from the Brigham in 1976, he continued to be active on the Brigham staff. Later he moved his office to Harvard Medical School's Countway Library to become a part-time editor of the *New England Journal of Medicine.* He became a consultant in surgical oncology at the Dana-Farber Cancer Institute, thus reflecting his earlier pioneering work (with Richard Wilson) in demonstrating that removal of the ovaries could prevent progression of metastatic breast cancer, a finding that was the forerunner of tamoxifen. He continued to be an active voice at Harvard, as well as in national and international surgery and medicine.

Francis Moore's monumental contributions to the science of surgery brought numerous awards and honorary degrees from around the world. He was elected to the National Academy of Sciences in 1981, and he was a member of the American Philosophical Society. But he would likely be most proud of the honors received by his trainees. Recently, three of his former residents have been so recognized. Dr. Murray Brennan, New York Memorial Hospital, was named president of the American Surgical Association; Dr. Robert Bartlett, University of Michigan, Ann Arbor, received the American Surgical Association's Medallion for Scientific Achievement; and Dr. Steven Rosenberg also received the American Surgical Association's Medallion for Scientific Achievement.

Dr. Moore was married in June 1935 to Laura Benton Bartlett. They raised five children, all highly successful. Their long and happy marriage has been recalled by many Brigham surgical residents who were entertained by the professor and his wife in their home or at hospital Christmas parties. Following Laura's death in 1988 in a tragic automobile accident, Dr. Moore married Katharyn Watson Saltonstall in May 1990. He is survived by five children, seventeen grandchildren, and four great-grandchildren.

In presenting a moving *In Memoriam* to the Harvard Medical School faculty in December 2003, Joseph Murray said of Francis Moore,

> In his autobiography, *A Miracle and a Privilege*, Franny reflects back 60 years to his first year medical student days in anatomy. He nostalgically recalls that anatomy professor, Bobby Green taught us not to say "I am a body, I have a soul," but rather, "I am a soul, I live in a body." In this book he continues, "This places human anatomy where it belongs, as a structure and serves as a dwelling place . . . Injury and disease can so destroy that warm dwelling place that it is no longer habitable and the dweller—energy, mind and soul—had best be permitted to depart."

When he felt his own body was no longer habitable, he decided to end his life. Death by his own hand did not come as a surprise to many of his friends. He had not been well and was having increasing difficulty in following the orders of his physicians. He had never been a good follower—if he could not be in control, he chose to end his life. We were immensely sad, but reluctantly accepted what we understood to be his wishes.

Dr. Francis D. Moore will always be remembered and will be sorely missed by patients throughout the world who were helped by his research, by students who were taught by him, and by his colleagues who were also his friends and to whom he was a magnificent role model.

SELECTED BIBLIOGRAPHY

1944

With O. Cope. A study of capillary permeability in experimental burns and burn shock: Using radioactive dyes in blood and lymph. *J. Clin. Invest.* 23:241.

1946

Determination of total body water and solids with isotopes. *Science* 104:157.

1949

With M. R. Ball. *The Metabolic Response to Surgery.* Springfield, Ill.: Charles C. Thomas.

1952

With I. S. Edelman, J. M. Olney, A. H. James, and L. Brooks. Body composition. Studies in the human being by the dilution principle. *Science* 115:447.

1955

With E. A. Boling, H. B. Ditmore Jr., A. Sicular, J. E. Teterick, A. E. Ellison, S. J. Hoye, and M. R. Ball. Body sodium and potassium. V. The relationship of alkalosis, potassium deficiency and surgical stress to acute hypokalemia in man; experiments and review of the literature. *Metabolism* 4(5):379-402.

1956

With A. G. Jessiman. Carcinoma of the breast; the study and treatment of the patient. *New Engl. J. Med.* 254(20):846.

1958

With N. I. Gold, E. Singleton, and D. A. MacFarlane. Quantitative determination of the urinary cortisol metabolites, tetrahydro F, allo-tetrahydro F and tetrahydro E: Effects of adrenocorticotropin and complex trauma in the human. *J. Clin. Invest.* 37(6):813-823.

1959

Metabolic Care of the Surgical Patient. Philadelphia: W. B. Saunders.

1960

With B. Lown and H. Black. Digitalis, electrolytes and the surgical patient. *Am. J. Cardiol.* 6:309-337.

1962

With L. L. Smith. Refractory hypotension in man—Is this irreversible shock? *New Engl. J. Med.* 267:733-742.

1963

With K. H. Olesen, J. D. McMurrey, H. B. Parker, M. R. Ball, and C. M. Boyden. *The Body Cell Mass and Its Supporting Environment. Body Composition in Health and Disease.* Philadelphia: W. B. Saunders.

1964

New problems for surgery, drugs that act on the cell nucleus affect the surgeon's work on cancer and on transplantation. *Science* 144:388-392.

Give and Take: The Development of Tissue Transplantation. Philadelphia: W. B. Saunders.

1967

With S. I. Woodrow, M. A. Aliapoulious, and R. E. Wilson. *Carcinoma of the Breast.* Boston: Little, Brown.

1968

With J. Lister, D. M. Boyden, M. R. Ball, N. Sullivan, and F. J. Dagher. The skeleton as a feature of body composition: Values predicted by isotope dilution and observed by cadaver dissection in an adult female. *Hum. Biol.* 40:135.

1970

Therapeutic innovation: Ethical boundaries in the initial clinical trials of new drugs and surgical procedures. *CA-Cancer J. Clin.* 20(4):212-227.

1972

Transplant. The Give and Take of Tissue Transplantation. New York: Simon and Schuster.

The normal state and brief history of intravenous nutrition. In *Intravenous Hyperalimentation.* (Conference on Intravenous Hyperalimentation, U.S. Army Institute of Surgical Research, San Antonio, Texas, 1970.) Eds., G. S. M. Cowan and W. L. Scheetz, pp. 17-19. Philadelphia: Lea and Febiger.

1973

Systemic indicators of the low flow state: Biochemistry and metabolism during tissue hypoperfusion. In *The Microcirculation in Clinical Medicine*, ed. R. Wells, pp. 195-212. New York: Academic Press.

1975

Report on the Manpower Subcommittee, Study of Surgical Services for the United States. *Ann Surg.* 182(4):526-530.

1977

With R. J. Nickerson, T. Colton, S. Harvey, R. H. Egdahl, W. B. Babson Jr., W. V. McDermott, and W. G. Austen. National surgical work patterns as a basis for residency training plans. The response of a panel of surgeons. *Arch. Surg.* 112(2):125-147.

Homeostasis: Bodily changes in trauma and surgery. The response to injury in man as the basis for clinical management. In *Davis-Christopher Textbook of Surgery. The Biological Basis of Modern Surgical Practice,* 11th ed., ed. D. C. Sabiston, pp. 27-64. Philadelphia: W. B. Saunders.

1982

Surgical manpower: Past and present reality. Estimates for 2000. *Surg. Clin. N. Am.* 62:579-602.

1983

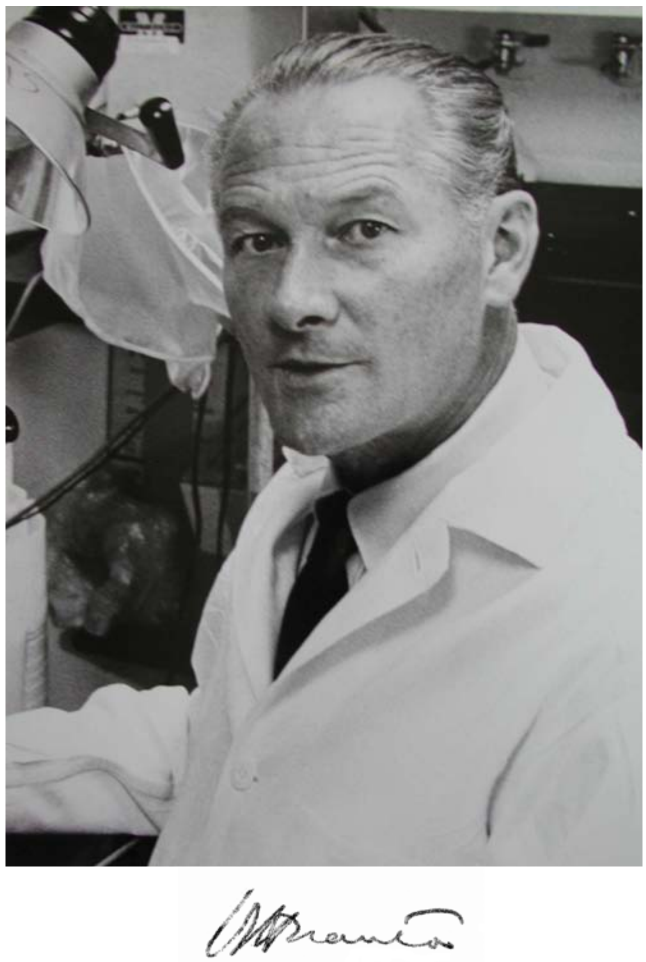
With L. D. Berrizbeitia. Periodicity in protein metabolism: Time patterns of substrate interaction and utilization. *J. Parenter. Enter. Nutr.* 7:398-409.

1985

Surgical streams in the flow of health care financing. The role of surgery in national expenditures. What costs are controllable? *Ann. Surg.* 201:132-141.

1995

A Miracle and a Privilege: Recounting a Half Century of Surgical Advance. Washington, D.C.: Joseph Henry Press.



WALLE J. H. NAUTA

June 8, 1916–March 24, 1994

BY EDWARD G. JONES

WALLE NAUTA WAS ONE of the leading neuroanatomists of his generation and a key figure in neuroscience history as a consequence of his development of a revolutionary technique for tracing connections in the nervous system. It was a technique that dominated the field of neuroanatomy at a time when it was forming one of the seminal influences in the rise of neuroscience as an integrated discipline.

Nauta came from that once large but now almost vanished group of Dutchmen that for centuries had made its home in the East Indies. He was born on June 8, 1916, in Medan, Sumatra, a thriving commercial center on the Malacca Strait. His father, from Leiden, had gone there as a missionary of the Dutch Reformed Church but had soon turned to issues of public health, education, and better governance for the Indonesian people and it was into this milieu that Nauta was born. As was typical, he was sent to Holland for his later elementary schooling. The whole family returned to Holland in the late 1930s, thus escaping internment during the Japanese occupation of the East Indies during World War II. Reportedly, Nauta was never a particularly engaged student until he entered medical school at Leiden University, where he found his métier.

By 1937 he had completed the preclinical part of his medical training at Leiden and before proceeding to the clinical years he served for two years as a student assistant in the Anatomy Department. It was there that his interest in neuroanatomy began. He was commencing his clinical rotations in 1940 when the German army occupied the Netherlands. On completing his clinical studies in 1942, he was about to return to an instructorship in anatomy when Leiden University was closed by the occupying authorities on account of its being a hotbed of subversion. He therefore received his qualifying medical degree from the State University of Utrecht and began to work in the Pharmacology Department there. It was at this time that he married Ellie Plaat, another Dutch Indonesian who had been stranded in the Netherlands without resources by the outbreak of war. In order to support herself she had become a nurse. After graduation and until 1946 Walle Nauta was both a practicing physician and a researcher at the University of Utrecht. The war years were periods of great hardship and want for all Dutch citizens. As a physician, Nauta was fortunate in having a permit that enabled him to pass beyond checkpoints and thus treat farmers in the countryside in return for food. But it was still a period of great privation. At one point in the lab, where he had begun studying the effects of hypothalamic lesions on sleep in rats, supplies ran out and the rats had to be fed with milk from Mrs. Nauta, who was at that time nursing their first child. His doctoral thesis was completed and the degree awarded in 1945.

The Nautas, like all the Dutch people, suffered not only hunger but also indignity and sometimes worse at the hands of the occupying forces. Despite these experiences, Walle Nauta never stereotyped individual Germans, and it was typical of him that he and his family at the end of the war protected the local German administrator—who had turned a blind eye to the Nautas' harboring of a Jewish girl for most of the war—until his bona fides could be established and safe passage back to Germany secured. For protecting that Jewish girl the names of Ellie and Walle Nauta are inscribed on the Wall of the Just in Jerusalem. In a reversal of fortunes the Nautas were to send care packages to the German administrator after his return to his homeland.

At the end of the war Nauta, now again at Leiden and in the Anatomy Department, contemplated a career in ophthalmology but felt that the circumstances prevailing at that time precluded this, and so he sought a permanent position in anatomy. He claimed that as a student assistant at Leiden he had been attracted to the hypothalamus by the work of Walter R. Hess on the sleep-waking cycle, by Philip Bard's studies of "sham rage," and by the mounting evidence for its endocrine and neurosecretory relationships. And he recognized that many of the issues raised by these reports demanded anatomical answers that could not be pursued with contemporary neuroanatomical techniques. His fluency in several languages gave him the opportunity to look for a position beyond the Netherlands, and in 1947 he was appointed to a lectureship at the University of Zürich, Switzerland. While many might imagine that it was the presence in Zürich of Walter Hess—perhaps the most prominent investigator of the hypothalamus at that time—that had attracted Nauta, Hess in fact had rejected Nauta's work as lacking behavioral relevance, and Nauta became a lecturer in the Department of Anatomy under the direction of Professor Gian Töndury.

In his four years at Zürich Nauta established himself as a very popular teacher, something that was to characterize him for the remainder of his career and something that was particularly important in Zürich, since his income depended upon student attendance at his lectures and labs. But it was also here that his obsession with developing an improved neuroanatomical tracing technique came to the fore. The direct investigation of neural connections in the hypothalamus, in which most fiber tracts are not myelinated, had always been hampered by the lack of a practical experimental method for revealing unmyelinated fibers belonging to particular functional systems. At the hands of earlier neuroanatomists such as Cajal, the study of normal fibers impregnated by the Golgi technique had yielded some basic information on local connectivity; but the sheer tangle of fibers in their fine meshworks in the hypothalamus made imperative a more selective technique for labeling fibers that began or ended among identified neuronal groups, some of them—such as the hippocampus—far removed from the hypothalamus. For nearly three-quarters of a century the identification of fiber tracts experimentally had meant placing a surgical lesion in one part of the brain of an animal and then following the anterograde (Wallerian) degeneration of axons away from the cell bodies affected by the lesion, or identifying the cells projecting their axons to the region lesioned by their retrograde reaction to destruction of their axon terminations. Unfortunately, in the 1940s the single available anterograde stain—that of Marchi—revealed only the degenerating myelin sheaths of myelinated fibers, which were generally lacking in the hypothalamus, and the cells of origin of many fiber systems afferent to or within the hypothalamus itself were protected from retrograde degeneration, probably because of their widespread axon collaterals. What was needed was a stain that would selectively pick out degenerating axoplasm, hopefully against a background in which staining of normal fibers unaffected by the experimental lesion was suppressed. In this way any fibers, myelinated or unmyelinated, could be traced from the point of lesion to their regions of termination. It was to this that Walle Nauta assiduously applied himself during his years in Zürich. Fortunately, he enjoyed both enormous popularity as a teacher and the unstinting support of Professor Töndury, because, from the point of view of publications, it was far from a productive enterprise.

The Nauta method, as we all came to call it, had its origins in the reduced silver methods of Bielschowsky and Cajal, in which metallic silver is deposited in axons in thin histological sections by reduction of a weak solution of silver nitrate. Regrettably, this lacks specificity, and all processes of all nerve cells, axons and dendrites, are impregnated. A modification—that of Paul Glees—described in 1946, could reveal the endings of those fiber systems in which the terminal boutons on degenerating axons enlarged and assumed a ring-like configuration as the result of what was later revealed as a neurofilamentous hyperplasia. However this technique did not suppress staining of that obscuring mass of normal fibers, and was plagued with false positives. Nauta had had some success in identifying degenerating terminal ramifications with other forms of silver stain (1947), such as that of Bodian (1936) but he saw in the Bielschowsky method, in which silver is precipitated from an unstable ammoniacal salt, a better chance of revealing degenerating fibers in their entirety (1993). The Nauta stain in its first prototype (1951) did reveal quite clearly the fragmenting axons undergoing anterograde degeneration, but it suffered from a failure to suppress the co-staining of the intact and still obscuring normal fibers. The suppression of the normal fibers was to come in a later iteration (1952; 1954,2; 1955), in which by mordanting the sections in potassium permanganate and potassium bisulfite, the later deposition of silver in normal fibers could be suppressed.

In retrospect it became clear that this was obtained at the cost of also suppressing staining of many of the finer degenerating fibers. However, until further improved by other hands (see below), the suppressive Nauta-Gygax method became the method of choice for experimental neuroanatomical studies for more than a decade. That period was one in which all the major connections of the central nervous system were reinvestigated at a level of resolution never before possible.

