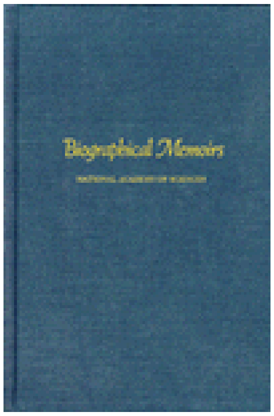

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VOLUME 70

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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.

PETER H. RAVEN
Home Secretary

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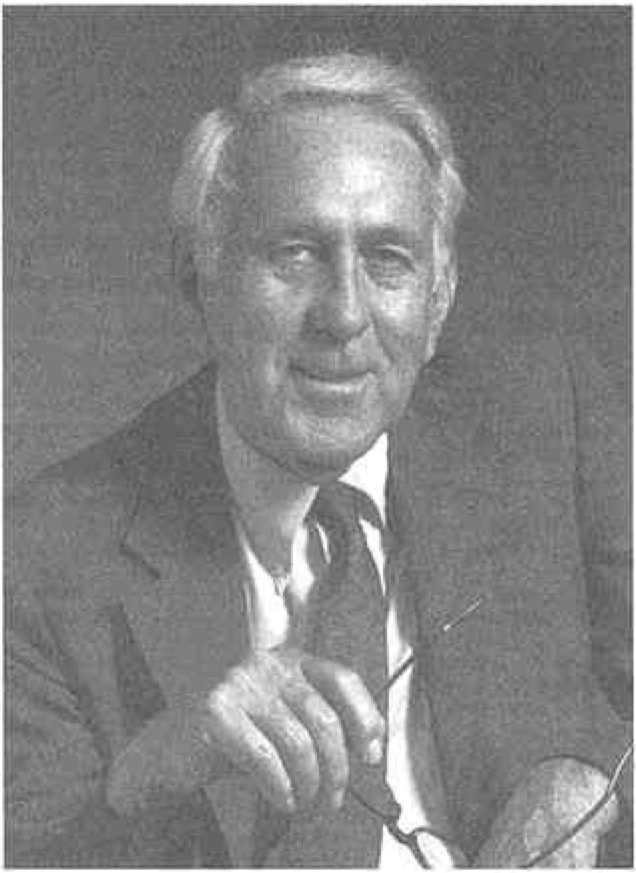

Courtesy of Pioneer Hi-Bred International, Inc.

WILLIAM L. BROWN

July 16, 1913-March 8, 1991

BY DONALD N. DUVICK

DURING A FIFTY-YEAR CAREER as geneticist, businessman, and public servant, William L. Brown made significant and lasting contributions to increasing and stabilizing food production worldwide. He did so through his personal research, his administration of a major international seed firm, and his service on key national and international agricultural boards. Throughout his career Brown was especially concerned with preserving, describing, and utilizing the wealth of germplasm represented by the hundreds of races of maize, collected or still available from indigenous farmers in the Americas and worldwide. He was a leader in efforts to conserve, classify, and utilize this diverse genetic resource.

Brown preached the cause of genetic diversity in all crops as a bulwark against pest epidemics and changing weather patterns. He prodded and then helped the U.S. Department of Agriculture to increase its effort and effectiveness in conservation of plant genetic resources. He reinvigorated the National Research Council's Board on Agriculture, leading it into path-breaking studies and reports on sustainable agriculture as well as genetic resource conservation. He was a leading figure in the movement to internationalize plant.

breeding in the service of the rural poor of developing countries.

Brown and his colleague Edgar Anderson published landmark monographs on the two ancestral maize types (Southern Dents and Northern Flints) that, when hybridized, gave rise to the modern North American race (Corn Belt Dent). Corn Belt Dent is the genetic foundation for all hybrid maize of the temperate zones.

Known and revered as a leader, but also as a scientist and a humanitarian, William L. Brown made lasting contributions to science and to humanity.

PERSONAL HISTORY

William Lacy Brown was born in Arbovale, West Virginia, on July 16, 1913, into a family of West Virginia hill farmers and grew up on a Greenbrier Valley livestock farm. According to Brown's friend and mentor, Edgar Anderson, William Brown's father was "a genuine gentleman, courteous and kind in manner, but with no interest in books or their contents."

Brown attended the local rural grammar school and then went on to high school in the nearby community of Green Bank. He developed an interest in the science of biology while still in high school, where he also was a star athlete active in football, basketball and track. Following graduation from high school, he enrolled at Bridgewater College, a small liberal arts school in the hills of western Virginia. He majored in biology, was captain of both the football and basketball teams and served as class president in all four years of college. He graduated with a degree in biology. After a year of graduate work at Texas A&M University he transferred to Washington University (St. Louis), where he studied under Edgar Anderson in the Henry Shaw School of Botany, majoring in cytogenetics and taxonomy. He received an M.A. (1939) and Ph.D. (1941) from that institution. His doctoral thesis was entitled "Cytogenetics of *Poa pratensis."* According to Brown the thesis demonstrated a strong likelihood that the apomictic species *P pratensis* (bluegrass) is a polyploid of hybrid origin.

In August 1941 William Brown married Alice Hannah, a high school classmate. From that union came two children, Alicia Anne (Matthes)Brown and William Tilden Brown.

Also in 1941 Brown left Washington University for a position as cytogeneticist with the U. S. Department of Agriculture, Forage Crops Division. His doctorate was granted in absentia. In 1942 he left the USDA for a position in industry, as director of sweet corn breeding for Rogers Brothers Company in Olivia, Minnesota. In 1945 he again changed jobs, accepting a position as geneticist in the Corn Breeding Department of the Pioneer Hi-Bred Corn Company (which would later change its name to Pioneer Hi-Bred International, Inc.). Brown stayed with Pioneer Hi-Bred until his retirement in 1984.

Although genetics applied to plant breeding was Brown's profession, botany in its broadest sense was his avocation. He was a keen gardener and horticulturist, growing an eclectic and purposely diverse mixture of useful fruits and vegetables and exotic trees and shrubs. He was particularly proud of his success in growing a handsome bald cypress tree, certainly not indigenous to Iowa. Brown introduced me to the mysteries and complexities of Iowa's native prairie flora, taking me and another friend to a nearby tall-grass prairie remnant on a hot, sunny August afternoon when the composites and grasses were in full flower. The prairie was floristically rich; the air was redolent with the resinous fumes of numerous, diverse sunflower species. Brown pronounced the outing a near success; it lacked only the sighting of a rattlesnake.

William and Alice Brown joined the Society of Friends soon after their move to Des Moines. As they said to me when they were contemplating the move, the Quaker emphasis on simplicity, integrity, service, and worth of the individual was a natural fit to their own tastes (and as I can also testify, to their own lifestyle). They were active in their local Meeting and also in the national American Friends Service Committee.

Brown was elected to the National Academy of Sciences in 1980. Active in his retirement years, he chaired the National Research Council's Board on Agriculture and Renewable Resources from 1982 to 1988. He also conducted a research program on the cytology and evolutionary history of a Native American maize variety with the goal of restoring it to its ancestral form, for use by the tribe (the Eastern Cherokee) that had developed it. Results of the study were published in his name after his death, due to the efforts of a fellow scientist and close friend, Major Goodman.

On March 8, 1991, William L. Brown died of emphysema at the age of seventy-seven.