Nauta's approach in developing his stain seems to have been largely empirical and to have consisted of little more than trying out again and again different combinations of oxidizing and reducing agents both before and during the silver reduction phase. Serendipity helped when one day an unusually successful result was traced to the use of formalin from an old bottle in which the concentration of formic acid would have been particularly high. Citric acid, for unknown reasons, also proved to be advantageous, especially in slowing down the rate of silver precipitation so that the background of the sections remained transparent under the microscope. Nauta was assisted in his attempts to understand the mechanisms of his stain by Lloyd F. Ryan, a U.S. Air Force major, later the European project manager for a General Electric Company defense contract, and a skilled photographer; and by Paul A. Gygax, a doctoral student in organic chemistry at the Swiss Federal Institute of Technology and later a chemist with the I. G. Farben Company, but it is probably fair to say that the mechanism for achieving selective suppression of one set of fibers and enhanced staining of another has never been satisfactorily elucidated. Nauta himself once remarked to the author of this biographical sketch that only Gygax understood it. I recall a student of mine invariably bowing three times in the direction of Boston before embarking on the silver deposition phase of the stain. Whether this was respect, disrespect, or merely superstition was never quite clear.

Knowledge of the stain began quickly to leak out and Nauta's demonstration of it at a meeting in Paris was met with an enthusiastic response in anticipation of the enormous possibilities it offered for a much higher resolution of neuroanatomical connectivity than had previously been available. On the strength of this he was invited by David McKenzie Rioch—who was at the Paris meeting and was forming the Research Division of Neuropsychiatry at the recently renamed Walter Reed Army Institute of Research in Washington, D.C.—to visit the division, and after a successful visit to join it as an investigator. This Nauta did in 1952, remaining there until moving to the Massachusetts Institute of Technology in 1964. During Nauta's time at Walter Reed and under the direction of Rioch, the Division of Neuropsychiatry became remarkable for the breadth and quality of its research; from it emerged some of the most distinguished neuroscientists of the recent past. Among those who were there during Nauta's time were John Boren, Joseph Brady, Boyd Campbell, Sven Ebbesson, Ford Ebner, Michael Fuortes, Robert Galambos, William Hodos, David Hubel, Harvey Karten, JacSue Kehoe, John Mason, William Mehler, James Petras, George Moushegian, Enrique Ramón-Moliner, Felix Strumwasser, and Eliot Valenstein. Many of these worked in Nauta's laboratory; others formed part of a very successful section of neurophysiology.

It was after moving to Walter Reed that Nauta's first papers on neural connectivity, as carried out with his new technique, began to appear. The first, with one of his Zürich colleagues, Verena Bucher, on efferent connections of the visual cortex in the rat came in 1954; his seminal paper on the distribution of the fibers of the fornix in the rat came in 1956. By this time the technique was coming into widespread use and it was to remain the principal method of experimental neuroanatomy until superseded by methods based upon axoplasmic transport in intact axons that came into use in the 1970s. Among the improvements made by others were those that gave enhanced visualization of the terminations of a degenerating axonal system, one of the most significant of them being made by Robert P. Fink and Lennart Heimer working in Nauta's laboratory at MIT and derived in large part from the original nonsuppressive Nauta-Gygax technique (Fink and Heimer, 1967).

Nauta's own output of publications was relatively small but all were influential and some of the most important appeared as chapters in books rather than as research reports. It would seem that at Walter Reed many experiments were done that never found their way into major publications. Perhaps the fact that there were relatively few publications was determined not only by a distaste for the customary rush to publication but also by a meticulous and perfectionist approach to writing that made the preparation of papers for publication unusually laborious.

Nauta's early papers commonly came at key moments when the field was ready for an examination of the connections of a particular part of the brain, and these publications served as an impetus to other work and as fundamental accounts against which all other studies had to be measured. His work on the distribution of the fornix (1956,1), on the connectivity of the amygdala (1956,2; 1961; 1962) and basal ganglia (1966,1), and on the spinothalamic tract (1960,2) all stand in this category. Some were published as solo authored papers, others with his associates at Walter Reed, of whom there were many, although not all became coauthors. Later studies on the connections of the habenula, substantia nigra, and basal ganglia, mainly carried out with students, reflected a concentration on the motor system and its relationships with the limbic system and less involvement with the hypothalamus. His broad knowledge, which extended beyond the mammalian brain, was reflected in occasional forays into the field of comparative neuroanatomy as well (1970,1; 1973,2). Many other studies that came out of his laboratory at MIT could probably have justifiably borne his name as a coauthor, but it was typical of him that in invariably promoting the interests of his students even at the expense of his own, his name did not appear.

Recognition, nevertheless, came steadily to Walle Nauta. He was elected to the National Academy of Sciences (in 1967), the American Philosophical Society, and the American Academy of Arts and Sciences, and received a number of honorary degrees from U.S. and foreign universities. He was a recipient of the Karl Spencer Lashley Award of the American Philosophical Society, the Henry Gray Award of the American Association of Anatomists, the Ralph W. Gerard Award of the Society for Neuroscience, and the Bristol Myers Award for neuroscience research. He was a founding member of the Society for Neuroscience and one of its early Presidents (1972-1973); he was a long-standing affiliate and active participant in the Neurosciences Research program in whose symposia and other activities he took great pleasure.

Nauta's approach to the brain was that of a generalist, not as one devoted to any single system, although the limbic system and motor control systems could always be seen as particular interests. His lectures revealed not only an enormous depth of neuroanatomical knowledge but also the capacity to form linkages between the systems of the brain: sensory-motor, motivational, visceral, cortical-subcortical. And he was never averse to promoting research not his own in his public lectures. His broad knowledge was kept fresh by a deep commitment to teaching. Nauta never separated his research, as so many do, from the teaching enterprise. He also remained rooted in medical neuroanatomy, and during his period at Walter Reed he regularly taught in the medical neuroanatomy course at the University of Maryland. His partners in this course were Hans Kuypers, another Dutchman who had been a student at Leiden from the end of World War II, and William Mehler, who were not only intimate friends but two of the premier neuroanatomists of their generation.

When Nauta moved to MIT in 1964 he became professor of neuroanatomy in the Department of Psychology, at that time headed by Hans Lukas Teuber, who had been instrumental in his recruitment. Nauta became Institute Professor in 1973 and remained in that position until his retirement and transfer to emeritus status in 1986. He taught neuroanatomy throughout his time at MIT, and his lectures were extremely popular among graduate students and others. Among those who benefited from his support and tutelage were many of today's neuroscientists, including Robert Beckstead, Valerie Domesick, Patricia Goldman-Rakic, Ann Graybiel, Elizabeth Grove, Susan Haber, Lennart Heimer, Miles Herkenham, Harvey Karten, Christiana Leonard, Gene Merrill, and Gerald Schneider, to name but a few. Nor was his influence unfelt at another institution located across the Charles River. But he seems to have yearned for medical neuroanatomy and was delighted when the joint Harvard-MIT M.D.-Ph.D. program was launched, because it gave him the opportunity to interact again with medical students.

Walle Nauta had a deep sense of social responsibility and obligation to his fellow man but the days of hardship during the Second World War had left their mark on him, and he had little tolerance for those whose sense of entitlement was far greater than their experience of the world.

He was convinced that there was real evil in the world and that it was often necessary to resort to any means, even military force, to overcome it. In this he was apt to be labeled a conservative, but if so, it was a conservatism born of adversity and it belied a deeper sense of humanity.

Walle Nauta enjoyed ships and sailing, possibly on account of his childhood travels to and from Indonesia, and he sometimes claimed that his interest in cross-sectional anatomy derived from his parallel interest in the deck plans of ships and from a family tradition as shipbuilders. The name Nauta is said to be derived from *nauta hollandis*, or Dutch sailor, a reminder of a medieval past when Dutch seamen raided the Baltic forests for timber for shipbuilding. Sailing was always a passion with Walle Nauta. As a young man he received numerous awards for sailboat racing and had he not had to face his medical school examinations in 1938, he would have competed for a place in the Dutch Olympic sailing team. It is claimed that his competitive instincts were never as forcefully revealed as when he was in command of his Thistle-class sailboat in which he raced on outer Boston harbor until well into his sixties. He was still sailing a Sunfish for recreation in his seventies.

Walle Nauta represented a type of neuroscientist that is no longer with us. A classical neuroanatomist with an enormous depth of knowledge who could work on any part of the brain, but one who was also in touch with modern developments in neuroscience and able to cast his neuroanatomical studies in a modern context. With a strong base in medicine, as so few basic scientists have today, he never lost sight either of the necessity of casting one's research in the context of the diseased nervous system. He was perhaps the epitome of nonreductionism, not a prevailing motif in today's neuroscience. Yet his influence was broad, not only on account of his modernizing, almost single handedly, the whole field of experimental neuroanatomy but also because of the influence that he had over so many students and fellow scientists as a collaborator or teacher or as an author of some of the most fundamental papers in neuroscience.

I AM INDEBTED TO DR. Haring J. W. Nauta and Dr. Harvey J. Karten for assistance in preparing this memoir.

REFERENCES

Bodian, D. 1936. A new method for staining nerve fibers in paraffin sections. *Anat. Rec.* 65:89-97.

Fink, R. P., and L. Heimer. 1967. Two methods for selective silver impregnation of degenerating axons and their synaptic endings in the central nervous system. *Brain Res.* 4:369-374.

Glees, P. 1946. Terminal degeneration within the central nervous system as studied by a new method. *J. Neuropathol. Exp. Neurol.* 5:54-59.

SELECTED BIBLIOGRAPHY

1947

With J. J. Van Straaten. The primary optic centres in the rat. An experimental study by the "bouton" method. *J. Anat.* 81:127-134.

1950

The so-called terminal degeneration in the central nervous system as seen in silver impregnation. *Schweiz. Arch. Neurol. Psychiat.* 66:353-376.

1951

With P. A. Gygax. Silver impregnation of degenerating axon terminals in the central nervous system: (1) Technic (2) Chemical notes. *Stain Technol.* 26:5-11.

1952

With L. F. Ryan. Selective silver impregnation of degenerating axon in the central nervous system. *Stain Technol.* 27:175-179.

1953

With J. V. Brady. Subcortical mechanisms in emotional behavior: Affective changes following septal forebrain lesions in the albino rat. *J. Comp. Physiol. Psychol.* 46:339-346.

1954

With D. G. Whitlock. An anatomical analysis of the non-specific thalamic projection system. In *Brain Mechanisms and Consciousness,* ed. J. F. Delafresnaye, pp. 81-116. Oxford: Blackwell.

With P. A. Gygax. Silver impregnation of degenerating axons in the central nervous system: A modified technic. *Stain Technol.* 29:91-93.

With V. M. Bucher. A note on the pretectal cell group in the rat's brain. *J. Comp. Neurol.* 100:287-295.

With V. M. Bucher. Efferent connections of the striate cortex in the albino rat. *J. Comp. Neurol.* 180:257-285.

1955

With J. V. Brady. Subcortical mechanisms in emotional behavior: The duration of affective changes following septal and habenular lesions in the albino rat. *J. Comp. Physiol. Psychol.* 48:412-420.

With P. Glees. A critical review of studies on axonal and terminal degeneration. *Mschr. Psychiat. Neurol.* 129:74-91.

1956

An experimental study of the fornix system in the rat. *J. Comp. Neurol.* 104:247-272.

With D. G. Whitlock. Subcortical projections from the temporal neocortex in *Macaca mulatta*. *J. Comp. Neurol.* 106:182-212.

1957

Silver impregnation of degenerating axons. In *New Research Techniques of Neuroanatomy,* ed. W. F. Windle, pp. 17-26. Springfield, Ill.: C. C. Thomas.

1958

Hippocampal projections and related neural pathways to the midbrain in the cat. *Brain* 81:319-340.

With H. G. J. M. Kuypers. Some ascending pathways in the brain stem reticular formation. In *Reticular Formations in the Brain,* eds. H. H. Jasper, L. D. Proctor, R. S. Knighton, W. C. Noshay, and R. T. Costello, pp. 3-30. Boston: Little Brown.

1959

With E. S. Valenstein. A comparison of the distribution of the fornix system in the rat, guinea pig, cat and monkey. *J. Comp. Neurol.* 113:337-364.

1960

Limbic system and hypothalamus: Anatomical aspects. *Physiol. Rev.* 40:102-104.

With W. R. Mehler and M. E. Feferman. Ascending axon degeneration following anterolateral cordotomy: Experimental study in the monkey. *Brain* 83:718-750.

1961

Fibre degeneration following lesions of the amygdaloid complex in the monkey. *J. Anat.* 95:515-531.

1962

Neural associations of the amygdaloid complex in the monkey. *Brain* 85:505-520.

1964

Some efferent connections of the prefrontal cortex in the monkey. In *The Frontal Granular Cortex and Behavior,* eds. J. M. Warren and K. Akert, pp. 397-409. New York: McGraw-Hill.

With M. Cole and W. R. Mehler. The ascending efferent projections of the substantia nigra. *Trans. Am. Neurol. Assoc.* 89:74-78.

1966

With E. Ramón-Moliner. The isodendritic core of the brain stem. *J. Comp. Neurol.* 126:311-335.

With W. R. Mehler. Projections of the lentiform nucleus in the monkey. *Brain Res.* 1:3-42.

1967

With L. Heimer and F. F. Ebner. A note on the termination of commissural fibers in the neocortex. *Brain Res.* 5:171-177.

1968

With M. Cole. Retrograde changes of axons in the medial lemniscus. *J. Neuropathol. Exp. Neurol.* 27:122-123.

1969

With L. Heimer. The hypothalamic distribution of the stria terminalis in the rat. *Brain Res.* 13:284-297.

1970

With H. J. Karten. A general profile of the vertebrate brain with sidelights on the ancestry of cerebral cortex. In *The Neurosciences: Second Study Program,* eds. F. O. Schmitt and F. G. Worden, pp. 7-26. New York: Rockefeller University Press.

With M. Cole. Retrograde atrophy of axons of the medial lemniscus of the cat. *J. Neuropathol. Exp. Neurol.* 29:354-369.

With S. O. E. Ebbesson, eds. *Contemporary Research Methods in Neuroanatomy*. New York: Springer-Verlag.

1971

The problem of the frontal lobe: A reinterpretation. *J. Psychiat. Res.* 8:167-187.

1972

Neural associations of the frontal cortex. *Acta Neurobiol. Exp.* (Warsaw) 32:125-140.

1973

With A. M. Graybiel, H. J. W. Nauta, and R. J. Lasek. A cerebello-olivary pathway in the cat: An experimental study using autoradiographic tracing techniques. *Brain Res.* 58:205-211.

With H. J. Karten, W. Hodos, and A. M. Revzin. Neural connections of the Visual Wulst of the avian telencephalon, experimental studies in the pigeon (*Columba livia*) and owl (*Speotyto cunicularia*). *J. Comp. Neurol.* 150:253-278.

1976

With P. Goldman. Autoradiographic demonstration of cortico-cortical columns in the motor, frontal association and limbic cortex of the developing rhesus monkey. *Neuroscience* 2:136.

With P. S. Goldman. Autoradiographic demonstration of a projection from prefrontal association cortex to the superior colliculus in the rhesus monkey. *Brain Res.* 116:145-149.

1977

With M. Herkenham. Afferent connections of the habenular nuclei in the rat. A horseradish peroxidase study, with a note on the fiber-of-passage problem. *J. Comp. Neurol.* 173:123-146.

With P. S. Goldman. An intricately patterned prefronto-caudate projection in the rhesus monkey. *J. Comp. Neurol.* 171:369-386.

With P. S. Goldman. Columnar distribution of cortico-cortical fibers in the frontal association, limbic, and motor cortex of the developing rhesus monkey. *Brain Res.* 122:393-414.

1978

With G. P. Smith, R. L. M. Faull, and V. B. Domesick. Efferent connections and nigral afferents of the nucleus accumbens septi in the rat. *Neuroscience* 3:385-401.

With V. B. Domesick. Crossroads of limbic and striatal circuitry: Hypothalamo-nigral connections. In *Limbic Mechanisms,* eds. K. E. Livingston and O. Hornykiewicz, pp. 75-93. New York: Plenum.

1979

With H. Potter. A note on the problem of olfactory associations of the orbitofrontal cortex in the monkey. *Neuroscience* 4:361-368.

With M. Feirtag. The organization of the brain. *Sci. Am.* 241:88-111.

With M. Herkenham. Efferent connections of the habenular nuclei in the rat. *J. Comp. Neurol.* 187:19-48.

With R. M. Beckstead and V. B. Domesick. Efferent connection of the substantia nigra and ventral tegmental area in the rat. *Brain Res.* 175:191-218.

1980

With I. R. Kaiserman-Abramof and A. M. Graybiel. The thalamic projection to cortical area 17 in a congenitally anophthalmic mouse strain. *Neuroscience* 5:41-52.

1982

With A. E. Kelley and V. B. Domesick. The amygdalostriatal projection in the rat—an anatomical study by anterograde and retrograde tracing methods. *Neuroscience* 7:615-630.

1983

With S. N. Haber. Ramifications of the globus pallidus in the rat as indicated by patterns of immunohistochemistry. *Neuroscience* 9:245-260.

1984

With H. P. Lipp and R. L. Collins. Structural asymmetries in brains of mice selected for strong lateralization. *Brain Res.* 310:393-396.

1985

With S. N. Haber, H. J. Groenewegen, and E. A. Grove. Efferent connections of the ventral pallidum: Evidence of a dual striato pallidofugal pathway. *J. Comp. Neurol.* 235:322-335.