PROFESSIONAL HISTORY

COLLEGE AND UNIVERSITY

After graduation from Bridgewater College Brown followed his biology professor, Walter S. Flory, to Texas A&M University, starting a graduate program under Flory's direction. Brown's funds were limited, and he supplemented his income by playing professional basketball with a Houston team at $50 per game.

He also met the maize geneticist Paul Mangelsdorf. This acquaintance resulted in a lifelong friendship and profes sional relationship between the two scientists. Mangelsdorf is internationally famous for his studies of maize landraces and their relationships and for his theories about the origin of maize.

Mangelsdorf and another renowned geneticist, Edgar Anderson, had been fellow graduate students at Harvard University. Anderson was now a geneticist in St. Louis with appointments at Washington University and the Missouri Botanical Garden. According to Brown, "Edgar had written telling Paul that they had support for a fellowship, and asked if there was a student down there that he would recommend." Mangelsdorf gave this information to Brown, who applied and was accepted for study with Anderson, thus solving or at least alleviating his problems with finances for graduate school. The fellowship paid $50 per month, and Anderson boarded Brown in his home rent-free.

Brown knew that Anderson was already recognized as a brilliant geneticist and taxonomist with interest in ethnobotany and evolution. He did not know that Anderson also was eccentric, even for a botanist. Anderson's first task for Brown was to study the grasses in the back yard of his home on Flora Place in St. Louis. Anderson gave no instructions other than to "work on them," and left Brown standing alone in the back yard. As Brown told me in later years (more than once), "I went out there and decided how many different kinds of grass there were. Most of it was crab grass but not all of it. After an hour or two I went and told him a few things about what I had learned. He said there must be more than that, why don't you go back and work at it a little more. We did that for a good part of a couple of days. I was just about ready to call it quits. Then I finally decided there really is something to this after all. I finally learned what a number of them were. But he wasn't going to tell me." From Anderson, Brown learned the value of keen—and independent—observation.

From Anderson, Brown also gained an appreciation of the role of hybridization in evolution. Anderson developed the concept of introgressive hybridization—usually termed "introgression"—and gave it its name. As the name implies, introgression refers to the gradual infiltration of germplasm of one species into another through repeated backcrossing. Throughout his entire career Brown's ideas about evolution and breeding were influenced by this aspect of Anderson's research and theory. He and Anderson were to work as a team for several years after Brown's graduation from Washington University, stimulated to do so by their mutual interest in the origins and categorization of maize varieties and races. They complemented each other in scientific capabilities and in temperament. Brown was skilled in the modern cytogenetic techniques. Anderson was skilled in recognizing and quantifying key morphological traits. Anderson was expansively literate with unbounded imagination in devising new theories about the evolution of races of maize and maize itself. Brown was concisely literate and, although not without imagination, tended to be much more cautious in pronouncements of new theory or in extrapolating from data.

I once spent an evening listening to the two men revise the manuscript for their 1953 paper, "Origin of Corn Belt Maize and Its Genetic Significance." Anderson would read aloud the sections he had written. Brown would listen without expression but from time to time would interrupt to say that it might be well to take out a superlative or to reduce the scope of a particularly sweeping conclusion. When Brown read his sections aloud Anderson would take the opposite tack. In the end they always agreed and their resulting papers speak for the success of their combined efforts.

CORPORATE CAREER

Brown's brief experience in the USDA left him with a lifelong distrust of the organization's bureaucratic rules. He told of needing to use a microscope on the table next to his, but being unable to use it officially until several weeks of form-filling and official approval were completed. The microscope was the property of a different division, and proper permission had to be obtained before it could be used by an outsider, even if the outsider was also in the USDA. Experiences like this made him ready to move out of the USDA at the first opportunity. He left the USDA in 1942 for a position with Rogers Brothers seed company in Minnesota.

His work as a sweet corn breeder in a small company was not very satisfying and, when Anderson told him in 1945 of an opening in a larger company, Pioneer Hi-Bred, he applied for the job and got it. Anderson had been doing some consulting work for Pioneer; they had tried to hire him, but he suggested they instead have a look at his former graduate student.

Brown's initial duties with Pioneer were not spelled out very clearly. He was supposed to do "fundamental studies," not corn breeding as such. He was the first doctoral level employee of the company. His employment represented a significant enlargement of the scope and concept of research and development activities at Pioneer Hi-Bred, which at that time employed only seven other professional researchers, all of them engaged in highly practical activities such as breeding and testing of maize inbreds and hybrids.

Brown soon developed a research program building on his training as a cytogeneticist. Within a few years he had published a paper showing the existence of a relationship between hybrid vigor and diversity of knob number of pa rental inbred lines. The diversity in knob number seemed to derive from the two postulated parents of corn belt dent varieties: Northern Flints and Southern Dents.

After a few years of work on fundamental studies Brown decided that corn breeding looked so interesting that he wanted to take an active part in it, even though he had no training in plant breeding, or even in agriculture, except for experience on the farm as a boy. But he found that lack of formal training in corn breeding was not a handicap. His training in genetics and botany gave him the grounding he needed. As he told me in later years, "Breeding is largely the application of recognized techniques, and one can pick that up pretty quickly."

Brown also gave large credit to his supervisor and mentor at Pioneer, Raymond Baker. Baker had established the corn breeding program for the company and was a mine of practical information about corn breeding. Brown has said, "Most of what I have learned about breeding I learned from Raymond. He is an amazing person as you know. He has an awful lot of knowledge that you have to pull out of him a little at a time." Interestingly, Baker did not have formal training as a corn breeder either. His highest degree was a B.S. in agriculture from Iowa State College. He had spent a lifetime of association with professionally trained corn breeders, however, and learned theory from them, which was then tested by him and his breeders in the practical world of plant breeding for profit.

Brown began to travel with Baker on his annual round of inspection trips to Pioneer corn breeding stations across the corn belt. Baker soon gave Brown responsibility for guidance of several of the breeding stations, particularly those in the eastern and southern part of the United States. From that beginning Brown's responsibilities in research continued to enlarge. In 1958 he was given the title of assistant director of research; in 1965 he was made vice-president and director of corporate research for the company.

Brown's abilities in administration eventually took him out of research management. In 1975 he became president of the company and in 1979 chairman and chief executive officer. Brown retired from day-to-day management in 1981, when he assumed the position of chairman of the board. In 1984 he retired from the company.

As research director Brown supervised and was largely responsible for a rapid rise in the company's research capacity primarily through addition of new plant breeders and support staff and new breeding locations. Under his direction the company undertook breeding and sales of several crops in addition to corn, including sorghum, soybeans, wheat, and alfalfa. Research was initiated to develop maize hybrids for the tropics and for northern Europe, two very different markets with very different breeding needs. Brown's vision of potentials for commercial plant breeding activity spread well beyond the company's original target of hybrid corn for the U.S. Corn Belt. He envisioned, and helped to develop, a company with international responsibilities in breeding and selling seed of many crops.

Brown's interest in and support for the company's research programs continued during his years as president, chief executive officer, and chairman of Pioneer Hi-Bred. He initiated a reorganization of the company's research programs soon after assuming the office of president. The previously independent research programs were united under one administrative head, giving them greater access to common facilities and knowledge across the company. Additional crop species were added to the company's research and development program. With his encouragement biotechnology research and development was initiated in 1979, early in his tenure as chief executive officer. Most impor tantly, product performance, sales, and profitability increased continually during his administrations as research director, president, and chief executive officer. For example, yielding ability of the U.S. maize hybrids sold by the company increased by approximately 25% from 1968 to 1984. Net income for the company in 1984 was approximately three times as great as in 1975.

Researchers at Pioneer remember Brown with great affection and respect. He understood the details and possibilities of research as it applied to needs of the company and, perhaps more importantly, he understood and empathized with the researchers as scientists and as people. It was clear to researchers that he was happiest when out in the research plots inspecting the latest varieties and discussing details of breeding and genetics with the scientists.

RESEARCH CAREER

Just as his high school and college careers had not been one-sided, Brown's career as a scientist-administrator had more than one dimension. Throughout most of his career he maintained a personal program of basic research in maize with emphasis on classification and relationships of the races of maize and on genetic diversity and its consequences for plant breeding.

Within a year after joining Pioneer he toured the southeastern United States, collecting local open pollinated varieties of maize. He did so because he knew that hybrid corn was soon going to replace all of the local varieties; if they were not collected and preserved, their germplasm would be lost and future breeders would have no chance to sample it for needed genes such as for special kinds of disease resistance.

In the next year, 1947, he grew out the collection for increase and study at the Arboretum of the Missouri Bo tanical Garden located west of St. Louis in the outskirts of the Missouri Ozarks. To do this he moved his young family onto the grounds of the Arboretum, where they lived in semi-isolation in a slightly refurbished barn, cooking over an open fire and depending on a spring for drinking water and the nearby Meramec River for bathing facilities. Alice Brown was then, as in later years, an essential party to Brown's research endeavors. When studies were completed Brown donated the collections, on behalf of Pioneer, to the North Central Plant Introduction Station at Ames, Iowa. They still reside there, available to anyone who asks for them.

The collections of southern open pollinated varieties of maize were an important foundation for one of two seminal publications co-authored with Edgar Anderson. "The Northern Flint Corns" and "The Southern Dent Corns" appeared in 1947 and 1948. The two monographs were followed by the publication of "Origin of Corn Belt Maize and Its Genetic Significance," also co-authored with Anderson. These landmark papers described the two North American racial complexes in great morphological and cytogenetic detail and discussed their role as parents of the Corn Belt Dent complex and as contributors to heterosis presently utilized in modern hybrid maize. The Corn Belt Dent complex ("the common yellow dents") is the progenitor of today's U.S. maize hybrids, as well as of hybrid maize for the temperate zones worldwide.

In 1952 Brown obtained a leave of absence from his work at Pioneer to accept a twelve-month position as Fulbright advanced scholar at the Imperial College of Agriculture in Trinidad. He collected maize varieties from Caribbean islands and grew them out at the Imperial College as a basis for their classification into races and discussion of their possible relationships. The study was published under the title, "Maize of the West Indies." This research evolved natu rally from his earlier work with U.S. open pollinated varieties. It was the beginning of a lifetime career of study and classification of the races of maize, sometimes independently but more often in association with other geneticists and botanists.

Brown made the acquaintance of Henry A. Wallace in the first year of his employment with Pioneer Hi-Bred. Wallace, a native Iowan, was secretary of agriculture for President Franklin Roosevelt in the 1930s, and then gave further service to the Roosevelt administration as vice-president of the United States, and secretary of commerce. Wallace had founded the Hi-Bred Corn Company (the forerunner of Pioneer Hi-Bred) in 1926. He turned the management of the company over to others when he went into government service but nevertheless maintained a deep interest in the company's research work in corn breeding and genetics. He routinely came out to Iowa to visit the company breeding nurseries in late summer when the corn plants were well developed and suitable for inspection and close observation.

Wallace soon developed an easy and close relationship with Brown, not only in regard to scientific subjects, but also in consideration of other philosophical topics. One result of the friendship was the book, *Corn and its Early Fathers,* first published in 1956. I remember well the day when Wallace proposed to Brown that they collaborate in writing the book. The two men had spent the previous day, a Sunday, in looking at a series of old open pollinated maize varieties that Brown was growing in order to study and compare their special characteristics, particularly as sources of inbred lines. Wallace and Brown had not only studied the varieties, they also had spent much of the day in speculating on the origins of the varieties, the purposes for which they might have been developed, and what this might mean for hybrid corn of the present. They now were seated in Brown's office laboratory discussing the previous day's activity. Early on in the conversation Wallace suggested that he and Brown should also look at the lives of the originators of the varieties: "They might be as interesting as the corn." From such an investigation they might be able to write a book, one that would educate the interested public in the societal and personal aspects of corn breeding and selection. Brown was silent for a time, then replied that although he had never written a book, he might be able to help.

This incident illustrates an important contribution that Wallace made to Brown's career. He encouraged Brown in his natural inclination to pursue studies on maize for their own sake, as well as for the possibility that they might make money for the company. The two men also agreed that plant breeding must be done with strong concern for its social consequences and with recognition of the personalities and social forces that shaped the nature of research and development programs.

Nobelist Barbara McClintock and Brown were friends and professional collaborators. Their acquaintance started in 1945 when Brown and Edgar Anderson went to Cold Spring Harbor to learn cytogenetic technique from her. Brown understood and agreed with McClintock's ideas about transposition of genetic material ('jumping genes") long before the majority of the genetic establishment was aware of her work; however, the main bond between the two scientists was their common interest in use of cytological markers, especially chromosome knobs, to classify races of maize.

PUBLIC SERVICE CAREER

Brown's rapid rise as an authority on genetic diversity and racial affinities of maize soon brought requests that he serve on boards and committees concerned with preservation and classification of indigenous maize varieties from all parts of the world. He was a member of the National Research Council's original Committee on Preservation of Indigenous Strains of Maize from 1952 through 1956. On this committee he classified NRC maize collections from Bolivia and Chile and gained great satisfaction from doing so.

A succession of similar committee and board assignments and chairmanships, most of them relating to genetic diversity and genetic resources, continued throughout his entire career. Most notably, he chaired the Rockefeller Foundation's Maize Germplasm Committee from 1969 to 1972, served on the National Academy of Sciences's Committee on Genetic Vulnerability of Major Crops in 1972, chaired the Maize Advisory Committee for the International Board for Plant Genetic Resources in 1975, and served as vice-chairman (and usually as acting chairman) of the USDA's National Plant Genetic Resources Board from 1976 to 1982.

Service on these committees gave opportunity for Brown to voice his deep concern for the genetic vulnerability of crop plants and to advocate policies of germplasm conservation intended to preserve farmer varieties that otherwise might have been lost as farmers abandoned them for new, professionally bred varieties. He repeatedly pointed out the relatively narrow genetic base of Corn Belt Dent maize hybrids and advocated development of breeding methods for introgression of diverse germplasm from the hundreds of races of maize that as yet have no representation in Corn Belt Dent maize. He also published on these topics in professional journals and other media.

While serving on the National Plant Genetic Resources Board, formed to make policy recommendations to USDA, Brown was a major force behind the board's successful rec ommendations that USDA increase and update its activity in conservation of plant genetic resources, through its National Plant Germplasm System.

Brown's warnings about genetic vulnerability were buttressed with realistic assessments of the situation. He pointed out that "total genetic diversity" does not in itself provide insurance against genetic vulnerability. Useful sources of genetic diversity must include needed new kinds of resistance to pests and other stresses that adversely affect productivity and quality. He noted that, contrary to statements from some critics, modern cultivars can be more stable in performance than older ones they have replaced. He also opined, in opposition to some critics, that a commercial seed industry could make significant contributions to the overall effort to increase genetic diversity in crop plants.