1986

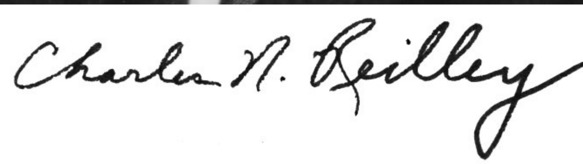
With E. A. Grove and V. B. Domesick. Light microscopic evidence of striatal input to interpallidal regions of cholinergic cell group Ch4 in the rat: A study employing the anterograde tracer *Phaseolus vulgaris* leukoagglutinin (PHA-L). *Brain Res.* 367:379-384.

With H. J. Groenewegen, S. Ahlenius, S. N. Haber, and N. W. Kowall. Cytoarchitecture, fiber connections and some histochemical aspects of the interpeduncular nucleus in the rat. *J. Comp. Neurol.* 249:65-102.

With R. L. M. Faull and V. B. Domesick. The visual cortico-striatonigral pathway in the rat. *Neuroscience* 19:1119-1132.

1993

Some early travails of tracing axonal pathways in the brain. *J. Neurosci.* 13:1337-1345.



CHARLES NORWOOD REILLEY

March 2, 1925–December 31, 1981

BY ROYCE W. MURRAY

CHARLES N. REILLEY WAS BORN in 1925 in Charlotte, North Carolina. His mother, a public school teacher, was widowed when Charles was young; her husband died from an illness. Charles was fascinated with radio and electrical things while still a grammar school child. His older brother Eugene said that when Charlie (everyone called him Charlie except his mother—there it was Charles) got to high school and found out about science, its beauty so attracted him that he never could turn his eyes away again. Reilley went on to become an outstanding scholar in the field of analytical chemistry, with a fundamentally-oriented approach that strongly influenced the character and reputation of that discipline in the 1950s and 1960s. He was elected to the National Academy of Sciences in 1977.

Charlie Reilley had enviable personal qualities. He was a soft-spoken, modest person whose conversations revealed a deep intellect and the soul of a teacher. His love for teaching almost certainly was nurtured by his mother, to whom Charlie was a devoted bachelor son until his death. A salute from his colleagues after his death stated: "Charlie Reilley was the friend, teacher, student, and colleague of almost everyone who worked with him. He was a scientist of unlimited imagination. Most of all he was a generous human being who believed in always giving more than he took." This author of this memoir—on whose professional life Reilley had great positive influence—deeply agrees with this analysis.

HIS EDUCATION

After graduating from Central High School in Charlotte in 1943, Charles Reilley enrolled at the University of North Carolina and graduated with a B.S. in chemistry in 1947. He was a capable student and was honored with Phi Beta Kappa, Alpha Chi Sigma undergraduate chemistry awards for three years (1945-1947), and the Archibald Henderson Medal in Mathematics in 1946. He returned to Charlotte and taught chemistry there at Queen's College for two years, and then returned to school to earn his M.A. (in 1951) and Ph.D. (in 1952) in chemistry at Princeton University under Professor Nathaniel Howard Furman, who at that time had made his institution an international center of analytical chemistry. As a graduate student Reilley was recognized with an ACS Division of Analytical Chemistry Graduate Fellowship in 1951. While at Princeton, Reilley was a colleague in Furman's laboratory of W. Donald Cooke and Ralph N. Adams, who respectively went on to distinguished careers in analytical chemistry at Cornell University and the University of Kansas. Furman's letter recommending him for a faculty position at UNC noted: "The other graduate students find him a very useful man to talk to" and "I believe that he will make a first-class teacher and research worker." Furman also noted that he had in hand "an offer to remain here."

After leaving Princeton, Reilley returned to Chapel Hill as an instructor and rose through the ranks to become an assistant professor in 1953, associate professor in 1956, professor in 1961 and Kenan Professor in 1963. The Kenan chair is a prestigious one at the University of North Carolina, linking the names of Venable and Morehead with Kenan in a saga in which the origins of the company Union Carbide can be found.

HIS RESEARCH CONTRIBUTIONS

Charles N. Reilley was among the first modern analytical chemists of the mid-twentieth century. His interests were both fundamental and broad—a "man for all seasons." Charlie made seminal contributions to all of the major legs of analytical chemistry: electroanalysis, optical spectroscopy, magnetic resonance spectroscopy, chemical separations, data analysis, instrumentation, and surface analysis. His research was based on fundamentals yet retained an eye to significant applications in analytical measurements. However, the signature of his research—in contrast to previous decades of the analytical chemistry subdiscipline that had weakened its stature in the eye of many—was that in it he declined empiricism and instead sought and demanded a more basic understanding of measurements and detection schemes that had evolved in analytical chemistry. Reilley recognized that measuring things was at the heart of modern chemistry, that everyone relied upon analytical measurements, and that it was the responsibility of the analytical chemist to outline their fundamental character. Today the analytical chemistry subdiscipline is a vibrant and respected science, in part owing to the leadership of Charles Reilley decades ago.

Many of Reilley's early important contributions centered on the understanding of methods for detection of chemical reactions as they reached stoichiometric completion. These stoichiometrically exact reactions are commonly called titrations and are crucial in numerous quantitative analytical measurements. Reilley's thesis research broached the first example of his talent in this direction. Others at that time had found that the equivalence points of titrations could be detected by imposing potential or current biases between two detecting electrodes, but the understanding of such detection was quite incomplete. Reilley's 1951 paper with Cooke and Furman elucidated the basic mechanism of current-polarized detecting electrodes and set a definitive stamp on this field.

Reilley's interest in detecting the completion of exact stoichiometric reactions led to a series of further works on detection of coulometric titration equivalence points. Because of the accuracy of controlling current for electrogeneration of reagents in quantitative reactions, coulometry was one of the earliest successful instrumental methods. Reilley introduced the exact electrogeneration of cerium (IV) as an oxidative titrant reactant. This reaction retained a consistent usefulness in analysis for many decades. Reilley extended coulometric titration themes into a succession of trace analytical measurements, and in reflection of his capacities in electronic circuitry, designed instruments (before the solid-state transistor) for more accurate and conveniently controlled currents in coulometric reactions. Reilley was also interested (presumably from his childhood examinations of how radios work) in using high-frequency potentials for electrochemical detection, and while still a graduate student published an independent paper (1953) on high frequency titrimetry. This paper set out ideas that were fundamental and correct, but not much used until later in their rediscovery in the modern (in the 2000s) field of contactless ionic conductivity detectors.

Work continued on instrumental detection of the completion of stoichiometric reactions on Reilley's arrival in Chapel Hill. One of his first publications at UNC (1954) involved detecting acid-base reactions in nonaqueous media using spectrophotometry, which at that time was a relatively new subject. (One must remember that quantitative optical absorbance measurements were a mid-twentieth-century development.) He also continued research for several years aimed at developing constant current coulometric analysis, and during these studies became interested in a vital chemical aspect of analysis, that of using metal complexes as a means to generate reaction selectivity. (Selectivity even now is a continuously sought goal in modern chemical analyses.) He demonstrated (1956) how to coulometrically release the multidentate chelating ligand ethylenediaminetetraacetic acid (EDTA), and described a number of trace metal analyses using it. His postdoctoral associate R. W. Schmid was one of the first in his laboratory to explore multidentate metal complexation as a means of selectivity in quantitative reactions, and to investigate ways to measure metal chelate complex formation constants in that context. Schmid and Reilley recognized that a basic understanding of complexation equilibria was central to predictive design of metal-complexation-based analysis. Reilley's interest in metal complexes ("chelons" was the term used) led him into a long series of metal chelate studies—their equilibria, complexation kinetics, and application, using measurements based on electrochemistry, nuclear magnetic resonance, and spectrophotometry. He appreciated that ligands that bound a metal with multiple binding sites would enjoy an entropic advantage in forming stable complexes. An important, and still employed method (1956) for measuring metal complex equilibria and detecting reaction completion involved using the extremely stable mercury EDTA complex and a Hg potentiometric electrode as an indicator of competition for the EDTA by an analyte metal ion. This principle was subsequently combined with coulometric titration methodology in 1958. Reilley fully appreciated and exploited the relationship between metal chelate stability and the redox potential of the involved metal ion's electron transfer reactions.

Reilley's research interests thereafter retained a continuing probing of metal complexation equilibria and how to measure such equilibria. He exploited Beer's law principles in assessing the effectiveness of colored indicator complexes in titrations. With Schmid he mathematically defined (1959) the qualities of equivalence points in relation to equilibrium parameters. His 1959 publication contained a periodic table in which more than one-half of the elements could be analytically measured using metal chelons. Using potentiometric data, he showed that diagrams equivalent to phase diagrams could be employed to show what titrations should be possible and which ones not.

Reilley's electrochemical research using constant currents at electrodes enabled him to quickly recognize the potential importance of a report by Paul Delahay (Louisiana State University) about behavior of electrodes under constant currents when the electrode reactant supply was controlled strictly by diffusion only. Reilley carried out the first quantitative analytical study of this experiment, which was called chronopotentiometry according to the measurement of electrode potential as a function of time. A great deal of attention was subsequently paid by others to the chronopotentiometry experiment, owing to the relative simplicity—due to Reilley—of circuits to control constant currents at electrodes.

In the 1960s Reilley became interested in thin-layer electrochemistry, an experiment introduced by Fred Anson in about 1962, in which the electrochemical cell was a thin layer of solution bounded by a working electrode. Reilley introduced in 1965 the notion of using two, facing, working electrodes in thin-layer cells—along with a mechanism for varying the important interelectrode distance. Reilley also provided an operational amplifier-based principle of controlling the potentials of both electrodes against a reference electrode and thereby contributed an important form of electrochemical generation-collection in a closed system. Reilley and his postdoctoral associate L. B. Anderson stated the theoretical principles for generation-collection, and showed how the thin-layer experiment could be used for study of chemical reaction kinetics.

In 1962 Reilley—in collaboration with faculty (Daniel Okun) in the School of Public Health at UNC—described an electrode useful for field determinations of the oxygen concentration in natural waters. The importance of this measurement—from an environmental viewpoint—was just beginning to be appreciated, and this electrode was patented and commercialized for measuring dissolved oxygen in lakes and streams. The electrode design, importantly, required no applied voltage, since it relied on a spontaneous (galvanic) reaction within the electrochemical cell. A second important innovation, key for field usefulness, was a oxygen-permeable membrane film that encapsulated the operating galvanic cell. Graduate student K. H. Mancy, who went on to a distinguished career in environmental chemistry, discovered in early versions that the latex membranes of condoms were good candidates for the encapsulation.

The coordination chemistry of metal chelates continued as a major theme throughout Reilley's research career. He developed new chelate reagents and in 1965 calorimetrically assessed the thermodynamic principles underlying the "chelate effect" and their relevance to analytical measurements. This was one of the first studies that paid attention to the relation between chelon structure and the ensuing stability constant with a metal complex. Reilley's interest in metal chelates also led him to nuclear magnetic resonance, at an early phase of use of NMR by chemists to study chemical equilibria and other phenomena. His were among the first publications that emphasized the proton NMR of the ligands of metal complexes—as opposed to small organic molecules. Starting in 1965 his students exploited NMR to investigate ligand conformations and exchange kinetics, microscopic protonation schemes in polyacidic bases, hydration of the counterions of ion exchange resins, and contact shifts of paramagnetic complexes. His 1964 introduction of proton NMR for measuring microscopic protonation patterns in polyprotic acids and bases established that method as the gold standard—continuing today—for microscopic equilibria studies. He introduced carbon(13) NMR for the same purpose in the 1970s. Reilley's corpus of work, with varied methodologies and always with an eye to fundamental understanding, did much to foster fruition of the existing appreciation of chelate structure and reactivity.

Reilley made important contributions in the 1960s to gas liquid partition chromatography and prepared a classical and much-cited chapter on differential kinetic analysis that set the stage for years of further development of kinetic analysis as a field. In the early 1970s Reilley and his student Richard Van Duyne published three landmark papers that firmly established the power of lowered temperatures for detection and quantitative kinetic elucidation of chemical reactions accompanying electrochemical transformations. Reilley was active in early designs of data acquisition systems and computer-interfaced analytical instruments, and in the birth of the field of chemometrics, entered a collaboration with Thomas Isenhour through studies on computerized learning machines (e.g., pattern recognition and artificial intelligence). These endeavors were published in 1969 and 1970, and dealt with mass spectrometric and vibrational spectral data.

Reilley's interests in the later 1970s turned to include polymer surface analysis, through collaboration with H. Yasuda of the Research Triangle Institute. The themes were the surfaces of plasma-polymerized films and the application of X-ray photoelectron spectroscopy (then called ESCA) to polymer surfaces. This line of research was unfortunately terminated by Reilley's death in 1981. Reilley's work published in 1981 with his student Dennis Everhart was—as typical of his entire career—an early entry into the literature of polymer surface functional group analysis by derivatization tagging.

HIS TEACHING, SERVICE, AND HONORS

Charles Reilley's research scholarship and leadership in fundamental science soon brought him to the national attention of other chemists, including analytical chemists, of course, but also a wider circle of disciplines. Reilley was elected as secretary-treasurer of the American Chemical Society Division of Analytical Chemistry for 1958-1959 and subsequently as its chairman in 1961. He was afterward elected as an ACS councilor from the North Carolina Section and then was chosen as a member of the ACS Council Policy Committee. He chaired the Gordon Research Conference on Analytical Chemistry in 1960.

Reilley was recognized for his research accomplishments with a Guggenheim Fellowship in 1962, the ACS Award in Analytical Chemistry (also known as the Fisher Award) in 1965, the Herty Medal in 1968, the Stone Award in 1971, the national ANACHEM Award in Analytical Chemistry of the Association of Analytical Chemists in 1972, and the Manufacturing Chemist's Association College Chemistry Teaching Award in 1975.

Reilley contributed to his department at Chapel Hill in innumerable ways, by example of his own high scholarship and by design of a farsighted undergraduate curriculum for the modern chemistry student. He advocated a more quantitative laboratory experience for freshmen chemists, and a drastic revision of what he called a "seriously out of touch" sophomore introduction of analytical chemistry. Noting that "the pressure of new knowledge continually forces basic concepts into earlier stages of the curriculum," he advocated an "adiabatic transition from the traditional course." Reilley recognized the importance and revolutionizing impact that instrumental methods of measurement in chemistry were exerting in the late 1950s and the 1960s, and in effect proposed that they be taught beginning at the sophomore level in analytical chemistry courses. Making sophomore analytical chemistry into an introductory instrumental analysis course helped propel his department's program nationally into one of the most successful centers granting undergraduate degrees in chemistry.

Reilley brought the above ideas into the national scene by proposing them publicly in his introductory remarks upon receiving the Fisher Award in 1965. They set off a national discussion of the teaching of analytical chemistry (*Anal. Chem.* 38[1966]:35A-57A) and had a subsequent, profoundly healthy effect on that part of the undergraduate chemistry curriculum.

Reilley was widely sought after as a research lecturer, the Baker Lectures at Cornell University in 1979 and the Reilly Lectures at Notre Dame in 1966 being examples. He was the editor of the series "Advances in Analytical Chemistry and Instrumentation" and was on numerous journal editorial boards, including *Analytical Chemistry* and *Accounts of Chemical Research.* He served on the National Science Foundation Chemistry Advisory Panel during 1958-1960.

Charlie Reilley is central in the history of the Society for Electroanalytical Chemistry, taking part in the predecessor San Clemente, California, CVs-on-the-sand meeting of about 15 electroanalytical chemists in 1963. That enormously stimulating gathering of young Turks and experienced wise heads like Reilley and Ralph Adams led to initiation of the Gordon Conference on Electrochemistry. Perhaps equally important in this inaugural meeting was the spirit of trust and intellectual fellowship engendered by the open conversations, there and subsequently, that have persisted in the science of electroanalytical chemistry to this day.

The level of respect for and admiration of Reilley following his death on the last day of 1981 was reflected in the actions of his friends, former students, and colleagues in steps that led ultimately to the formation of the Society of Electroanalytical Chemistry as a vehicle for managing an award in electroanalytical chemistry, called the Charles N. Reilley Award. The financial sponsorship for the award was provided by former Reilley student Peter Kissinger, who following a postdoctoral study with Ralph Adams and a successful academic career at Purdue University, founded the company Bioanalytical Systems. The award was established in conjunction with the Pittcon Conference and was first awarded in 1984. The distinction of this award can be measured by the fact that four awardees subsequently became members of the National Academy of Sciences (1984, Allen J. Bard; 1986, Fred C. Anson; 1988, Royce W. Murray; and 1990, Jean-Michel Saveant).

Following his death a memorial fund established in 1982 in his name, funded by donations from friends and former students, provided fellowships (Reilley fellows) to outstanding entering graduate students at the Chemistry Department at the University of North Carolina. There are now numerous successful chemists in all areas of the discipline whose resumés acknowledge this support from Reilley's friends and in his memory.

Reilley's own intellectual contributions and leadership were recognized by election to the National Academy of Sciences in 1977. He was the first analytical chemist to be so selected after the also legendary I. M. Kolthoff, 19 years before.

In writing about Charlie Reilley, who became prominent both by scholarship and a love for teaching, it is difficult to completely express his dedication to teaching. It was not just in formal classes that Charlie taught. When you had a scientific conversation with Charlie, it was a stimulating experience. He was a teacher and a student at the same time; your questions to him always prompted questions in return. It was said that "he has the knack of leading the horse to water and patiently waiting for him to drink (or be traded in)."

I WAS A CLOSE PERSONAL friend of Charles Reilley, and shared an office suite. I had the sad task of cleaning his office, and came into possession of numerous files and records, from which I drew the information for this memoir.

SELECTED BIBLIOGRAPHY

1951

With N. H. Furman and W. D. Cooke. Coulometric titrations with electrically generated ceric ion. *Anal. Chem.* 23:945.

1953

With W. H. McCurdy Jr. Principles of high-frequency titrimetry. *Anal. Chem.* 24:86.

1954

With G. W. Everett. Coulometric titrations with photometric end point. *Anal. Chem.* 26:1750.

1955

With G. W. Everett and R. H. Johns. Voltammetry at constant current. Experimental evaluation. *Anal. Chem.* 27:483.

1956

With R. W. Schmid. A simple, rapid method for determination of metal chelate stability constants. *J. Am. Chem. Soc.* 78:2910.

With R. W. Schmid. A rapid electrochemical method for the determination of metal chelate stability constants. *J. Am. Chem. Soc.* 78:5513.

1958

With R. W. Schmid. Chelometric titrations with potentiometric end point detection: Mercury as pM indicator electrode. *Anal. Chem.* 30:947.

1959

With R. W. Schmid. Principles of end point detection in chelometric titrations using metallochromic indicators. Characterization of end point sharpness. *Anal. Chem.* 31:887.

1962

With L. J. Papa. Analysis of binary mixtures by second order kinetics using equal concentrations of reactants. *Anal. Chem.* 34:801.

1962

With K. H. Mancy and D. A. Okun. A galvanic cell oxygen analyzer. *J. Electroanal. Chem.* 4:65.

1964

With R. J. Day. Nuclear magnetic resonance studies of metal aminopolycarboxylate complexes. Liability of individual metal ligand bonds in (ethylenedinitrilo)tetraacetate complexes. *Anal. Chem.* 36:1073.

With J. L. Sudmeier. Nuclear magnetic resonance studies of protonation of polyamine and aminocarboxylate compounds in aqueous solution. *Anal. Chem.* 36:1698.

1965

With N. E. Rigler, S. P. Bag, D. E. Leyden, and J. L. Sudmeier. Determination of a protonation scheme of tetracylcine using nuclear magnetic resonance. *Anal. Chem.* 37:872.

1965

With D. L. Wright and J. H. Holloway. Heats and entropies of formation of metal chelates of polyamine and polyaminocarboxylate ligands. *Anal. Chem.* 37:884.

With L. B. Anderson. Thin-layer electrochemistry: Steady-state methods of studying rate processes. *J. Electroanal. Chem.* 10:295.

With L. B. Anderson. Thin-layer electrochemistry: Use of twin working electrodes for the study of chemical kinetics. *J. Electroanal. Chem.* 10:538.

1969

With P. C. Jurs, B. R. Kowalski, and T. L. Isenhour. Computerized learning machines applied to chemical problems: Investigation of convergence rate and predictive ability of adaptive binary pattern classifiers. *Anal. Chem.* 41:690.

With P. C. Jurs, B. R. Kowalski, and T. L. Isenhour. An investigation of combined patterns from diverse analytical data using computerized learning machines. *Anal. Chem.* 41:1949.

With B. R. Kowalski, P. C. Jurs, and T. L Isenhour. Computerized learning machines applied to chemical problems: Multicategory pattern classification by least squares. *Anal. Chem.* 41:695.

With B. R. Kowalski, P. C. Jurs, and T. L Isenhour. Computerized learning machines applied to chemical problems: Interpretation of infrared spectrometry data. *Anal. Chem.* 41:1945.

1970

With P. C. Jurs, B. R. Kowalski, and T. L. Isenhour. Computerized learning machines applied to chemical problems. Molecular structure parameters from low resolution mass spectrometry. *Anal. Chem.* 42:1387.

1971

With R. F. Evilia and D. C. Young. Contact shift studies of nickel-butylenediamine complexes. *Inorg. Chem.* 10:433.

1972

With R. P. Van Duyne. Low-temperature electrochemistry. I. Characteristics of electrode reactions in the absence of coupled chemical kinetics. *Anal. Chem.* 44:142.

With R. P. Van Duyne. Low-temperature electrochemistry. II. Evaluation of rate constants and activation parameters for homogenous chemical reactions coupled to charge transfer. *Anal. Chem.* 44:153.

With R. P. Van Duyne. Low-temperature electrochemistry. III. Application to the study of radical ion decay mechanisms. *Anal. Chem.* 44:158.

1977

With H. Yasuda, H. C. Marsh, and E. S. Brandt. ESCA study of polymer surfaces treated by plasma. *J. Appl. Polym. Sci.* 15:1977.

1979

With H. Yasuda, N. Morosoff, and E. S. Brandt. Plasma polymerization of tetrafluoroethylene. I. Inductively coupled radio frequency discharge. *J. Appl. Polym. Sci.* 23:1003.

With H. Yasuda, N. Morosoff, and E. S. Brandt. Plasma polymerization of tetrafluoroethylene. III. Capacitive audio frequency (10 kHz) and AC discharge. *J. Appl. Polym. Sci.,* 23:3471-3488.

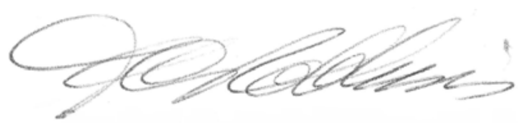
1980

With H. L. Surprenant, J. E. Sarneski, R. R. Key, and J. T. Byrd. Carbon-13 NMR studies of amino acids: Chemical shifts, protonation shifts, microscopic protonation behavior. *J. Magn. Res.* 40:231.

1981

With D. S. Everhart. Chemical derivatization in electron spectroscopy for chemical analysis of surface functional groups introduced on low-density polyethylene film. *Anal. Chem.* 53:665.

Photo by Don Schad, Case Western University School of Medicine.



FREDERICK C. ROBBINS

August 25, 1916—August 4, 2003

BY ADEL MAHMOUD

FREDERICK C. ROBBINS WAS a major force in the fields of infectious diseases, pediatrics, and public health. Early in his career he established a prominent position in the science of medicine, having won the Nobel Prize in Physiology or Medicine in 1954 for work performed in the laboratory of his mentor and co-Nobel Laureate, John Enders. The two of them were joined by Tom Weller in receiving this remarkable honor. Fred Robbins's talent and leadership were demonstrable all through the subsequent 50 years as a physician, investigator, educator, and a statesman of science.

The seminal observation for which Enders, Weller, and Robbins were awarded the Nobel Prize stems from their discovery of how to grow poliomyelitis virus in human cell cultures. This finding led to the development of the two most effective poliomyelitis vaccines and their use in eliminating paralytic polio in many parts of the world. During his scientific career, Fred Robbins was gratified to witness the impact of this seminal discovery on the lives of billions; it was unfortunate that he was unable to see this eradication come to a successful conclusion during his lifetime.

I had the honor and good fortune to be mentored by Fred for over 30 years and to watch him as a professor, dean, and national and international leader. While he was dean and professor at the Case Western Reserve University School of Medicine, he presided over the shaping of the careers of several graduating medical classes and continued his spiritual and role model guidance of these generations of physicians for many years thereafter. As president of the Institute of Medicine he brought the public health as well as the scientific basis of medicine to the forefront of the national and international agenda. Upon returning to Case Western Reserve University, Fred once again was in his best element: leading, being a statesman of science and education, and guiding his colleagues and institutions. Fred was a warm, gentle, caring, and inspiring mentor to the thousands he touched during his remarkable career. For many years he watched and participated in the interplay of the Cleveland medical institutions. He never interfered, but rather encouraged mutual respect and the necessity for collaboration.

EDUCATION AND EARLY LIFE

Frederick C. Robbins was born in Auburn, Alabama, on August 25, 1916. His father was a professor of botany; his mother also was a botanist. The family soon moved to Columbia, Missouri, where he shared the early years with his two brothers. In September 1928 as Fred was turning 12, his father accepted a two-year appointment in Paris. The Robbins family did not think that schooling in Paris was desirable for the boys; so his mother admitted him to a public school in Switzerland. The following year Fred was enrolled in a boarding school, Institut Sellig, located on the shores of Lake Geneva. Fred seems to have enjoyed Switzerland and had a good time exploring the mountains around Lake Geneva and skiing during winter. Although Fred was not fluent in French, he managed many subjects being taught in French, including mathematics. Reflecting on his experience in Switzerland, Fred said that in Switzerland he learned in two years the equivalent of four years in an American school. This was clearly evident as he returned to Columbia, Missouri. Fred was expected to enter ninth grade, but the principal placed him in the eleventh grade because of all the courses he took in Switzerland.

Upon graduation from high school Fred Robbins enrolled in the Arts and Science College of the University of Missouri in 1932. He was on the Honor Rank List of the university in the subsequent three years, graduating with an A.B. degree in 1936. During his college years he changed his plans for a career in chemical engineering to one in medicine. While at college, Fred lived at home but maintained the habit of taking his noon meals and Sunday dinners at his fraternity house. The college years were important for Fred's academic achievements and equally for sports and membership in the university Reserve Officer Training Corps. Fred's favorite sport was polo; he enjoyed the practice and competition. As a member of the ROTC he demonstrated excellence in the basic elements of the military and was admitted to the Tiger and Crack Battery. In 1936 Cadet Major Frederick C. Robbins was awarded a medal of the Sons of the Revolution for his ROTC activities. The medal was for the "highest rating in leadership, soldierly bearing, and excellence."

Following college, Fred enrolled in the two-year medical school of the University of Missouri and graduated with a bachelor of science degree in medicine in 1938. He transferred to Harvard Medical School; at that time Harvard admitted only one Missouri student to each class. Fred earned the M.D. degree from Harvard Medical School in 1940.

THE BEGINNING OF A SCIENTIFIC CAREER

Robbins began his postgraduate career as a resident physician in bacteriology at the Children's Hospital Medical Center in Boston, Massachusetts. He left that position in 1942 for military service and was assigned to the Fifteenth Medical General Laboratory as chief of the Viral and Rickettsial Disease Section. His military service years were spent in Italy and North Africa. While serving in the military, Fred focused his attention on the major infectious diseases of relevance to the services at that time. These included infectious hepatitis, typhus, and Q fever. In a series of carefully studied cases of Q fever Robbins and his colleagues published their findings in the *American Journal of Hygiene* in 1946, detailing the clinical features of the disease, its epidemiology, etiological agent, and the findings during a laboratory outbreak of Q fever. This was followed by examining the impact of vaccination against the disease.

Fred's early encounter with infectious diseases dominated his scientific pursuits for years to come. It reflected his commitment to meticulous science, the value of sharp clinical observations and mechanistic rather than descriptive exploration of scientific phenomena. It also allowed him to focus on infectious diseases within the total conceptual framework of public health and its imperatives.

Fred was awarded the Army's Bronze Star for Distinguished Service; he was discharged in 1946 with the rank of major. He returned to the Children's Hospital Medical Center to complete his training as an assistant resident. Robbins was then accepted for a three-year research fellowship that was supposed to be divided into a year with Dr. John Enders, a year in Australia, and the final year in New York. But the polio work in Enders's laboratory was fascinating, and Fred ended up staying there for the entire three-year fellowship.

THE SCIENTIST

The years in John Enders's laboratory mark the most significant inflection of Fred Robbins's career. In 1947 Dr. John Enders accepted the invitation to establish a laboratory for research on infectious diseases at the Children's Hospital in Boston. He moved to four rooms on the second floor of the Carnegie building and began recruiting assistants. Enders wanted to hire people he knew. The first person was his secretary, and then he hired Alice Northrop (soon to marry Fred Robbins) in September 1947, because he knew her from the Bacteriology Department at Harvard Medical School. Dr. Thomas Weller was there from the beginning to help Dr. Enders set up the lab. Fred Robbins arrived in January 1948. At that point Drs. Enders and Weller were exploring how to grow viruses in cultures of human cells. Enders suggested they study a familiar virus, such as mumps. Weller succeeded in propagating the mumps virus in vitro for the first time; they later developed serologic assays to quantify viral replication.

During the spring of 1948, two observations on poliomyelitis virus were obtained in Enders's laboratory. Weller inoculated a few unused human embryonic skin and muscle cultures with poliovirus. When he inoculated mice with the three-week supernatant from these cultures, all animals demonstrated paralysis. Enders suggested that Robbins use cultures of intestinal tissue similarly. Supernatant of polio-inoculated intestinal tissue cultures induced paralysis equally in mice.

Enders's laboratory shifted its major focus thereafter to poliomyelitis. The laboratory focused on culturing other virus types, examining changes in infected cells, and developing in vitro assays. By 1949 Robbins used the cell culture system to isolate poliovirus from individuals infected with the virus. These observations resulted in the first publication that Enders, Weller, and Robbins communicated to *Science* in 1949. Robbins stayed in Enders's laboratory for a few more years. He continued working on tissue culture methodology to isolate polio and nonpolio enteroviruses from clinical materials. He also devoted a significant effort to studying the growth characteristics and pathological consequence of poliovirus in cultures.

The years Fred Robbins spent in Enders's laboratory were unique scientifically and socially. At the beginning Fred was not assigned a desk or a technician, and he used to confine his mild displeasure quietly to Alice. They finally gave him a desk and then a technician. The laboratory atmosphere was congenial and relaxed. Dr. Enders and the group always ate lunch together and the conversation would cover all sorts of subjects. The Enders lab was a unique, small, and intimate group of five people. They worked hard.

During the early months of 1948, Fred Robbins asked Alice to go to the movies or out for dinner on several occasions; they were engaged in May of the same year. Alice suggests that the speed of their friendship and enjoyment was a result of seeing each other every day. Fred was a very pleasant, easygoing, and relaxed guy. Alice reflects, "Fred always appeared very relaxed. Maybe he was not as relaxed as he seemed, but he just went along with whatever the three of them (Enders, Weller, and Robbins) decided to do." Fred and Alice were married on June 19, 1948, merely six months following Fred's joining the Enders laboratory. The wedding took place in Princeton, New Jersey, where Alice's family lived and where Alice grew up. The whole family enjoyed the wedding party on the lawn, which overlooked Lake Carnegie in Princeton. Following their wedding, Alice had to leave the Enders laboratory, since it was known that Dr. Enders did not want married couples in his laboratory. Alice joined the Department of Nutrition and continued working with experimental animals. The Robbins family maintained close relationship with Dr. Enders for many years. They gave their second daughter the middle name of "Enders."

THE NOBEL PRIZE

Here are Fred's words describing how he got the news:

> In 1953, we heard a rumor that our group had been nominated for the Nobel Prize. We did not take it too seriously and nothing happened. In October 1954, however, I received a call from Western Union, and they read me a telegram from Stockholm, Sweden, stating that Enders, Weller, and Robbins had been selected as recipients of the Nobel Prize in Physiology or Medicine. Western Union said that they would send me the telegram, but it never arrived and I have often wondered who, if anyone, has that piece of paper. Needless to say, the news was a shock but a pleasant one.

Alice was not home when Fred received the call. Upon her return the couple had to get organized quickly getting passports, buying clothes, and preparing his talk. The Robbins and Enders families traveled on the *Queen Elizabeth* to England and through the North Sea to Sweden. During the last leg of the trip, big storms and big waves made for a rough journey, which exhausted the travelers. The festivities in Stockholm, however, were rewarding to everybody.

THE FIRST CLEVELAND PERIOD: PROFESSOR THEN DEAN

Cleveland medical institutions recruited Fred Robbins and his colleague Bill Wallace in 1952. Fred became a professor and the head of pediatrics at City Hospital; Bill became a professor and the head of pediatrics at Children's Hospital, which is a part of University Hospitals of Cleveland. The arrangement then was that the leader of pediatrics at Children's also became the chair of the department at the Medical School. Fred was not happy with such arrangements, but since Bill Wallace was a good friend, the two worked well together. It is interesting to note that this arrangement has remained a cause of friction for the different Cleveland medical institutions to this day.

City Hospital at the time Fred joined its faculty was a major academic institution in Cleveland, with such distinguished academic figures as Charles Rammelkamp, Edward Mortimer, Fiorindo Simeone, and many others. A remarkable number of fellows and junior faculty trained under the guidance of Fred and his colleagues at Cleveland. His leadership supported the growth of the pediatric faculty in performing basic as well as clinical research. The hospital pioneered the first child life program in the country. It was Fred's personality and vision that created a nurturing atmosphere that motivated faculty, fellows, and residents to embark on careers in patient care and medical sciences. These were the great days of City Hospital, a part of the legends of the institution's history.

In 1966 Fred Robbins accepted the position of dean of the Medical School at what is now known as Case Western Reserve University. He immediately established a new way to lead multiple institutions that spent a lot of energy on controversies. His stature, personality, and vision guided the school at a difficult time of transition. The institution was at the tail end of the Joe Wearn era, which was epitomized by a generation of department chairs who developed a new curriculum and a nationally distinguished reputation. For several years Fred maintained the momentum, but by the mid-1970s it was clear that new blood and new leadership was acutely needed. He initiated a cycle that brought new chairs of medicine, surgery, family medicine, molecular biology, biochemistry, and pharmacology. They constituted the next wave of chairs that led the institutional revival and brought a new surge of energy, scientific excellence, educational commitment, and clinical acumen.