AFTER ELECTION TO THE ACADEMY

Brown chaired and revitalized the National Research Council's Board on Agriculture and Renewable Resources from 1982 to 1988, taking on this responsibility soon after retiring from active management duties at Pioneer Hi-Bred. As chair he brought about major NRC publications in the fields of sustainable agriculture and plant genetic resources and thereby moved agricultural concerns into a prominent place in the National Academy of Sciences.

Brown was directly responsible for founding in 1982 the quarterly news journal *Diversity.* He garnered funding for its establishment and operation from industry, government, and private foundations, convincing donors of the need for a journal that would foster international dialogue and cooperation in the conservation and use of humanity's plant genetic heritage. In 1985 he organized the nonprofit Genetic Resources Communications System, Inc., to publish the journal. He served as president of the board of directors for its first three years.

Brown put some of his company's funds behind his pronouncements on conservation of plant genetic resources. In 1987 Pioneer Hi-Bred granted $1.5 million to the USDA to initiate and direct the Latin American Maize Program (LAMP), a five year project. LAMP was a path-breaking cooperative effort of twelve Latin American countries, which rejuvenated, grew out, and evaluated collections of maize varieties that had been languishing in their germplasm banks for decades due to lack of funds for operation. Unprecedented cooperation among the twelve participating countries resulted in evaluation of more that 14,000 landrace varieties, many of which had immediate utility in their country of origin or in nearby countries. This project, perhaps more than any other, epitomized Brown's hopes and aspirations for the cause of plant genetic diversity in support of humanity worldwide. Although he did not live to see its completion or the follow-up programs that it stimulated, he would have heartily approved the final form of the project and its results.

BROWN'S LEGACY

William I. Brown was a biological scientist. He personally produced consequential scientific research in maize genetics and cytogenetics, maize evolution, and germplasm conservation and utilization. His research in general was descriptive and analytical in nature, rather than experimental. Brown's personal accomplishments in science are not, however, the primary reasons for his high regard in the scientific community; rather, he was known and will be remembered as a visionary leader, a stimulator, an organizer. The captain of industry was also an initiator and executor of worthy new public service projects in science. Brown in spired affection and emulation among his peers; he also had an unbending will when he was convinced that the path he had chosen was the right one. Brown brought projects to completion; he left no dangling threads. These manifold and diverse qualities, applied to science that mattered to him, gave results. Brown classified and described landraces of maize, but he aided and inspired far more work than he turned out personally. He was a practical maize breeder and developed valuable germplasm, but under his direction scores of plant breeders in all parts of the globe annually boosted yielding ability of many of the world's staple crops. He personally investigated and experimented to increase genetic diversity in crop plants, but in leadership of such bodies as the USDA's National Plant Genetics Resources Board, in formation of the journal *Diversity* and in instigation of the Latin American Maize Program, he multiplied his personal efforts by orders of magnitude.

Perhaps most consequential was the fact that Brown put his talents to work in the right causes at the right time. He raised the banner for conservation of genetic resources and for increased genetic diversity just as professionally bred, new varieties were displacing countless numbers of farmer varieties worldwide. He reorganized his company's research activities at the point when they had grown too voluminous to operate well under an earlier policy of nearly total decentralization. He instigated the Latin American Maize Program at the point when it was becoming clear to all that germplasm collections in many developing countries were going to die on the shelf, unknown and unused for lack of funds to rejuvenate the seed or to categorize and characterize the materials.

Brown's legacy is the message that science in service of humanity can be accomplished when scientists persistently seek out and constructively work with appropriate bases of power and funds.

IMPORTANT HONORS AND MEDALS

Fellow, American Society of Agronomy, 1970
Fellow, Iowa Academy of Science, 1970
Fellow, Drake University, 1970
Agronomic Service Award, American Society of Agronomy, 1979
Distinguished Fellow, Iowa Academy of Science, 1980
Distinguished Alumni Award, Bridgewater College, 1980
Member, National Academy of Sciences, 1980
Honorary Phi Beta Kappa, Drake University, 1981
President, Crop Science Society of America, 1982
Distinguished Economic Botanist, Society for Economic Botany, 1982
Distinguished Alumni Award, Washington University, 1983
Genetics and Plant Breeding Award for Industry, Crop Science Society of America, 1986
Henry Shaw Medal, Missouri Botanical Garden, 1986
Honorary D.Sc., Drake University, 1987
Fellow, American Association for the Advancement of Science, 1989
Honorary Ph.D., West Virginia University, 1989

WILLIAM L. BROWN'S FRIENDS and family and his personal files as well as those maintained by Pioneer Hi-Bred International have been valuable sources of information for this short memoir. Two additional excellent aids have been a brief but fact-filled biography from the National Academy of Sciences issued at the time of his death, and a superb biographical article by Isabel Shipley Cunningham published in 1992 in the journal *Diversity.* Finally, I have drawn on my forty-plus years of companionship with Brown, my mentor and fellow scientist at Pioneer Hi-Bred, and family friend and fellow botanist in other walks of life.

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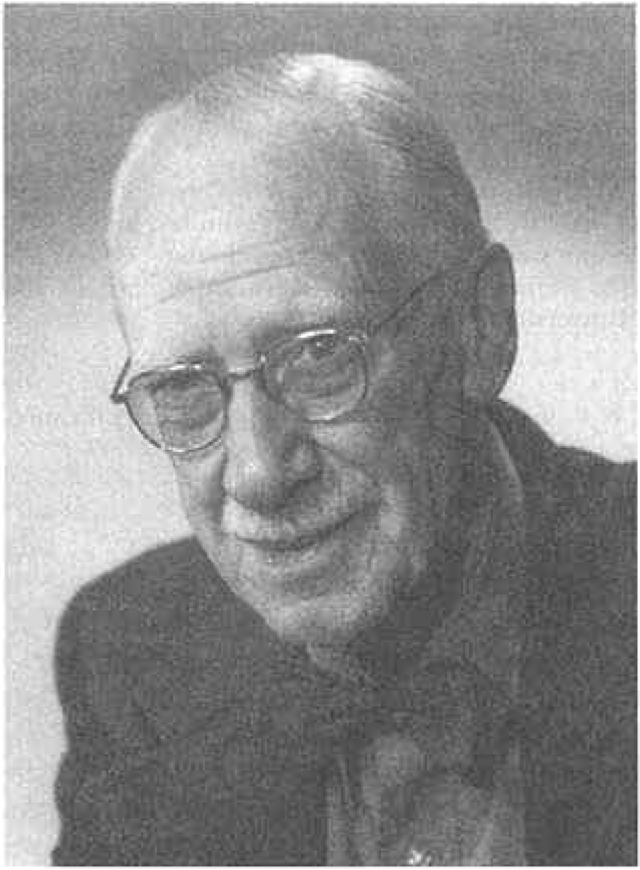

KENNETH STEWART COLE

July 10, 1900—April 18, 1984

BY SIR ANDREW HUXLEY

KENNETH STEWART COLE'S training was in classical physics and electrical engineering but he turned his skills to the investigation of the electrical properties of living tissues. Through an impressive combination of theoretical and experimental approaches, he made major contributions to our understanding of the surface membranes of many types of cells, and especially of the changes undergone by the electrical properties of the membranes of excitable cells when activated. In particular, his demonstration in 1938 (with H.J. Curtis) of a large increase in membrane conductance during the passage of a nerve impulse, without change of capacitance, was a major landmark.