In the meantime, Fred's national and international reputation as a statesman of medical sciences was growing. His interest in polio was maintained all through his career. He chaired the Pan American Health Organization commission that was overseeing polio eradication in the continent. He was a happy person when the commission certified that polio had been eradicated in the Americas. It is unfortunate that the global polio eradication program is facing significant recent challenges in Africa and Asia. Fred would have liked to have been with us when the final curtain came down on poliomyelitis.

INSTITUTE OF MEDICINE: PRESIDENT

In 1980 following retirement from Case Western Reserve University at age 65, Dr. Robbins decided to accept the presidency of the Institute of Medicine, a part of the National Academies. His influence on the national and international scientific issues had been expanding. Fred had been a member of the institute, and he liked the organization and its people. While Fred was excited about the move to Washington, Alice was not. She was going to leave a much settled pattern of life in Cleveland, lots of friends, and lots of activities. But the two of them finally managed to enjoy the scientific and social scene in Washington. Fred's presidency was a period of reflection and positioning of the organization. His role also involved chairing of the Commission on Life Sciences of the National Research Council. He served as a member of the Executive Board of the Office of International Affairs of the National Academy of Sciences. He was involved in several other academy activities and participated actively in the functions of several of its boards. Fred testified before congressional committees and commanded the respect of the administration as well as legislative branches of the U.S. government. Fred's tenure at the Institute of Medicine was also marked by an appreciable increase in the organization's involvement in global health issues as well as an enhanced understanding of the disparities in health care in America.

THE RETURN TO CLEVELAND

Once Fred's tenure at the Institute of Medicine came to its end, he and Alice headed back home to Cleveland. The medical scene had been changing rapidly, but the place of the Robbinses in the hearts and minds of their colleagues and the community at large was well preserved. Fred elected to become emeritus professor in epidemiology despite my invitation to have him join the Department of Medicine. His vision, which he never shared with me openly, must have been to stay away from clinical departments that were involved in all sorts of competition among the multiple Cleveland medical institutions.

The "neutral" base he chose in epidemiology marked the beginning of another most productive and influential phase in his career. During the ensuing two decades, he initiated a university-wide center for international health, a center for adolescent health, and guided several exciting research areas. But perhaps the one with the most impact was his championing of the development of a scientific collaborative effort on HIV/AIDS between several institutions in Uganda and Case Western Reserve University. His visits to Uganda and meetings with the president of that country marked the beginning of a sustained and productive interaction. The unit that Fred initiated in 1985 is still a thriving research enterprise in Uganda and has resulted in the involvement of many U.S. academic institutions and several international organizations—a remarkable set of achievements in controlling HIV in that country.

PROFESSIONAL MEMBERSHIPS AND OTHER ACTIVITIES AND AWARDS

Fred Robbins was elected to many societies both in the United States and globally. He was a member of the American Academy of Arts and Sciences, American Philosophical Society, American Society for Clinical Investigations, Association of American Physicians (honorary member), and Infectious Diseases Society of America. Dr. Robbins was a member of the National Academy of Sciences as well as the Institute of Medicine. He served as president of Society for Pediatric Research; president, American Pediatric Society; chair, Technology Assessment Advisory Council, Office of Technology Assessment, Congress of the United States; chair, NASA Life Sciences Strategic Planning Study Committee; and chair, International Commission for the Certification of Poliomyelitis Eradication, Pan American Health Organization.

During his prominent service at national and international levels, Dr. Robbins consulted and advised multiple institutions, including the U.S. Public Health Service, U.S. Armed Forces Epidemiological Board, National Institutes of Health, Pan American Health Organization, Food and Drug Administration, NASA, and the World Health Organization.

Fred Robbins was honored by many national and international organizations, including the First Mead Johnson Award; Kimble Methodology Research Award; Abraham Flexner Award of the Association of American Medical Colleges; NASA Public Service Award; and the Benjamin Franklin Medal of the American Philosophical Society. He received honorary degrees from John Carroll University, the University of Missouri, University of New Mexico, University of North Carolina, Tufts University, Medical College of Ohio, Albert Einstein College of Medicine, Medical College of Wisconsin, Medical College of Pennsylvania, and Case Western Reserve University.

MENTOR AND FRIEND

I was introduced to Fred Robbins early in 1973 upon my arrival in Cleveland to begin postdoctoral training at Case Western Reserve University. He was the dean, Nobel Laureate, and scientific giant. In spite of the halo he demonstrated kindness and a light touch that made everyone around him at ease. He used to walk the corridors of the medical school, hospital, and research building and casually drop by just to keep in touch. Since my training was funded by a grant from the dean's office, I was convinced that he was making sure the funds were put to a task of his liking. As I gradually got acquainted with him, his skills and dedication to mentoring of students and faculty became obvious. Over the years his mentorship gradually matured into a special friendship of no equal. He exuded warmth and good sense and graciously provided advice. We stayed in touch while he served as president of the Institute of Medicine. His return to Case Western Reserve reflected the deep bond he had with that institution and its faculty and students. He instantly became the celebrated statesman he always was, but with a broad vision and remarkable appreciation of the new challenges facing academic medicine. He is deeply missed.

I WOULD LIKE TO ACKNOWLEDGE and express my appreciation and gratitude to Alice Robbins. She devoted a considerable amount of time, shared documents, and helped portray the life and achievements of Fred Robbins.

SELECTED BIBLIOGRAPHY

1943

With C. E. Dunlap. The effect of roentgen rays, radon and radioactive phosphorous on thiamin chloride. *Am. J. Roentgenol.* 50:641-647.

1946

With C. A. Ragan. Q fever in the Mediterranean area: Report of its occurrence in allied troops. I. Clinical features of the disease. *Am. J. Hyg.* 44:6-22.

1948

With J. E. Smadel and M. J. Snyder. Vaccination against Q fever. *Am. J. Hyg.* 47:71-81.

1949

With J. F. Enders and T. H. Weller. Cultivation of the Lansing strain of poliomyelitis virus in cultures of various human embryonic tissues. *Science* 109:85-87.

1950

With J. F. Enders. Tissue culture techniques in the study of animal viruses. *Am. J. Med. Sci.* 220:316-338.

With J. F. Enders and T. H. Weller. Cytopathogenic effect of poliomyelitis viruses *in vitro* on human embryonic tissues. *Proc. Soc. Exp. Biol. Med.* 75:370-374.

1952

With D. C. Gajdusek and M. L. Robbins. Diagnosis of herpes simplex infections by the complement fixation test. *JAMA* 149:235-240.

With T. H. Weller and J. F. Enders. Studies on the cultivation of poliomyelitis virus in tissue culture. II. The propagation of the poliomyelitis viruses in roller-tube cultures of various human tissues. *J. Immunol.* 69:673-694.

1958

With D. C. David, M. J. Lipson, D. H. Carter, and J. L. Melnick. The degree and duration of poliomyelitis virus excretion among vaccinated household contacts of clinical cases of poliomyelitis. *Pediatrics* 22:33.

1960

With M. L. Lepow and W. A. Woods. Influence of vaccination with formalin inactivated vaccine upon gastrointestinal infection with polioviruses. *Am. J. Public Health* 50:531-542.

With M. L. Lepow and N. Gray. Studies on attenuated measles-virus vaccine. V. Clinical, antigenic and prophylactic effects of vaccine in institutionalized and home-dwelling children. *New Engl. J. Med.* 363:170-173.

1961

With W. A. Woods. The elution properties of Type 1 polioviruses from $Al(OH)_3$Gel. A possible genetic attribute. *Proc. Natl. Acad. Sci. U. S. A.* 47:1501-1507.

1962

With M. L. Lepow, D. H. Carver, H. T. Wright, and W. A. Woods. A clinical, epidemiologic and laboratory investigation of aseptic meningitis during the four year period, 1955-1958. I. Observations concerning etiology and epidemiology. *New Engl. J. Med.* 266:1181-1187.

With R. W. Friedman, R. L. Kirschstein, and G. S. Borman. Characteristics of Sabin Type 1 poliovirus after gastrointestinal passage in new born infants. I. Monkey neurovirulence and temperature marker findings. *Am. J. Hyg.* 76:137-143.

1964

With R. J. Warren, M. L. Lepow, and G. E. Bartsch. The relationship of maternal antibody, breast feeding and age to the susceptibility of newborn infants to infection with attenuated polioviruses. *Pediatrics* 34:4-13.

1967

With N. Balassanian. Mycoplasma pneumoniae infection in families. *New Engl. J. Med.* 277:719-725.

1974

The long term effects of infection in early life (Long View II). Presidential address, American Pediatric Society, Washington D.C., May 1, 1974. *Pediatr. Res.* 8:972-977.

1981

With E. O. Nightingale. Saccharin and society. *Am. J. Med.* 71:9-12.

1982

With E. A. Mortimer Jr., M. L. Lepow, E. Gold, G. H. Burton, and J. F. Fraumeni Jr. Long-term follow-up of persons inadvertently inoculated with SV40 as neonates. *New Engl. J. Med.* 305:1517-1518.

1983

Prospects for worldwide control of measles. Discussion I. *Rev. Infect. Dis.* 5(5):957-968.

1986

Guest Lecture. The several faces of science. *Clin. Res.* 34(2):159-162.

1993

Eradication of polio in the Americas. *JAMA* 270(15):1857-1859.

1995

With E. A. Mortimer Jr. Vaccine licensure: a proposal to meet changing needs. *Ann. N.Y. Acad. Sci.* 754:368-376.

1997

With C. A. de Quadros. Certification of the eradication of indigenous transmission of wild poliovirus in the Americas. *J. Infect. Dis.* 175(suppl. 1):281-285.

Richard Evans Schultes

Biographical Memoirs V.88
http://www.nap.edu/catalog/11807.html

RICHARD EVANS SCHULTES

January 12, 1915–April 10, 2001

BY LUIS SEQUEIRA

THE SPEAKER JUST DID not look the part. He was tall, thin, clean-shaven with closely cropped hair, and wore a tweed coat and a Harvard tie. He spoke softly, with a clipped Boston accent, and peered at the students behind wire-rimmed glasses while he explained in a bemused tone the advantages of the use of snuff as a means to clear a stuffy nose. A highly conservative, proper Bostonian no doubt and about to deliver what we expected would be a scholarly, probably dull lecture on the taxonomy of some plant family. Yet, as he spoke, all the students in a course on economic botany at Harvard in the spring of 1949 became gradually transfixed when he began to describe some of his experiences while exploring the upper reaches of the Amazon River in Colombia. He seemed the most unlikely person to have survived alone for several years in one of the most remote areas of the world, where he faced incredibly harrowing, perilous conditions.

He had gone to the jungle in Colombia to trace the origin of curare in 1941, but remained there for the next eight years to collect wild specimens of the *Hevea* rubber tree as part of a mission for the U.S. government. What he told us was the stuff of fiction. There he was, accompanied only by a native guide, paddling in some strange tributary of the Amazon when he hit some rapids and, oops, the canoe overturned. He managed to scramble back into the canoe, but had lost his guide as well as all the equipment. Weakened by malaria and beriberi, he had to paddle for another 10 days before he could get help at some remote outpost.

There were numerous other stories concerning his life among the native people of the Amazon, partaking with them in their religious ceremonies and learning about the use of plants for their medicinal as well as hallucinogenic properties. He managed to collect more than 20,000 plant specimens, many new to science and many that would eventually bear his name when they were described. That he had done this alone and for so many years seemed incredible. Yet, he remained a quiet, reserved, modest man, apparently unaware at that time of the significance of his contributions to science. That was the Richard E. Schultes I met for the first time.

He had been invited to give a lecture in the course on one of his rare visits to Harvard after years of uninterrupted exploration of the upper Amazon in Colombia. I am certain that most of the students in that course had a hankering for plant exploration and admired the accounts of Humboldt, Spruce, Dampier, and other nineteenth-century explorers. However, few of us would have been willing to face the hardships associated with exploration of the immense Amazon Valley. As the inheritor of a grand tradition, however, Richard Schultes seemed the epitome of the plant explorer of the Victorian era. Most of the students realized on that day in 1949 that they were in the presence of an unusually brilliant, brave individual, who someday would become one of the most distinguished botanists of his generation. That, indeed, was a safe prediction.

I saw Richard again two years later, when I happened to be at the Inter-American Institute of Agricultural Sciences (today's CATIE) in Turrialba, Costa Rica, on a temporary assignment to do research for my Ph.D. thesis. Richard had come to inspect the rubber trees that had been planted there from seeds that he had collected in the Amazon. I accompanied him on several trips. Many of the trees were magnificent specimens, but those that were high yielding were not highly resistant to the South American leaf blight fungus (*Dothidella ulei*) as he had hoped. Grafting was being used as a means to combine the good qualities of different wild accessions, but Richard shared with me his deep concern that the U.S. Department of Agriculture was about to remove support for the work on *Hevea* rubber. The reason was that in the United States synthetic rubber had taken over the automobile tire industry and use of natural rubber seemed to be on the way out. In retrospect, the loss of that important activity in Turrialba was a tragedy, not only because it negated the important outcome of Richard's hard work of many years but also because it slowed progress of the potential development of a natural rubber industry in Latin America, which is only now being promoted again.

Richard contributed to the science of botany far more than the immense collection of plants, many of them new to science, that he managed to bring back to this country after a dozen years of exploration in the Amazon. He was the first to point out the close dependency on and intimate knowledge of plants by the native inhabitants of the tropical forests. For his own survival in the Amazon he depended on his ability to communicate with tribes that had never seen a white man before and he was able to obtain valuable knowledge of their use of native plants for food, medicine, and religious rites. Thus, he became the father of a new branch of science called "ethnobotany," the field that explores the relationship between indigenous people and their use of plants. Many of today's leading ethnobotanists obtained their Ph.D. degrees at Harvard under Richard's guidance. In addition, he was one of the first to decry government development programs that resulted in unrelenting loss of Amazon forests. He pointed out that the disappearance of native tribes, and their knowledge of plants in the forest was a more catastrophic consequence to the world than the loss of the trees to the ax of the developer. His compassion and deep respect for native cultures, their languages, habits, medicinal knowledge, beliefs, and their religious ceremonies became his trademark.

An unintended consequence of Richard's interest in the active principles of the hallucinogenic higher plants and mushrooms used by native tribes was the increased attention to his work by some of his colleagues who promoted the use of these drugs. In the 1960s Richard's name became associated with those of Timothy Leary, William Burroughs, Aldous Huxley, and others who were participants in the drug culture. Richard had nothing but disdain, however, for those who would use these drugs solely for entertainment and complained that they could not even spell the Latin names of the plants that produced the drugs. In fact, in his 1979 book with A. Hoffman, *Plants of the Gods*, the title page contains a caution: "This book is not intended as a guide to the use of hallucinogenic plants." In a review of the book, Lee Dembart of the *Los Angeles Times* concluded that "at a time of public hysteria over the use of drugs it takes guts to bring out this book, caution or no."

Richard Evans Schultes was born in Boston on January 12, 1915. His parents were working-class German immigrants; his father was a plumber who worked for a local brewery and his mother was a homemaker. They soon realized that their son was a gifted individual and exposed him to literature and scientific books from a very early age. When he was six years old, illness forced him to lie in bed for several months. During this period, his parents read to him from *Notes of a Botanist on the Amazon and the Andes*, a travel diary kept by the English naturalist, Richard Spruce, and published in 1908. The adventures described in this book made a strong impression on young Richard and years later he confided that they strongly influenced his decision to pursue a career in plant exploration.

Richard attended East Boston High School, where he was a distinguished student and earned a scholarship to Harvard University. He had originally intended to pursue a career in medicine, but soon changed his mind and returned to his earlier interest in botany. For his undergraduate honors dissertation he chose a study of the peyote, a hallucinogenic cactus used by Indian tribes in the western United States during ceremonies intended to commune with their ancestors. Young Richard spent a month with the Kiowa Indians of Oklahoma and gained new knowledge about the importance of peyote in their culture. When asked if he partook in the use of peyote during ceremonies, he replied, "It would have been unpardonable rudeness to refuse." Taking peyote with the Kiowa and listening to their stories during nightlong ceremonies, he came to understand the role of peyote in their lives.

That experience was the initial step in the route he would follow for the rest of his life: to investigate the role of plants in native cultures. It is not surprising that over the centuries, man selected many plants because they provided stimulation and a surreal sense of well-being to many who could not otherwise escape the harsh reality of day-to-day life. It has been so from the time man first inhabited this planet. We still consume huge amounts of caffeine, nicotine, and ethanol, as well as illegal drugs like *Cannabis*, coca, and many other narcotics in an effort to receive stimulation and/or escape reality. Richard believed that the use of peyote was a relatively harmless activity and, indeed, the U.S. government still allows its use by Western tribes for ceremonial purposes.