PERSONAL HISTORY

Kenneth Cole, known to his wife as Ken but to all his friends as Kacy, was born on 10 July 1900 at Ithaca, New York. His father, Charles Nelson Cole, was at that time an instructor in Latin at Cornell University; two years later the family moved to Oberlin, Ohio, as his father took a post at Oberlin College, of which he later became Dean. Cole's

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mother was Mabel Stewart; both his parents came from Urbana, Illinois. There was one younger brother, Robert, with whom he remained very close throughout his life despite a large difference in age; they were joint authors of four papers published between 1936 and 1942.

In 1932, Cole married Elizabeth Evans Roberts, an attorney. Later, her work was mostly concerned with civil rights and in 1956 she joined the staff of the new Civil Rights Commission; in this work she made many journeys to the South investigating the validity of racial segregation complaints. She died in 1966. They had one son and one daughter, both still living.

From his early years he had strong interests in electricity: he records that as a youngster he 'produced sparks and shocks with worn out parts from the telephone company and put together a licensed wireless station with a Ford spark coil and galena (for a detector) begged from the head of the Geology Department' (Cole 1979). He majored in physics at Oberlin College, but delayed completion of the course by working for more than a year in the General Electric Research Laboratory at Schenectady. Here he met, and was much influenced by, Irving Langmuir, whose famous work on surface films at an air-water interface may well have been one of the origins of Cole's interest in the surface membranes of living cells. For his Ph.D. he moved to Cornell where he developed an electron spectrograph and studied the photographic action of electrons under the supervision of F.K. Richtmyer, at the same time as holding an instructorship (1922-1926).

His switch of interest to biological objects that could be investigated by physical, especially electrical, techniques was kindled by summer visits that he made during this period. In 1923 he spent some weeks at the Cleveland Clinic with

H. Fricke, who was just completing his epoch-making measurements of the electrical capacitance of the surface membrane of red blood corpuscles, using a high-frequency alternating current bridge (Fricke 1923, 1925). He records (Cole 1979) that he became committed to biology after spending the next summer at the Marine Biological Laboratory at Woods Hole on Cape Cod, Massachusetts, where he worked on heat production by the eggs of the sea urchin *Arbacia* under C.G. Rogers. His interest in the electrical properties of living things was probably stimulated by his lifelong friendship with W.J.V. Osterhout, which began during this visit to Woods Hole.

After completing his Ph.D. Cole was awarded a postdoctoral fellowship by the National Research Council to use Fricke's method to determine the membrane capacity of sea-urchin eggs. He held this fellowship (1926-1928) at Harvard, and went to Woods Hole for his experiments. During this period he also did important theoretical work on the impedance of a suspension of spherical cells and on the representation of impedances by plotting reactance against resistance.

He spent the year 1928-29 in theoretical work on cell membranes at Leipzig in the laboratory of Debye, supported by a fellowship from the General Education Board. He made himself familiar with the theory, due to Nernst and Planck, of the potential differences generated by diffusion between two different electrolyte solutions—very relevant to the potentials across biological membranes. During this time, L.E. Sutton, F.R.S., was also in Debye's laboratory, and he and Cole jointly made suggestions for the improvement of the centrifuge microscope developed by E. Newton Harvey, whom Cole had met at Woods Hole.

Cole returned to the U.S.A. to take up two posts associated with Columbia University, New York: as Assistant Pro fessor (later Associate Professor) in the Department of Physiology (1929-46) and as Consultant Physicist at the Presbyterian Hospital (1929-42). His duties were varied: calibrating radiotherapy machines; advising on safety when using cyclopropane as an anaesthetic (there had been an explosion in an operating theatre); overhauling a medical physiology teaching laboratory; and giving a few lectures. He collaborated with surgeons in developing an operation for aortic aneurysm using an electrically heated wire. In his own research, he continued his investigations, both theoretical and experimental, on the electrical properties of animal and plant tissues, using at first what he describes as a 'crude' bridge; in 1935 he settled down to designing and building a high-precision alternating current bridge which could be used quickly over a wide range of frequencies and which he used in his famous demonstration of the decrease in membrane resistance of nerve during the passage of an impulse. He also built an apparatus with which he measured the surface tension and elasticity of the surface membrane of sea-urchin eggs, following up observations that Newton Harvey had made with the centrifuge microscope that Cole had helped to design.

Cole spent most of his summers working at Woods Hole on sea-urchin eggs, on the fresh water alga *Nitella* and, after 1936 when he met its discoverer J.Z. Young, on the giant nerve fibre of the squid. One summer was spent with Fricke at the Cold Spring Harbor Laboratory on Long Island Sound, where he worked on the electrical impedance of the marine alga *Laminaria,* and another summer at the marine biology station on Bermuda.

Early in 1936, Cole was joined by Howard J. Curtis, who had previously worked with Fricke and was thus familiar with alternating current methods of investigating the electrical properties of biological objects. Most of Cole's scien tific work from that date until Curtis moved to a post at Johns Hopkins in 1942 was done in collaboration with Curtis, including the demonstration of membrane impedance changes in the nerve membrane during an impulse. Also in 1936, Cole visited Britain and met A.L. Hodgkin, who later spent a year (1937-38) at the Rockefeller Institute in New York, during which he joined Cole for a short spell at Woods Hole to measure the D.C. resistance of the membrane of the squid giant nerve fibre in the resting state (a quantity that could not be determined by the A.C. methods then being used by Cole and Curtis). In 1939 they began using internal electrodes inside the giant nerve fibre of the squid.

Cole spent 1941-2 on leave from Columbia as a Guggenheim Foundation Fellow at the Institute for Advanced Study at Princeton, studying literature on non-linear systems. From 1942 to 1946, still on leave from Columbia, he was Principal Biophysicist at the Metallurgical Laboratory, University of Chicago with Curtis as his next-in-line; here he was in charge of research on the biological effects of radiations and radioactive materials produced by the uranium fission chain reaction process and was responsible for biological aspects of safety in the Manhattan project (atomic weapon development). D.E. Goldman told me that Cole and Szilard were among those who voted against the decision to drop the first bomb.

In 1946, the University of Chicago set up a new Institute of Radiobiology and Biophysics and Cole accepted appointment as Professor of Biophysics and Physiology and head of the Institute; he was re-joined by George Marmont, who had joined him at Columbia in the early 1940s. On a suggestion from J. Savage, Marmont made an internal electrode to be inserted into a squid giant fibre, with long conducting surface so as to ensure that the internal potential was uniform within this length; current would be fed to the electrode under feedback control and Cole added an arrangement by which alternatively the potential of the electrode could be controlled electronically. In the latter mode of operation, this was the first 'voltage clamp' (a phrase that Cole did not like). This was the method by which great advances were made in the years after the war; unfortunately Cole contributed little to those advances largely, it seems, because Marmont had a strong preference for operating the equipment in the 'current clamp' mode which turned out to be far less informative than the 'voltage clamp' (Cole 1982, p. 316).

The first experiments with this equipment were made in the summer of 1947. Cole told Hodgkin about these experiments in a letter later that year, and gave him full details in a visit that Hodgkin made to the U.S.A. next spring; at this meeting Hodgkin also told Cole of the experiments on squid nerve that he and Katz had done in 1947 establishing that the action potential is generated by sodium ions moving down their concentration gradient. Hodgkin had already been thinking of making a voltage clamp, but the information he was given by Cole and Marmont was a great help and stimulus towards developing the equipment that he used, together with Katz and myself, in the summers of 1948 and 1949.

In 1949 Cole moved again, to become Technical Director of the Naval Medical Research Institute at Bethesda, Maryland, close to Washington D.C. Although he had an intellectually powerful team, including Manuel Morales, David Goldman, Terrell Hill and John Moore, Cole's time at NMRI appears from his own account (Cole 1979, p. 17) not to have been happy, partly on account of Senator McCarthy's activities; nor was it scientifically very productive. As a result, Cole moved once more, in 1959, to set up a new Laboratory of Biophysics in the National Institute of Nervous

Diseases and Blindness, National Institutes of Health, just across the road from the NMRI. He took John Moore with him, got an improved voltage clamp running, and produced a series of valuable papers in 1960. He stepped aside from the headship of the laboratory in 1966, shortly after reaching the age of 65, but continued working there as Senior Research Biophysicist (part time from 1971). For the first semester of 1963-64 he was Regents' Professor at the University of California at Berkeley; there he gave a course of lectures which became the foundation for his book *Membranes, ions and impulses,* published in 1968.

Cole exerted a great influence on the development of membrane research, not only by his discoveries, his techniques and his precise measurements but also through his book and many general articles; through personal guidance of many, both in his laboratory and elsewhere; and through the Training Program on Excitable Membranes at the Marine Biological Laboratory, Woods Hole. In a broader field, he was one of the prime movers in establishing the Biophysical Society and the International Union of Pure and Applied Biophysics.

RESEARCH ON CELL MEMBRANES

By far the greater part of Cole's published scientific work was on electrical aspects of cell membranes. This originated as an extension of the measurement of the capacitance of suspensions of red blood corpuscles by H. Fricke, with whom Cole had spent part of the summer of 1923. Fricke (1925) found a value of 0.81 $\mu F\ cm^{-2}$ for the capacitance of the surface membrane of these cells, in close agreement with more recent determinations; he took the dielectric constant of the lipid material of the membrane to be 3 and deduced a thickness of 3.3 nm, implying a monomolecular layer. By the time Cole began his work on membranes, in

1926, Gorter & Grendel (1925) had shown that the area of the monomolecular film formed on a Langmuir trough by the membrane lipids was double the surface area of the red blood corpuscles from which the lipids had been extracted, showing that the membrane was a bimolecular, not a monomolecular, layer, and that Fricke had underestimated the dielectric constant. Thus, the outline of present-day ideas of the basic structure of cell membranes was established just before Cole's work began.

Up to 1936, Cole explored the linear, passive electrical properties of cell membranes. At first, in place of the alternating current bridge used by Fricke, he adopted the method of Philippson (1921) and measured the ratio of mean square voltage to mean square current as a function of frequency when the output of an oscillator was applied to the cell suspension, using vacuum thermocouples as detectors. His first measurements were on the eggs of the sea urchin, *Arbacia punctulata,* which he chose largely because these were the objects whose heat production he had measured in the summer of 1924 and there were suggestions that heat was liberated by the superficial part of the cell when it was fertilized. These eggs also had the advantage of being spherical, so that the theoretical treatment was much simpler than for red blood corpuscles with their shape of biconcave discs. He analysed his results by means of an original theoretical treatment which was a great advance on the simplified theory used earlier by Fricke and others. His results, which turned out later to be misleading, was that the surface capacitance of the cells varied inversely with the square root of frequency, and he concluded that the apparent capacitance was a 'polarization capacitance', i.e. that it was due to accumulation of ions on the two sides of the interface between cell contents and the surrounding solution as current flowed, rather than to a dielectric layer separating the two conduct ing phases. The dependence of apparent capacitance on frequency was not a surprise as similar effects had been found by Philippson and others when measuring the electrical properties of various animal and vegetable tissues.

Cole's next contribution (7)* was a theoretical treatment of the bulk electrical properties of a cell suspension in which the surface of the cell had a frequency-dependent capacitance with loss such that the ratio *m* of the equivalent series resistance to the capacitative reactance was independent of frequency ('constant phase angle'). He proved a result that he had stated in a previous paper (5), namely that when (capacitative) reactance is plotted against series resistance for a suspension of cells with membranes showing this property, the locus as frequency is changed in an arc of a circle with centre below the resistance axis. Cole presented almost all his observations on the passive electrical properties of cells in graphs of this form. In this paper (7) he stated that many sets of observations on a wide range of tissues, some by himself and others from the literature, could be fitted in this way with various values of the ratio *m.*

Cole's next measurement on sea-urchin eggs (21) used a different species, *Hipponoë esculenta.* To his surprise, the reactance plots were semicircles with the centre on the resistance axis (phase angle 90°), implying that the membrane capacity showed no loss and was independent of frequency, i.e. it was an ideal capacitance. The same result was found with the eggs of the starfish *Asterias* (22) and also on reinvestigation of the eggs of the sea-urchin *Arbacia* (23). The reason for the low phase angle found previously with this material was not resolved at that time; in a later review (93,

* Numbers in this format refer to entries in a microfiche prepared by the Royal Society, London, England. A photocopy is available from the Royal Society upon request.]

p. 32) Cole hints that it may have been due to using mixed batches of eggs from different individuals, as both the diameter of the eggs and the apparent capacitance vary substantially between individuals.

These experiments clarified the situation greatly as regards uniform cells in suspension (mammalian red blood corpuscles as well as echinoderm eggs) in showing that their membranes had a nearly perfect capacitance such as would be expected from a thin lipid surface membrane, but created a discrepancy between cell suspensions and whole tissues, which gave phase angles between 55° and 78°. Cole returned to the theory of constant phase angle behaviour in two papers written jointly with his brother Robert H. Cole and published in 1941 and 1942 (44,48). They show that the transient response of a circuit element of this type is a power function of time, and they discuss several possible underlying causes for such behaviour without reaching a definite conclusion.

Meanwhile, Cole turned his attention from cell suspensions to excitable tissues, mostly the fresh-water alga *Nitella* which has long cylindrical cells capable of propagating a long-lasting (approximately one second) action potential, and the giant nerve fibre of the squid to which he was introduced by its discoverer J.Z. Young in 1936; there is also one paper (20) on muscle. From 1936 to 1942, the greater part of this work was done jointly with H.J. Curtis; their principal tool was the A.C. bridge that Cole designed and built in 1935 (26).

Measurements of the resting electrical characteristics of *Nitella* cells (28) and of the giant nerve fibre of the squid (32) showed membrane properties not unlike those of other cells: a capacitance of around 1μF cm^{-2} with a constant phase angle a little less than 90°. These measurements were made with the current flow perpendicular to the long axis of the cell. In this situation, the cell is shunted by the low resistance of the solution in which it is immersed, and the resistance of the cell membrane in parallel with its electrical capacity is so high as to be effectively infinite and its actual value cannot be determined by this technique. However, when current flow is parallel to the fibre axis, between electrodes separated by a good many millimetres, a substantial fraction of the current flows through the surface membrane even at low frequencies or with direct current, and the membrane resistance can be measured by either A.C. or D.C. methods. *Nitella* being a fresh-water organism, Blinks (1930) had been able to make the external resistance high enough so that its shunting effect was slight and a D.C. measurement of longitudinal resistance gave the membrane resistance with only small corrections; he found values of the order of 10^5 ohm cm^2. The situation is much more complicated in the case of the squid giant nerve fibre as it has to be surrounded by a layer of salt solution of high conductivity and the passage of current through the membrane is not uniform and is distributed over an appreciable length of the fibre; Hodgkin had worked out a possible method and he and Cole (36) did the experiments during Hodgkin's visit to the U.S.A. in 1938; they found a value around 10^3 ohm cm^2.