Richard went to graduate school at Harvard and came under the influence of Oakes Ames, a distinguished orchidologist and director of the Harvard Botanical Museum. Richard became an expert on orchid taxonomy, but for his dissertation he chose to search the botanical sources of *teonanacatl* and *ololiuqui*, hallucinogenic plants that were revered by the Aztecs. He traveled to Oaxaca in Mexico and together with local botanist Blas Pablo Reko visited a tribe of Mazatec Indians who used *teonanacatl* in their religious ceremonies. He discovered that the hallucinogenic plant was a mushroom, now known as *Panaeolus sphinctrinus*, described by native healers as "the little holy ones." A year later he was able to identify the morning glory, *Turbina corymbosa*, as the source of the even more potent hallucinogen. It was found to contain chemicals very close to LSD. Those reports marked the birth of the psychedelic era in the United States and the widespread use of "magic mushrooms," LSD, and the like, by Timothy Leary and his followers.

Soon after obtaining his Ph.D. degree in 1941 Richard accepted a fellowship to study the plant sources of curare and other arrow poisons used by native tribes in the Amazon Valley. On his first day after arriving in Bogota, Colombia, he discovered a new species of orchid that he proceeded to press and send on to Oakes Ames, who later names it *Pachyphyllum schultesi*. It was the fist of numerous plants named after their discoverer. Richard proceeded to the Colombian Amazon and soon established a simple modus operandi: a pith helmet, an aluminum canoe, a minimum of food and medical supplies, plant-pressing materials, and one change of clothes. He relied on the hospitality of local Indians and never carried a firearm. He did not believe that there were hostile Indians; "all that is required to bring out their gentlemanliness," he said, "is reciprocal gentlemanliness." After months of intensive, exhausting journeys in the Amazon forest, traveling alone or with native guides, he discovered that more than 70 species of plants produced arrow poisons. Indeed, curare sometimes was a mixture of 15 different ingredients. These were important findings, for curare has been used by hospitals in the United States and Europe as a muscle relaxant, and to this day it is frequently used during anesthesia and for the treatment of tetanus.

World War II broke out while Richard was in the Amazon in Colombia, and he promptly attempted to enlist in the U.S. Army at the American embassy in Bogota. The government had other plans for him, however. "You are not going back to the States; you are going right down into the Amazon and try to get the Indians to tap wild rubber." These were the orders given to young, intrepid Richard; he answered his government's call and soon he was promoting the tapping of the *Hevea* rubber trees in the wild. He ended up staying in the Amazon for the next 12 years. In the 1920s the natural rubber industry had moved to Malaysia and other countries in Southeast Asia when it became clear that *Hevea* could not be grown in extensive plantations because of a devastating fungus, the agent of South American leaf blight, *Dothidella ulei*. In the forest individual rubber trees grow at widely separate locations and the fungal parasite does not build up to epidemic proportions. No so when there are almost unlimited hosts in a homogenous plantation. The fungus does not exist in Asia. Thus, the industry moved there when the demand for natural rubber for automobile, tractor, and airplane tires surpassed the ability of native tappers in South America to provide it. World War II changed all that, however. By 1941 the Japanese had invaded Malaysia, and other Asian sources of rubber were gone. Thus, the need to promote again the exploitation of rubber trees in the wild.

Richard's responsibilities were not limited to encouraging the tapping of rubber trees in the wild. Far more important was the search for sources of *Hevea* that were resistant to the fungus and thus could be grown in plantations somewhere in Central or South America. He made a grueling series of journeys along the thousand miles of the Apaporis River, spotting all the rubber trees within a thousand yards of the riverbank. It was Richard's job to collect seeds from natural variants of *Hevea* and to ship them to the U.S. Department of Agriculture station at Turrialba, Costa Rica, where a team of scientists would test for disease resistance and rubber yield. It was there that I met Richard in 1951, when he came to review the work on the accessions he had provided, as I described previously.

Back at Harvard in 1953 he began the intensive work of identifying the multitude of plants he had collected in Colombia, published extensively about hallucinogenic plants, wrote several books about his experiences with native tribes in the Amazon, and taught a course in economic botany. Although not a charismatic teacher, he nonetheless became popular for his endless tales of exploration and his wickedly dry humor. (He professed to be a strong supporter of monarchy for the United States, and voted for the Queen of England at all presidential elections.) He frequently entertained students by demonstrating his prowess with a six-foot blowgun. Yet, he imbued his students with a deep sense of the fragility of the tropical forests and respect for the knowledge and traditions of their native inhabitants.

He eventually became professor of biology and director of the university's botanical museum. His laboratory became the center of ethnobotany in the United States, and he attracted numerous students who followed in his footsteps, among them Tim Plowman, who died in 1989, and Mark J. Plotkin and Wade Davis, well-known authors of bestselling books on the use of hallucinogenic plants by native people in the tropical forests. He became editor of the *Journal of Economic Botany* and continued to be an ardent critic of development programs for the Amazon basin.

Although he was a leader in conservation activities worldwide, he had a deep interest in the utilization of forest plants for commercial purposes. From time to time over the years I would participate with Richard in meetings designed to convince industry to invest in plantations of new sources of useful medicines, drugs, oils, insecticides, wood, rubber, food, and other products from tropical forests. He provided the basis for modern conservation programs that are supported today by large drug companies that obtain the rights to search for medicinal plants.

During his long and extraordinary career, Richard received numerous honors and awards. Perhaps no honor was as significant to him as the naming of a large section of the Colombian Amazon as the Schultes Preserve in recognition of his work on conservation. He received the Order of the Cross of Boyaca (the highest honor given by the Republic of Colombia). He became a member of the National Academy of Sciences (elected in 1971), the American Academy of Arts and Sciences, and the Linnean Society of London (recipient of its Gold Medal in 1992). Similar honors were bestowed by the academies of sciences of Colombia, Ecuador, and Argentina. He received the Gold Medal of the World Wildlife Fund, and the Society for Economic Botany provided an annual research award in his honor. Harvard named him the Edward C. Jeffrey Professor of Biology in 1980. He was the recipient of the Tyler Prize for Environmental Achievement in 1987.

In 1959 Richard married Dorothy Crawford McNeil, an opera soprano who performed in Europe and the United States. They had three children: Richard Evans Schultes II, a corporate executive; Alexandra Ames Schultes, a physician; and Neil Park Schultes, a molecular geneticist.

Richard Schultes retired from Harvard in 1985. A caring head of a distinguished family and a renowned plant scientist, Richard seemed oblivious of his many accomplishments throughout his life. He often described himself as "just a jungle botanist." His death in 2001 was a serious loss to the field of economic botany and the cause of preservation of tropical forests. It certainly deserved a much earlier account in the biographical memoirs of the National Academy of Sciences.

SOURCES OF INFORMATION used to summarize the distinguished career of Richard E. Schultes included obituaries published at the time of his death in *The Washington Post, The New York Times, The Guardian* (Manchester)*, Los Angeles Times, The Daily Telegraph, The Economist,* and *Erowid.*

SELECTED BIBLIOGRAPHY

1937

Peyote and the American Indian. *Nature* 30:155-157.

Peyote (*Lophophora williamsii*) and plants confused with it. *Bot. Mus. Leaflet* 5:61-88.

1940

Teonanacatl—the narcotic mushroom of the Aztecs. *Am. Anthropol.* 42:429-443.

1945

Glimpses of the little-known Apaporis River in Colombia. *Chron. Bot.* 9:123-127.

The genus *Hevea* in Colombia. *Bot. Mus. Leaflet* 12:1-32.

1947

Studies in the genus *Hevea*. I. The differentiation of *Hevea microphylla* and *H. minor*. *Bot. Mus. Leaflet* 13:1-11.

1949

The importance of plant classification in *Hevea*. *Econ. Bot.* 3:84-88.

1955

Twelve years in a green heaven. *Nat. Hist.* 64:120-127.

1956

The Amazon Indian and evolution in *Hevea* and related genera. *J. Arnold Arboretum* 37:123-148.

1960

Etymologists loose amongst the orchids. *Am. Orchid Soc. Bull.* 29:86-93.

1962

The role of the ethnobotanists in the search for new medicinal plants. *Lloydia* 25:257-262.

1966

The search for new natural hallucinogens. *Lloydia* 29:293-308.

1968

The plant kingdom and modern medicine. *Herbarist* 34:18-26.

1969

The unfolding panorama of the New World hallucinogens. In *Current Topics in Plant Science*, ed. J. E. Gunkell, pp. 336-364. New York: Academic Press.

1972

From witch doctor to modern medicine: Searching the American tropics for potentially new medicinal plants. *Arnoldia* 32:198-219.

1973

With A. Hoffman. *The Botany and Chemistry of Hallucinogens*. Springfield, Ill.: Charles C. Thomas.

Orchids and human affairs: What of the future? *Am. Orchid Soc. Bull.* 42:785-789.

1976

Rubber and man—a century of partnership. *Horticulture* 54:18-26.

1977

A new infrageneric classification of *Hevea*. *Bot. Mus. Leaflet* 25:243-257.

1978

Plants and plant constituents as mind-altering agents throughout history. In *Handbook of Psychopharmacology*, vol. 11, eds. L. L. Iversen, S. D. Iversen, and S. H. Snyder, pp. 219-241. New York: Plenum.

1979

The Amazonia as a source of new economic plants. *Econ. Bot.* 33:259-266.

With A. Hoffman. *Plants of the Gods*. New York: McGraw-Hill.

1982

With W. A. Davis. *The Glass Flowers at Harvard.* New York: E. P. Dutton.

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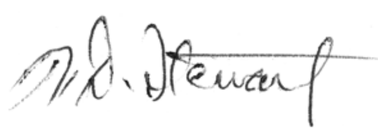

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THOMAS DALE STEWART

June 10, 1901–October 27, 1997

BY DOUGLAS H. UBELAKER

THOMAS DALE STEWART WAS A founder of modern forensic anthropology and a major contributor to most areas of human skeletal biology, paleopathology, and related areas of physical anthropology. His quiet and modest demeanor and meticulous approach to problem-oriented research made him one of the most respected and accomplished physical anthropologists on record. Although his interests touched many areas of anthropology and medical science, he is widely regarded as a champion of accurate, detailed scholarship and remarkable career productivity. His name will forever be linked to the development of modern forensic anthropology, focused research in paleopathology, and issues surrounding the peopling of the New World and the Shanidar Neanderthals.

Dale was the chair of my dissertation committee at the University of Kansas in the early 1970s and a mentor during my own academic formative years. We worked closely on research projects and forensic issues for many years. I was hired at the Smithsonian Institution when he retired in 1971, and we subsequently worked together for over two decades. I consider myself to be his student and, like all his friends and colleagues, gained tremendous respect for his work ethic, scholarly productivity, attention to detail, and amiable personality.

GROWING UP IN DELTA, PENNSYLVANIA

Stewart was born in the Welsh community of Delta, Pennsylvania, the son of Thomas Dale Stewart Sr. and Susan Price Stewart. Although his parents were not of Welsh ancestry, they had moved to Delta from Delaware at the encouragement of Dale's uncle, the Reverend Kensey Stewart, who was the minister in charge of the local Slateville Presbyterian Church. Reverend Stewart had noted that the town lacked a good pharmacy, and so Dale's pharmacist father decided to move there and set up what became known as Delta Pharmacy.

Dale attended the local Delta school system. The small one-building school carried students from first grade through high school, with two grades in each room. Dale felt this was helpful because it brought a preview of work to come and a review of work completed throughout the school experience. His primary school teacher Mary Arnold doubled as his Sunday school teacher and continued to assist him later in his career.

Following graduation from high school in 1920, Stewart accepted a position with the First National Bank of Delta as a runner; Dale had to open the bank in the mornings, maintain the furnace, and complete other mundane tasks needed to keep the facility operational. Within a year he became a bookkeeper, working with an adding machine to balance the books. Each day he had to go over the bank's transactions until every cent was accounted for, an experience in detail and accuracy that he felt offered good training for his later scientific career. His father died in 1916 following a stroke and Dale's deaf mother had become dependent on him, adding an incentive to maintain his new position in the local bank.

Dale was well on his way to becoming a small-town banker when family friend John L. Baer convinced him to enroll in George Washington University in Washington, D.C. Baer had worked in Delta Pharmacy, married Dale's former teacher Mary Arnold, and was living and working in Washington. In 1922 with his mother's approval, Dale rented a room from his old friends from Delta and launched his college career.

INTRODUCTION TO ANTHROPOLOGY AND THE SMITHSONIAN

At that time Mr. Baer held a temporary position with the Smithsonian Institution as a substitute for archeologist Neil Merton Judd and physical anthropologist Aleš Hrdlička when they were on travel. Through Baer, Stewart agreed to share his room with a young man who had just arrived to work at the Smithsonian, Henry B. Collins Jr., who recently had worked with Judd on an archeological project at Pueblo Bonito in New Mexico. The following year Collins departed for work in Mississippi, and again through Baer, Stewart's roommate became Karl Ruppert (a future Mayanist), who also was working for Judd at the Smithsonian. Through these three primary contacts, Stewart attended lectures, took classes in anthropology, and became familiar with anthropological activity at the Smithsonian.

In 1924 Baer was offered an opportunity to conduct research in Panama, and he asked Stewart if he would consider substituting for him on a temporary basis at the Smithsonian. At that time Baer was working as an assistant to the curator of physical anthropology, Aleš Hrdlička. In the employment interview with Hrdlička, Stewart discussed his previous banking experience. Hrdlička needed someone to tabulate numbers and he was impressed with Stewart's experience with the adding machine back in the Delta bank. Stewart accepted the position expecting it to end when Baer returned from Panama. When Baer died of yellow fever and malaria in Panama, Hrdlička invited Stewart to stay on, again on a temporary basis. After graduating from George Washington University in midyear 1927, he accepted a permanent job with the Smithsonian as an aid to Hrdlička.

Hrdlička admired Stewart's work ethic and indicated that if he would obtain his M.D. degree, he could eventually succeed Hrdlička as curator. With a leave of absence from Hrdlička and his guarantee that he would return to the Smithsonian after receiving the degree, Stewart enrolled in medical school at Johns Hopkins University in Baltimore. He had previously met Johns Hopkins professors Adolph Hans Schultz and William Louis Straus of the anatomy department.

With medical degree in hand in 1931, Stewart returned to the Smithsonian as assistant curator working with Hrdlička. He was promoted to associate curator in 1939. Following Hrdlička's death in 1943, Stewart became curator and continued at the Smithsonian for the remainder of his career, accepting appointments as head curator in 1960 and museum director in 1962. He retired in 1971 but continued work there until the 1990s.

RELATIONSHIP WITH HRDLIČKA

Dale Stewart loved to reminisce. During breaks on field expeditions or after dinner at his lovely home in McLean, Virginia, discussion would frequently shift to his years working with Aleš Hrdlička. Dale considered himself to be Hrdlička's primary student through his long apprentice arrangement at the Smithsonian. He credited Hrdlička with teaching him most of what he knew about physical anthropology, public speaking, scientific rigor, and the work ethic. Yet, he was mindful of Hrdlička's shortcomings and difficult personality and admitted that he was always on guard when working with the man.

Since both Hrdlička and Stewart held medical degrees, Smithsonian staff would frequently turn to them for help on medical issues. Two of Dale's stories relating to this medical experience reveal the delicate road he walked in his relationship with Hrdlička. Apparently Hrdlička maintained in his office a supply of citrine ointment that he would dispense to colleagues at the Smithsonian for a wide variety of minor ailments. One day Stewart was approached by a member of the department who had been a long-time user of Hrdlička's citrine ointment. She was in need of more ointment but had experienced a recent falling out with Hrdlička and did not feel comfortable approaching him with the request. In short she wanted Stewart to intervene and secretly obtain some of the valued ointment for her own use. Although Stewart was sympathetic to the woman, he turned her down, pointing out that if Hrdlička ever found out, his own relationship with him would be permanently damaged.

On another occasion in 1940 Stewart returned from lunch to learn that he had been requested to go to the office of a colleague in another department on a medical emergency. Both he and Hrdlička arrived at the same time to find the elderly colleague lying on the floor. Hrdlička immediately approached the patient while Stewart, mindful of Hrdlička's seniority, stood to the side. Hrdlička, perhaps influenced by his own recent experience with a heart attack, knelt down next to the man, listened to his heart, and quickly diagnosed that condition. Observing from a distance, Stewart gained a broader perspective, noting that one side of the face was sagging and the arm and leg on the same side were flaccid. Stewart could not hold back, and he approached Hrdlička pointing out his observations. Hrdlička turned and walked out, upset that he had been corrected by his student.

Although Stewart always referred to his years with Hrdlička as being an apprentice or assistant, the record shows that he also worked independently and productively during this period. From 1929 (date of his first publication) through 1943 (date of Hrdlička's death) Stewart authored 163 publications, or over 10 per year, and none coauthored with Hrdlička. This record is particularly impressive when one recognizes that in addition, Stewart provided support for most of Hrdlička's own voluminous publications. Following Hrdlička's death, Stewart maintained a remarkable publication rate, but somewhat reduced from his years with Hrdlička (about six per year from 1944 to 1971 and about three per year following his retirement).

RESEARCH DESIGN AND IMPACT

Throughout his career Stewart's research focused on a variety of specific issues that he would approach in original, detailed, and comprehensive ways. Stewart would define a specific problem that needed a solution, assemble all the relevant literature, and then craft a research design, frequently including the Smithsonian's collections. Early in his career, research topics complemented those of Hrdlička and his work at the Smithsonian: dental caries in Peru, separate neural arch and spondylolisthesis in Eskimo and other samples from the Americas, ossuary excavation and analysis in eastern North America, cranial deformation, dental alterations, anthropometry, and detailed analysis of specific skeletal samples recovered from archeological excavations.

Following Hrdlička's death, Dale's research expanded to include forensic topics, historical issues, and analysis of the fossils from Shanidar cave, Iraq. His forensic interests were shaped through casework with the FBI and concern with the science supporting human identification. Dale became a regular consultant in forensic anthropology for FBI Headquarters, then located just across Constitution Avenue from the Smithsonian's National Museum of Natural History. Between 1943 and 1969 he reported on at least 254 forensic cases for the FBI and others but had to testify in court on only eight occasions. Through casework Dale realized the need to improve scientific methodology. This concern led him not only to publish extensively on forensic issues but also to gather data from identified individuals, examined in a forensic context, to improve methodology. In 1954 he accepted an invitation from the secretary of the U.S. Army to work in Japan organizing an effort to gather data from Americans killed in the Korean Conflict. Stewart successfully argued that existing methodology had been developed primarily from the study of medical school collections, which tended to be composed mostly of the elderly. The military fatalities were primarily of the young and thus offered an opportunity to help remedy the problem. He also noted that most of the published literature offered only general statements about epiphyseal closure and other age changes, whereas his observations indicated that the issues were more complex stages of closure, and change needed to be recognized and calibrated especially in young adults. The resulting classic 1957 publication with Thomas R. McKern, *Skeletal Age Changes in Young American Males,* continues to be a primary source for the estimation of age at death.

Later in Dale's career his writing turned more toward synthetic works, bringing together his own research and that of others to focus on topics of considerable scientific interest. Edited volumes focused on human identification issues (with Mildred Trotter in 1954 and his own *Personal*

Identification in Mass Disasters in 1970) and included the 1970 *Handbook of Middle American Indians* and his revision of Hrdlička's *Practical Anthropometry*. His 1973 *People of America* volume presented a unique synthesis of the physical characteristics and historical issues of populations from the Americas. His *Essentials of Forensic Anthropology* (1979) continues to be a frequently cited, classic overview of an application of physical anthropology that he helped to shape.

Following his retirement in 1971, Dale continued to quietly conduct research and publish at the Smithsonian. Free from administration and other Smithsonian duties, Dale came to the office regularly and continued productivity for another two decades. Although he always modestly cited Hrdlička as a model of career research accomplishment, the number of his own publications surpassed that of Hrdlička, totaling at least 394 contributions, including five edited volumes and four books or monographs. He published his final monograph, *Archeological Exploration of Patawomeke*, in 1992 at the age of 91.

His work in paleoanthropology included detailed study of the Neanderthal material from Shanidar cave in Iraq and late Paleolithic remains from Egypt. Stewart's contributions included perspectives on cleaning and casting techniques, dating issues, taxonomy, and detailed anatomical documentation and interpretation.

His scientific record is especially impressive in consideration of his commitment to administration and contributions to organizations. In addition to the routine museum curatorial duties, Dale served as head curator in the Smithsonian's Department of Anthropology from 1961 to 1962 and director of the National Museum of Natural History from 1962 to 1965. In 1964 he was largely responsible for the production of a major Smithsonian exhibition on physical anthropology, which presented aspects of human variation and evolution, including many personal research interests of Dale's. He taught at the Washington University School of Medicine in St. Louis in 1943, at the Escuela Nacional de Antropologia in Mexico City in 1945 and at the George Washington University School of Medicine in Washington, D.C., from 1958 to 1967.

Stewart was elected to the National Academy of Sciences in 1962. He also was a member and president of the Anthropological Society of Washington, a vice-president of the Washington Academy of Sciences, president of the American Institute of Human Paleontology, a fellow of the American Anthropological Association, and an active member of the Committee on Research and Exploration of the National Geographic Society. He was president of the American Association of Physical Anthropologists from 1950 to 1952 and its treasurer-secretary from 1960 to 1964. Stewart received the Viking Fund Medal in 1953 and the Charles Darwin Lifetime Achievement Award in 1993 from the American Association of Physical Anthropologists.

In 1974 he was elected an honorary member of the American Academy of Forensic Sciences, and he regularly attended their annual meetings. Stewart could usually be found at those meetings sitting attentively in the first row of the physical anthropology section. In 1978 he accepted an appointment as a consultant to the American Board of Forensic Anthropology Inc., a newly founded organization offering certification and recognition to forensic anthropologists. In 1981 he became the second recipient (following Ellis R. Kerley) of the Physical Anthropology section award. This award was renamed the T. Dale Stewart Award in 1987 and represents the highest award offered by the section of Physical Anthropology for career achievement in physical anthropology. Between 1987 and 2005, 13 forensic anthropologists have received this award.

PROFESSIONAL QUALITIES

Growing up in Delta, Pennsylvania, Dale developed an interest in local history, searching for Indian artifacts along the Susquehanna River. This interest grew when he was exposed to anthropology in college and at the Smithsonian Institution. In the academic environment of the Smithsonian he flourished. With a work ethic that had been well established early in his life and honed during his long years working with Aleš Hrdlička, Stewart became a prodigious researcher who focused on accuracy and detail. Through extensive national and international travel, he was recognized throughout the academic world as the authority in his field. Soft-spoken and modest, Dale quietly shaped the academic development of his areas of interest.

Although he worked long hours even after retirement from the Smithsonian, Dale always took time for family and friends. He married Julia C. Wright in 1932. Following her death in 1951, he married Rita Frame Dewey in 1952. He had one daughter, Cornelia Gill, and numerous grandchildren and great grandchildren.

Dale played the piano and became an amateur portrait painter. He was a gracious host and enjoyed interaction with younger scholars and students. I remember many pleasurable evenings at Dale's home in McLean, Virginia, when after dinner we would retire to the living room and his stories would emerge: working with Hrdlička, ship travel to Alaska, meetings in Europe, and the complexities of various colleagues. This delightful person touched the lives of many anthropologists and others around him.

HIS LEGACY

Stewart led the professional development of forensic anthropology, paleopathology, and related areas of human skeletal biology. Through example in lectures, publications, and editing, he set the standard for scientific conduct in his areas of interest. A frequent international traveler, he was a diplomat for the Smithsonian Institution and American physical anthropology. His scientific contributions include original, detailed, and interpretive analyses of such diverse topics and conditions as treponemal disease, trephination, dental alterations, vertebral osteophytosis, anterior femoral curvature, Neanderthal morphology, and the complexity of skeletal aging.

MUCH OF THE INFORMATION presented here results from many conversations I enjoyed over the years with Dale and Rita Stewart and materials assembled for a symposium held in Dale's honor at the 51st annual meeting of the American Academy of Forensic Sciences in Orlando, Florida, on February 19, 1999, and published in the *Journal of Forensic Sciences* in March 2000.

SELECTED BIBLIOGRAPHY

1935

Spondylolisthesis without separate neural arch (Pseudospondylolisthesis of Junghanns). *J. Bone Joint Surg.* 17(3):640-648.

1940

The life and writings of Dr. Aleš Hrdlička (1869-1939). *Am. J. Phys. Anthropol.* 26:3-40.

1941

New examples of tooth mutilation from Middle America. *Am. J. Phys. Anthropol.* 28(1):117-124.

1949

Comparisons between Tepexpan man and other early Americans. In *Tepexpan Man*, vol. 11, eds. H. De Terra, J. Romero, and T. D. Stewart, pp. 137-145. New York: Viking Fund Publications in Anthropology.

1951

Scientific responsibility. *Am. J. Phys. Anthropol.* 9(1):1-4.
What the bones tell. *FBI Law Enforc. Bull.* 20(2)1-5.

1952

With A. Spoehr. Evidence on the paleopathology of yaws. *Bull. Hist. Med.* 29(6):538-553.

1953

The age incidence of neural-arch defects in Alaskan natives, considered from the standpoint of etiology. *J. Bone Joint Surg.* 35A(4):937-950.

1954

Metamorphosis of the joints of the sternum in relation to age changes in other bones. *Am. J. Phys. Anthropol.* 12(4):519-535.

With M. Trotter, eds. *Basic Readings on the Identification of Human Skeletons: Estimation of Age*. New York: Wenner-Gren Foundation for Anthropological Research.

1956

Examination of the possibility that certain skeletal characters predispose to defects in the lumbar neural arches. *Clin. Orthop.* 8:44-60.

1957

With T. W. McKern. Skeletal age changes in young American males. Rept. no. EP-45, May. Natick, Mass.: Quartermaster Research and Development Center, Environmental Protection Research Division.

1958

Rate of development of vertebral osteoarthritis in American whites and its significance in skeletal age identification. *Leech* 28(3-5):144-151.

1960

A physical anthropologist's view of the peopling of the New World. *Southwest J. Anthrop.* 16(3):259-273.

1962

Anterior femoral curvature: Its utility for race identification. *Hum. Biol.* 34(1):49-62.

1969

With L. G. Quade. Lesions of the frontal bone in American Indians. *Am. J. Phys. Anthropol.* 30(1):89-109.

1970

Ed. *Personal Identification in Mass Disasters*. Washington, D.C.: Smithsonian Institution.

Ed. *Handbook of Middle American Indians*, vol. 9. Austin: University of Texas Press.

1973

The People of America. New York: Charles Scribner's Sons.

1977

The Neanderthal skeletal remains from Shanidar cave, Iraq: A summary of findings to date. *Proc. Am. Philos. Soc*. 121(2):121-165.

1979

Essentials of Forensic Anthropology, Especially as Developed in the United States. Springfield, Ill.: Thomas.

1980

Responses of the human skeleton to changes in the quality of life. *J. Forensic Sci.* 25(4):912-921.

1981

The evolutionary status of the first Americans. *Am. J. Phys. Anthropol.* 56(4):461-466.

1983

The points of attachment of the palpebral ligaments: Their use in facial reconstructions on the skull. *J. Forensic Sci.* 28(4):858-863.

1992

Archaeological Exploration of Patawomeke: The Indian Town Site (44St2) Ancestral to the One (44St1) Visited in 1608 by Captain John Smith. Smithsonian Contributions to Anthropology, no. 36. Washington, D.C.: Smithsonian Institution Press.

VLADIMIR KOSMA ZWORYKIN

July 30, 1889—July 29, 1982

BY JAN RAJCHMAN

TELEVISION AND ELECTRONICS PIONEER Vladimir K. Zworykin died at the Princeton Medical Center on July 29, 1982, one day short of his ninety-third birthday. His inventions of the tubes for image pickup and display provided the keys to television. He was a prolific inventor, an inspired leader of research, and one of the most illustrious innovators of the twentieth century.

Vladimir Zworykin was born in the town of Mourom in Russia, where his father owned and operated a fleet of steamships on the Oka River. Vladimir studied electrical engineering at the Petrograd Institute of Technology, the elite technical center in tsarist Russia, and graduated in 1912. There he worked with Professor Boris Rosing, who already in 1906 was interested in television and believed it would become practical through the use of cathode-ray tubes, rather than the mechanical systems that were being proposed at the time. Zworykin credits Professor Rosing for his decision to become a scientist and innovator and in particular for his interest in developing television by the new techniques that came to be known years later as electronics.

In 1912 Vladimir Zworykin entered the prestigious College de France in Paris, where he engaged in X-ray diffraction research under Professor Paul Langevin. His studies were interrupted in 1914 by the outbreak of World War I. He returned to Russia and served as an officer in the Russian Army Signal Corps, mostly in radio communications. The Bolshevik Revolution forced him to flee his native country. He made two trips around the world before settling in the United States in 1919. After some odd jobs, such as bookkeeping in the Russian Embassy, he finally joined the staff of Westinghouse Company in Pittsburgh in 1920. He became a U.S. citizen in 1924, the earliest date at which this was possible.

At Westinghouse he worked first on photocells, which at that time were not very sensitive and could not be made uniformly. He greatly improved the method of sensitization using alkali metals. In 1926 he obtained a Ph.D. from the University of Pittsburgh for his work in this field; he also coauthored a book on the subject. But his main interest was television. For years television systems had operated by scanning successive picture elements by means of rotating discs or drums that exposed one element at a time to a photosensitive cell. These systems were very cumbersome, but worse, they worked only for extremely bright scenes, as only the light from the element being scanned was used, while the enormously greater light from all other elements was wasted.

This last question intrigued Zworykin for some years and he finally invented a way to capture all the light from all elements of the frame, rather than just that from the element being scanned, and thereby provided for the first time a means for a viable television system. His idea was to have a vast number of tiny photocells, a "mosaic" of photoemitting spots, on an insulating sheet such as mica. Actually the spots were tiny islands of silver sensitized by the method he had developed for macroscopic photocells. When the scene to be transmitted is focused on the mosaic, each element loses electrons and thereby acquires a positive potential. An electron beam scans the mosaic, and each element when bombarded by electrons, is successively discharged. The amount of discharge, equal to the charge accumulated, is therefore proportional to the light intensity on the element. The discharge produces the video signal through capacitive coupling to a plate backing the mosaic of elements. Each element stores the charge being produced in accordance with its illumination during the frame time (i.e., between one scan and the next). Hence all light on all elements is used. The video signal in this storing system is what would be obtained in a nonstoring system multiplied by an enormous factor, in practice a large fraction of the number of picture elements. This is the principle of the justifiably famous "television eye" or pickup tube that Zworykin named the "iconoscope," from the Greek *eikon* (image) and *skopon* (to watch).

Zworykin demonstrated the iconoscope in 1924 at Westinghouse, but the executives were not impressed. In 1929 he demonstrated an electronic television system that used an improved iconoscope and a refined all-vacuum cathode-ray tube as a viewing device. Still he could not convince Westinghouse to pursue television. As it happens, at that time a number of engineers, including Zworykin, were transferred from Westinghouse to RCA. In a first meeting with David Sarnoff, a fellow Russian immigrant who headed RCA, Zworykin was asked how much it would cost to perfect his television system, and he replied, "About $100,000." This meeting became a legend. David Sarnoff was never tired of relating that RCA spent $50 million before ever earning a penny from television.

At RCA, Zworykin had at last the means to develop a practical television system. Sarnoff, who presided over the creation of radio broadcasting, was convinced that television would be even more beneficial to humanity than radio and, of course, more profitable. He had the vision and the power to insure ample support for a research group, headed by Zworykin, to develop the basic electronics for picking up the image and for a receiver to display it, as well as for other groups to develop the other parts needed to create an entire television system, such as transmitters, antennas, system standards, and studios. Sarnoff never wavered in his determination to bring television into being. Television owes much to the fortunate team of Zworykin, with his technical vision, and Sarnoff, with his business acumen.

Soon after arriving at RCA, Zworykin was able to assemble a remarkable group of collaborators. Under his leadership this group developed the basics of the all-electronic television as we know it today. The iconoscope for image pickup and the kinescope, as Zworykin termed the cathode-ray tube, for display were developed into practical devices. Both depended on the precise control of electron trajectories so that electron optics became a main preoccupation of the group. Electron guns were developed for producing intense beams focused to very small spots. The complex details of electron trajectories in the iconoscope were unraveled and improved tubes were made. Cathodoluminescent materials producing a much brighter white image replaced the willemite that produced relatively dim green images on the face of the original kinescopes. The group also developed the electronic circuits for the whole television system, for amplifying weak signals at frequencies higher than possible hitherto, for producing the desired number of scanning lines for synchronizing deflection of the beams at the transmitting and receiving ends, and so forth. Many ingenious circuits were invented that involved basic new principles. By 1936 RCA was able to field test the whole system.

In April 1939 Sarnoff announced at the New York World's Fair that RCA was establishing a regular TV broadcast service. In 1940 the present television standards were adopted,

By the mid-1930s Zworykin's laboratory with its mastery of electron optics and clever pulse circuitry had in effect created a new technology, that of electronics. Zworykin was already dreaming of new applications of this new tool. When I had the good fortune of joining his laboratory in January 1936, he asked me, "What do you want to work on?" and to my reply, "television," he said, "You are too late!" Of course he knew this was an exaggeration, as many problems remained in television, and his laboratory made many new fundamental contributions to that art. He asked me to work on photomultipliers and eventually together with a colleague I developed a type still made today. Zworykin's real interest, however, was the electron microscope.

Early models of electron microscopes had already shown a much higher resolution than is fundamentally possible with light microscopes. Unfortunately, these laboratory instruments were totally unsuited for practical use. Zworykin, intrigued by the idea of the electron microscope for years, realized the enormous impact on mankind's world that would result from the real capability to see objects orders of magnitude smaller than was possible hitherto. He set his laboratory to the task of developing a practical instrument, and he engaged a young Canadian, James Hillier, who had already participated in the building of an electron microscope in Toronto. Hillier succeeded in demonstrating a working model in a very short time, and eventually, with the collaboration of several members of Zworykin's laboratory, he developed a very practical instrument that RCA developed into a product. This achievement involved the design of highly symmetric magnetic lenses; extremely stable high-voltage supplies; foolproof means for inserting the specimen in high vacuum; and the solving of many other difficult problems. Eventually, the laboratory became a center for biologists eager to use the new instrument and to learn the new techniques it demanded, such as making very thin specimens. Shortly thereafter the scanning microscope was developed, which enabled the viewing of surfaces of solids, and it became of great interest to materials scientists such as metallurgists. The pioneer work of Zworykin's laboratory was taken up by many groups the world over and, indeed, had the impact he foresaw; the ability to probe at angstrom scale literally transformed biology and materials science. Such developments as modern semiconductor integrated circuits would have been impossible without electron microscopy.