The most widely accepted theory of excitation and conduction in excitable cells at that time was that of Bernstein (1902) according to which the resting potential (inside 50-100 mV negative relative to the external solution) was a concentration potential due to the membrane being appreciably permeable to potassium (but not to sodium) ions while the concentration of potassium inside was around 50 times that in the external solution; excitation consisted in a great increase of the permeability of the membrane to all ions so that the membrane potential would fall nearly to zero. Current would then flow from adjacent regions of the cell, causing a decrease in the absolute value of the membrane potential there and this in turn was assumed to cause a similar increase of permeability, which thus travelled along the cell as a self-propagating wave. There were already hints from the work of Lullies, Dubuisson, and especially of Blinks (1936) that the propagated impulse was accompanied by a decrease in impedance but their results were not quantitative and they could not distinguish fully between changes in the capacity and the resistance of the membrane, though Blinks's result (on *Nitella)* clearly showed that there was a substantial decrease in the membrane resistance.

Cole and Curtis made a major advance in 1938 by demonstrating, first in the long excitable cells of *Nitella* (33,34) and then in the giant nerve fibre of the squid (33,35) that the change in impedance was due to a very large drop in the resistance in parallel with the membrane capacity while the latter hardly changed in value. In both cases, the membrane resistance fell to a few percent of its resting value, and the change began rapidly at the moment of the point of inflexion half way up the rising phase of the action potential. This is the moment when the EMF of the membrane must be undergoing a rapid change, so the simultaneity of the resistance drop implied that it was closely related to the change in EMF. The result was therefore a strong confirmation of one of the essential features of Bernstein's theory. It was important from the theoretical point of view not only for this reason but also because it showed that the drop in resistance, presumably due to an increase in permeability of the membrane to ions, took place without any drastic change in the basic structure of the membrane.

These measurements were made with transverse current using the A.C. bridge that Cole had constructed. Both in *Nitella* and in the squid nerve fibre, the membrane resis tance fell during the action potential to a value low enough to be measured with fair accuracy by this method. It was a technical triumph to have obtained so clear a result, especially in the case of the squid nerve fibre whose action potential lasts for less than a millisecond: Cole and Curtis recorded the out-of-balance signal from the bridge on a cathode-ray oscilloscope, altered the resistance and capacity values in the balancing arm of the bridge, and noted the two times, within the action potential duration, when the out-of-balance signal fell to zero. In the case of *Nitella,* with an action potential lasting about one second, they had been able to photograph the Lissajous figures created by the out-of-balance signal at a series of times within the action potential.

The next step forward was to take advantage of the great size of the squid fibre to put an electrode inside so as to measure directly the potential difference across the membrane and its change during the impulse. This was done in the summer of 1939, simultaneously and independently by Curtis & Cole (39) at Woods Hole and by Hodgkin and the present writer (1939, 1945) at the laboratory of the Marine Biological Association at Plymouth. Curtis and Cole found action potentials averaging 50 mV with some reaching 80 mV. In this series of experiments they used platinized metal electrodes and an amplifier without satisfactory D.C. response and were therefore unable to measure the resting potential. For these reasons it was not until the next season's work, with improved apparatus, that they confirmed (49) the observation that was the most important outcome of the measurements by Hodgkin and myself, namely that the action potential (around 90 mV in our experiments) was much larger than the resting potential (inside about 45 mV negative to external solution) so that the internal potential became positive at the peak of the action potential by some

40 or 50 mV whereas according to Bernstein's theory it should have approached but not passed zero potential difference. The origin of the EMF was not identified until after World War II when Hodgkin & Katz (1949) showed that it was due to the permeability increase being specific for sodium ions and not a generalized 'breakdown' of the membrane as had been proposed by Bernstein and widely accepted.

The measurements by Curtis and Cole in 1940 and 1941 were not only more extensive than those of Hodgkin and myself in 1939 but their electrodes allowed a better estimate of the junction potential where they made contact with the interior of the nerve fibre, an important point when considering the difference between resting and action potential. Their paper (49) did, however, contain two unfortunate errors which, after the War, contributed to the delay in acceptance of the idea that the 'overshoot' of the action potential (interior of fibre becoming positive relative to external solution at the peak of the action potential) was due to entry of sodium ions moving under the influence of their concentration difference. The first was that they used a variable inductance in their amplifier to compensate for the lag caused by the high resistance of their internal electrodes combined with the input capacitance of the amplifier; Cole (1968, p. 145) admitted later that they must have over-compensated because they obtained much larger action potentials than have been recorded in later years with equipment not subject to this source of error. The action potential illustrated in their paper had a total amplitude of 168 mV and an overshoot of 110 mV, which could have been produced by the sodium mechanism only if the internal sodium concentration had been many times lower than the measured value. The other error was the statement that the action potential as well as the resting potential was 'not appreciably affected' by replacing the external solution with an almost iron-free solution (isosmotic dextrose), which again would have been impossible if the overshoot had been due to entry of sodium ions. All subsequent work has shown that the action potential is abolished when the fibre is surrounded by a solution free from sodium ions or a few other ions (e.g. lithium) which are able to substitute for sodium; it is not clear how Curtis and Cole came to make this statement.

Two important papers from the early war years showed that the membrane of the squid fibre rectifies strongly, having a much higher conductance for outward than for inward current. This was demonstrated by changes in the transverse impedance during current flow (42) and by direct measurement of the current-voltage relation using an internal electrode for the potential measurement (43). The current was applied through a narrow external electrode and consequently the current density through the membrane and the potential difference across the membrane were not uniform along the fibre; Cole deduced a relation, sometimes known as Cole's theorem, by which they obtained current density I_m at membrane potential V_m from the measured total current I_o : $I_m = I_o dI_o/dV_m$. The resistance values at the resting potential were very low in these experiments (23 ohm cm^2 as against 1000 ohn cm^2 found by Cole & Hodgkin (36)). This was evidently due to damage to the fibre resulting from impalement by the internal electrode: when the membrane potential was raised by inward current flow the resistance rose to several hundred ohm cm^2.

Another observation made at this time (45) with longitudinal current flow was that at low frequencies (a few hundred hertz), the membrane had the characteristics of an inductance in parallel with the capacitance. This was puzzling because, as Cole pointed out in a paper (46) discuss ing these two phenomena of rectification and inductance, it is not reasonable to suppose that the apparent inductance is due to creation of a magnetic field. However, Cole drew attention in that paper to known physical systems which have inductive characteristics unrelated to magnetism and one of these was important because it suggested the mechanism shown later to underlie the inductive behaviour of the nerve membrane. This was the carbon filament lamp, in which the electrical resistance falls with rise of temperature so that when a constant voltage is applied and the filament heats up, the current rises slowly just as it does through an inductance in series with a resistance.