During World War II, much of the electron optics expertise was devoted to infrared image tubes—the snooperscope and the sniperscope—that enabled one to see in the dark. Zworykin's invention of an airborne television system to guide radio-controlled torpedoes was brought to fruition and was used before V-J Day. On a more humanitarian side, which always appealed to Zworykin, a device to help the blind to read was developed, though fortunately there were fewer blind victims of the war than anticipated. The war days also brought the laboratory to pioneer in computers.

As early as 1939 the U.S. Army was concerned about the slowness in the control of antiaircraft guns, as the Nazis then had superiority in the air, and asked RCA whether electronics could remedy this inadequacy. Zworykin thought this quite possible and assigned me to the job. In a few months I was joined by others, including Arthur W. Vance. We developed analog-type computers that saw action in the war. More significantly for the future, we pioneered in digital techniques with such devices as registers, counters, arithmetic units, read-only memories, and input and output devices. It soon became apparent that digital techniques could be applied to a more critical problem: that of speeding up the computation of ballistic tables that were then laboriously obtained by many human operators aided only by slow mechanical calculators. The task was eventually undertaken by a group at the University of Pennsylvania and resulted in the first modern electronic computer, the ENIAC. Many of RCA's pioneer techniques were used in that project, in which we collaborated in its initial stages.

John von Neumann, who started the development of such a computer at the Institute for Advanced Studies in Princeton, New Jersey, championed the concept of the stored-program computer, which evolved in that project. He asked Zworykin's laboratory to undertake the development of the necessary memory, and the task fell on me. I developed the first truly digital random-access memory, the selectron tube, which was an integrated vacuum tube as it would be called today, and also independently conceived and developed the core memory. Novel magnetic devices and magnetic logic were also developed. Zworykin's early foresight in 1939 of the potential of electronic computers was a driving force behind the basic contributions that his laboratory was able to pioneer. In the mid-1940s, when there was a struggle to achieve any working models aimed at solving strictly mathematical problems, Zworykin had the vision of the universality of the computer and in particular its use for weather prediction, now a daily routine, and for medical diagnosis, now reaching a clinical stage.

Fundamental advances in television continued to be made in Zworykin's laboratory while the new fields of electron microscopy and computers were being pioneered. The shading due to uncontrollable redistribution of secondary electrons was a basic difficulty in the iconoscope. The orthicon conceived by Albert Rose in 1937 avoided that problem altogether by using low-velocity scanning. The image orthicon was developed shortly thereafter and provided a tube of great sensitivity. Another pickup tube, the vidicon, developed in 1950 by Paul Weimer, uses photoconductivity rather than photoemission. This tube has great sensitivity and can be made in a small size suitable for portable TV cameras. Storage tubes capable of retaining one frame of television opened a variety of other new applications. Many improvements in television circuits were made as well.

The kinescope was improved radically by covering the screen with a thin aluminum coating that permitted the use of very high voltages and also reflects the backlight otherwise wasted. This technique, with improved cathodoluminescent materials, provided tubes that could be viewed comfortably in ordinary ambient light and permitted the making of high-intensity tubes for projection.

The most spectacular advance, however, was the color cathode-ray tube. In the mid-1940s, mostly at Sarnoff's prompting, RCA embarked on the development of color television. Among the many very difficult problems was that of developing the color display itself, a problem that was assigned to a task force. Many viable solutions were proposed. Eventually, the shadow mask tube, developed in Zworykin's laboratory by Alfred Schroeder, proved to be the solution. Harold B. Law invented very clever methods for making the tube, whose principle had been proposed some years ago. Mass production of color cathode-ray tubes is one of the production marvels of our age. The perspective of years makes apparent the great significance of the cathode-ray tube, which owes so much to the early work of Zworykin at Westinghouse—with high-vacuum rather than gas-filled tubes and electrostatically rather than magnetically focused guns—and mostly due to his leadership at

RCA, leading to advances in guns, cathodoluminescent materials, and screen construction. Fast addressing and efficient light production are obtained with ease and elegance, and as it turned out, color is attained by a radical yet natural extension. Despite efforts of many over many years, no alternative display has challenged the cathode-ray tube, which remains the key to television and computer terminals—two hallmarks of our age.

In his constant quest for broader applications of electronics to serve people's needs, Zworykin saw medicine as a prime fertile field. Typically he was not content to generalize but brought to fruition a number of concrete devices, such as an ultraviolet translating microscope; a radio endosonde (a tiny radio transmitter that when swallowed, could signal any desired internal condition, such as temperature or acidity); a cane with an ultrasonic radar to help the blind avoid obstacles; a quick method for measuring white corpuscles in the blood; and an electronic personal card to keep medical records. He realized that regardless of the number of ingenious devices a brilliant electronic researcher like himself might propose, an intimate working relationship with biologists and medical practitioners was indispensable for finding and developing the many potential possibilities. He became a champion of interdisciplinary research. As its main mentor, he was the chairman of the Professional Group for Medical Electronics of the International Federation for Medical Electronics and Biological Engineering. Many young pioneers were inspired by these institutions and are responsible for the varied electronic instruments that help to give us healthier and longer lives.

Another Zworykin interest was the automatic driving of cars. Annoyed at the boredom of driving on turnpikes and its consequent dangers, he thought the traffic sufficiently orderly to be amenable to automation. In cooperation with

General Motors, several cars were equipped with sensing and control devices, and automatic driving was demonstrated on a test loop at RCA Laboratories in Princeton. While the idea turned out to be too difficult to apply broadly, it had a useful by-product: the sensing loops buried in highways at intersections to control traffic lights according to traffic needs.

Ever since Zworykin joined RCA he was able to assemble, nurture, and direct a group of remarkable collaborators, many of whom gained international reputations for their own contributions. This may well be due to Zworykin's uncanny intuition in discovering latent talent in young recruits and his leadership, which provided a unique and superb training unimaginable elsewhere. Overflowing with imaginative ideas, Zworykin radiated enthusiasm and utter confidence in the workability of his proposals even though they were often based on the latest scientific or technological advances and were still full of unknowns. Zworykin (or "the Doctor," as he was known in his group) had an intense personal interest in every project. In his frequent—almost daily—visits to every researcher, he invariably asked, "What's new?" and then grasped every detail, often embroidering on what he heard with realism and imagination. When difficulties were identified, he would often suggest a solution. In any case he urged one to continue, and I remember he used to say, "One cannot stumble on an idea unless one is running." As most great minds, in complex situations he was able to pinpoint the central issue and make it a clear and simple concept. There was also a great deal of healthy skepticism and plain common sense in the Doctor's reasoning. He used to say, "It stands to reason that . . . ," which turned out to be a sure guide in some of the esoteric terrain we were exploring. To most, particularly the young, he inspired great self-confidence. He took it for granted that everyone was as knowledgeable as he was in the latest advances of any matter he was discussing. As a result, most of us had no hesitation in plunging into new fields and often managed to make contributions in a short time. There is no doubt that the Doctor inspired us to do better than our best. One of his collaborators used to say that only the impossible is a little difficult. The Doctor was an inspiring leader who brought all his energies toward the accomplishment of his laboratory. He kept administrative matters to an absolute minimum—sometimes even below that level—and he was greatly impatient with bureaucracy.

Zworykin's visions for an innovation were not limited to a key invention however brilliant or elegant. They included all the elements necessary for bringing it to fruition. Above all, he envisaged the betterment for humanity that would result from his idea, and he expressed it in such a way that a businessperson could be convinced of its utility and profitability. The doubting technical person was soon convinced by a direct experiment proving the most critical point. Zworykin was not a gadgeteer but an innovator of great breadth who saw his ideas in the grand perspective of humankind's progress.

When Zworykin joined RCA in 1929, he became the director of the Electronic Research Laboratory in Camden, New Jersey, which later also included a group in Harrison, New Jersey. In 1942 both groups were united in Princeton when most RCA laboratories moved to the new facility at that location. In that year he also became an associate director of RCA Laboratories. In 1947 he was named vice-president and technical consultant to RCA and continued to direct his laboratory. On August 1, 1954, he retired and was named honorary vice-president of RCA. He was the only one ever to hold that distinction and continued as a consultant to RCA for many years.

After his formal retirement, he continued to be very active. He pursued his passionate interest in medical electronics. He directed a medical electronics center at the Rockefeller Institute in New York. As a visiting professor for the Center for Theoretical Studies and Molecular and Cellular Evolution at the University of Miami (Florida), he directed the work of doctoral candidates. In addition, he lectured in various countries on the merits of electronic medical research. He never ceased to have imaginative proposals in medical electronics and in such fields as nuclear power generation. At age 91 he still drove to his office at RCA Laboratories to read his large collection of scientific journals.

Zworykin was the author or coauthor of more than 100 technical papers as well as 5 technical books. Among these, the book on television (1940), in its second edition, and the book on electron optics and the electron microscope (1945) are classics still widely read today. Zworykin was granted, as inventor or coinventor, more than 120 U.S. patents.

Dr. Vladimir K. Zworykin was the recipient of 29 major awards:

1934 Morris Liebmann Memorial Prize of the Institute of Radio Engineers
1938 Honorary degree of doctor of science, Polytechnic Institute of Brooklyn
1939 Overseas Award from the British Institution of Electrical Engineers
Modern Pioneer Award from the National Association of Manufacturers
1941 Rumford Medal of the American Academy of Arts and Sciences
1945 War Department Certificate of Appreciation

1947 Navy Certificate of Commendation
The Howard N. Potts Medal of the Franklin Institute
1948 Presidential Certificate of Merit
Chevalier of the French Legion of Honor
1949 Lamme Medal of the American Institute of Electrical Engineers
Gold Medal of Achievement, Poor Richard Club
1951 Progress Medal Award of the Society of Motion Picture and Television Engineers
Medal of Honor, Institute of Radio Engineers
1952 Edison Medal, American Institute of Electrical Engineers
1954 Medaille d'Or, L'Union Française des Inventeurs
1959 Trasenster Medal, University of Liege
Christoforo Colombo Award
Order of Merit, Italian government
1960 Broadcast Pioneers Award
1963 Medical Electronics Medal, University of Liege
Albert Sauveur Award, American Society of Metals
1965 Faraday Medal of the British Institution of Electrical Engineers
1966 De Forest Audion Award
National Medal of Science
1967 Golden Plate Award of the American Academy of Achievement
1968 Founders Medal of the National Academy of Engineering
1977 Installation in National Inventors Hall of Fame
1980 Ring from Eduard Rhein Foundation

Among these awards the most prestigious is the National Medal of Science—the highest scientific honor in the United States—which President Lyndon Johnson presented to Zworykin in 1966 "for major contributions to the instruments of science, engineering, and television, and for his stimulation of the applications of engineering to medicine." Zworykin was proud and very appreciative of all the awards and honors that were bestowed on him. However, as he once told me, he valued most his first award, which he received while still relatively young and unknown: the Morris Liebmann Memorial Prize "for important technologies recognized within recent years" and given traditionally to young contributors. When the Institute of Electrical and Electronics Engineers established a Vladimir K. Zworykin Prize Award in 1950 "for the most important contributions to electronic television," Zworykin insisted on the stipulation that it be given to the young.

Vladimir K. Zworykin was a member of 21 scientific and technical societies, including The National Academy of Sciences, to which he was elected in 1943. In most of them he had a senior or highly honorary status. These are:

American Academy of Arts and Sciences (member)
American Association for the Advancement of Science (fellow)
American Institute of Physics (fellow)
American Philosophical Society (member)
British Institution of Radio Engineers (honorary member)
Electron Microscope Society of America (charter member)
Eta Kappa Nu Association (eminent member)
French Ministry of Education (officer of the academy)
Institute of Electrical and Electronics Engineers (life fellow)
National Academy of Engineering (member)
National Academy of Sciences (member)
New York Academy of Sciences (member)
Sigma Xi (member)
Société Française des Electroniciens et Radioélectriciens (honorary member)
Society of Motion Picture and Television Engineers (honorary member)
Society of Television Engineers (charter member)
Society of Television Pioneers (charter member)
Television Engineers of Japan (honorary member)
Television Society (England) (honorary fellow)

Dr. Zworykin was elected a member of the American Philosophical Society in 1948. The nominating paper says of him: "His invention of the iconoscope has been a major factor in the development of television. He has made notable contributions to the development of the electron microscope and to war applications of electron optics." He read two papers to the society: "An Electronic Reading Aid to the Blind" with L. R. Flory in 1946 and "Medical Electronics: Promise and Challenge" in 1960.

Beyond his passion for electronics, Zworykin had a wide interest in other sciences, particularly in biology, meteorology, and nuclear physics. He had an insatiable curiosity for all natural phenomena. He was also interested in philosophy and current affairs. He had many friends and acquaintances in these various fields, and he liked to have lengthy discussions with them. This was greatly facilitated by his warm and generous hospitality. He had frequent social gatherings and dinners that included the most knowledgeable and brilliant personalities in practically every endeavor, which were hosted by his charming wife, Katyusha. Zworykin was unusually robust and healthy until his last years. Believing that *mens sana in corpore sano* (a healthy mind in a healthy body), he engaged in physical activities that included swimming, which he indulged in at his home in Taunton Lakes, New Jersey, and later on Hibiscus Island at Miami, Florida. He was also a regular walker. Zworykin loved to hunt, perhaps because in his childhood he participated in rather wild hunting expeditions that he relished in recounting. He always had a dog to which he was highly devoted.

Vladimir K. Zworykin is survived by his second wife, the former Katherine Polevitzky whom he married in 1951; Elaine Zworykin Knudsen from Pasadena, California, a daughter from his first marriage; and seven grandchildren.

Zworykin was an innovator who changed our lives as profoundly as did Edison, Bell, and Marconi.

NOTE

Dr. Alexander B. Magoun and Dr. George Cody provided editorial assistance in preparing this memoir for publication.

SELECTED BIBLIOGRAPHY

1929

Facsimile picture transmission. *Proc. I.R.E.* 17:895-898.

Television with cathode-ray tube for receiver. *Radio Eng.* 9:38-41.

1930

With E. D. Wilson. *Photocells and Their Application.* New York: John Wiley.

1933

Description of an experimental television system and the kinescope. *Proc. I.R.E.* 21:1655-1673.

1934

The iconoscope—A modern version of the electric eye. *Proc. I.R.E.* 22:16-32.

1936

With G. A. Morton and L. Malter. The secondary emission multiplier—A new electronic device. *Proc. I.R.E.* 24:351-375.

With G. A. Morton. Applied electron optics. *J. Opt. Soc. Am.* 26:181-189.

Electron optical systems and their applications. *I.E.E. J.* 79:1-10.

Iconoscopes and kinescopes in television. *RCA Rev.* 1:60-84.

1937

With G. A. Morton and L. E. Flory. Theory and performance of the iconoscope. *Proc. I.R.E.* 25:1071-1092.

1940

With G. A. Morton. *Television: The Electronics of Image Transmission.* New York: John Wiley.

1941

With J. Hillier and A. W. Vance. An electron microscope for practical laboratory service. *Electr. Eng.* 60:157-162.

With E. G. Ramberg. Surface studies with electron microscopes. *J. Appl. Phys.* 12:692-698.

1942

With J. Hillier and R. L. Snyder. A scanning electron microscope. *J. Am. Soc. Test. Mat.* 117:15-23.

1943

Electron microscopy in chemistry. *Ind. Eng. Chem.* 35:450-458.

1945

With G. A. Morton, E. G. Ramberg, J. Hillier, and A. W. Vance. *Electron Optics and the Electron Microscope*. New York: John Wiley.

1949

With E. G. Ramberg. *Photoelectricity and Its Applications*. New York: John Wiley.

1952

With L. E. Flory. Television in medicine and biology. *Electr. Eng.* Jan. pp 40-45.

1957

With F. L. Hatke. Ultraviolet television color-translating microscope. *Science* 126:805-810.

Television techniques in biology and medicine. In *Advances in Biological and Medical Physics Vok. 5*, ed. J.H. Lawrence and C.A. Tobias, pp. 243-283. New York: Academic Press.

1958

With L. E. Flory. Electronic control of motor vehicles on the highway. Part I. Development. *Proc. Highw. Res. Board* 37:436-451.

With E. G. Ramberg and L. E. Flory. *Television in Science and Industry*. New York: John Wiley.

1961

Medical electronics—Promise and challenge. *Proc. Am. Philos. Soc.* 105:340-347.

With F. L. Hatke. The measurement of internal physiological phenomena using passive-type telemetering capsules. *IRE International Convention Record*, Part 9, Bio-Medical Electronics, Nuclear Science, Instrumentation, pp.141-144.

1965

Medicine and biology, challenge to the engineering profession. *Proc. I.E.E.E.* 53:227.