During the War, it became clear both to Cole and to Hodgkin that an understanding of the excitation process would be greatly helped by an experiment in which the potential difference across the membrane was controlled by the experimenter and the time course of current through the membrane was measured; the unstable character of the membrane which underlies the 'all-or-none' character of the propagated impulse would be overcome if the impedance of the circuit controlling the potential were low enough. Experiments on the squid nerve were necessarily postponed until after the War, but in 1941 Cole suggested to J.H. Bartlett that he should do the analogous experiment on the 'iron wire model': it had been known for many years that an iron wire, passivated in strong nitric or sulphuric acid, would propagate a short-lived period of reactivity on electrical stimulation in a way that had many analogies with nerve conduction. Bartlett (1945) used a low-resistance potentiometer to apply step changes of potential to a piece of iron with a small surface exposed to sulphuric acid of appropriate concentration and recorded the current using a D.C. amplifier and cathode-ray oscillograph.

The first apparatus for making this type of measurement on nerve (the squid giant fibre again) was built shortly after the War by Marmont in Cole's Institute. He used a long internal electrode so that the internal potential was held almost uniform over its length and the current passing from the electrode through the membrane was controlled by a feedback circuit; at Cole's suggestion he arranged that it was also possible to control the current by feedback from the potential on the internal electrode. In the latter mode, the time course of current could be recorded when the membrane potential was caused, by a command signal, to undergo a step change to a new constant level, a device that came to be known as the 'voltage clamp'. The only full account of their experiments with this equipment (in 1947) is a paper under the authorship of Marmont alone (1949); it gives detailed descriptions of the apparatus and of one type of experiment performed with it, in which a stimulating pulse of current was passed through the membrane via the internal electrode and subsequently the membrane current was held at zero by the feedback circuit. The result was an action potential in which the time course was not complicated by longitudinal current flow; however, it was not very different from an ordinary propagated action potential.

Despite Marmont's preference for the controlled-current mode, a few records were taken in the voltage-clamp mode; these were reported briefly by Cole (57) at a conference held in Paris in the spring of 1949. Strikingly, these records did not show an all-or-none response to reduction of the potential difference across the membrane, but over a certain range of membrane potential there was a phase of inward current that clearly would have generated an action potential if the feedback from recorded potential had not prevented it. The records showed qualitatively all the main features that Hodgkin and I found in our experiments in

1948 and 1949, except that the late outward current was not well maintained. This was due to polarization of the electrode by the rather large current density that had to be passed through its surface; this error was avoided in our experiments by using two internal electrodes, one for potential measurement and the other for passing current. The main difference between their work and ours was, however, that they did not use solutions with altered sodium concentration and therefore were unable to analyse the current into components carried by sodium and by potassium ions.

In one respect, Marmont's records of 1947 were better than ours of 1948: they showed an appreciable lag between the step of membrane potential and the rise of the transient inward current (due to sodium entry). At the Paris meeting, Cole cited this as evidence against the theory that we tentatively proposed at that meeting (which made sodium current an instantaneous function of membrane potential) whereas we attributed the lag to instrumental delays. Later we fully confirmed the existence of the lag as a genuine feature of the membrane response, and it was an important factor in determining the formulation that we finally adopted in our mathematical representation of the permeability changes (Hodgkin & Huxley 1952).

Cole made many investigations on voltage-clamped squid fibres in the following years, in collaboration with J.W. Moore and later R.E. Taylor, but this work was not published until 1960 and 1961 (72-78). They used a second electrode for potential measurement, thus eliminating errors due to polarization of the current electrode, but, unlike the potential electrode used by Hodgkin and myself, this was a glass micropipette, with the advantage that the potential was recorded from just below the surface membrane but also the disadvantage that the high resistance of the electrode, together with its capacitance and that of the input stage of the amplifier, put a limit to the speed of response of the feedback system. They recorded much larger ionic currents (up to 5 or 10 mA cm^{-2} peak inward current) than had been obtained by Hodgkin and myself or by Cole in his experiments with Marmont; an unwanted result of this improvement (due partly to better condition of the nerve fibres and partly to the use of applied current to raise the resting potential) was that the feedback was not always able to control the membrane potential fully, and 'notches' and oscillations appeared in the current traces. These were taken by some critics as invalidating the conclusions drawn from voltage-clamp records, but Cole and his colleagues showed by their extensive experimental and theoretical investigations that these irregularities were instrumental artifacts, arising only when the surface resistance of the long internal electrode was not low enough to ensure that at each instant the potential difference across the fibre membrane was uniform over the length from which the current was being recorded. It is fortunate that the fibres used by Marmont and Cole in 1947, and by Hodgkin and myself in 1948-49, did not produce large enough inward currents to cause instabilities of these kinds.

Cole & Moore (75) also investigated the delay with which the current carried by potassium ions rises following a sudden depolarization of the fibre (interior of the fibre made positive from its resting negative potential). It was already known that this delay increased when the resting potential was artificially raised by means of applied current, but Cole & Moore found that when the resting potential was raised to an extreme level, the increase in the delay was greater than could be plausibly explained by the mechanism that Hodgkin and I (1952) had proposed; this effect has not yet been given a satisfactory explanation. Cole & Moore (75) confirmed however that our formulation gave a satisfactory fit within the range of membrane potential that occurs in a living fibre.

Cole's last major contribution was his book *Membranes, ions and impulses* (93), published in 1968. This book gives a very full account of work in the field covered by its title, putting Cole's own work into relation with that of others. As regards Cole's own work, it not only collects his contributions in a consecutive and readily accessible form, but presents much of his thinking and theory that do not appear in his papers published in the scientific journals. More about the origins of his interests is to be found in his autobiographical chapter in the 1979 issue of the *Annual Review of Physiology* (Cole 1979) and in an article in the *Annual Review of Neuroscience* (Cole 1982). Two collections of essays in his honour have been published (Agin 1972, Moore 1976).

ACADEMIC HONOURS

Member, US National Academy of Sciences, 1956

Hon. Doctor of Science: Oberlin College 1959; University of Chicago, 1967

Hon. Doctor of Medicine, University of Uppsala, 1967

National Order of the Southern Cross, Government of Brazil, 1966

National Medal of Science, 1967

Foreign Member of The Royal Society, 1972

In 1973, the Membrane Section of the Biophysical Society (U.S.A.) established an annual 'Cole Award' in his honour.

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Courtesy of the University of Utah

HENRY EYRING

February 20, 1901-December 26, 1981

BY WALTER KAUZMANN

HENRY EYRING WAS FORTUNATE in entering the arena of chemical physics at the time that quantum mechanics began impinging on the fundamental problems of chemistry. He was also fortunate in possessing to an unusual degree a fertile imagination, unbounded curiosity, a warm and outgoing personality, a high degree of intellectual talent, the ability to work hard, and a determination to succeed. The result was that, beginning in the early years of the 1930s, he exerted an important influence on the large numbers of students and colleagues lucky enough to come into contact with him. This influence continued to spread throughout the chemical community for the rest of his life.

He broke new ground in a wide sweep of scientific activities, involving matters that ranged from fundamental principles of chemistry to problems of a highly practical and applied nature. Some of his ideas contain elements that remain controversial and a considerable number of contemporary scientists continue to work on them.

Eyring was born in 1901 in the prosperous Mormon community of Colonia Juarez, Mexico (about 100 miles south of Columbus, New Mexico). He was a third generation Mormon, his grandparents on both sides having participated in the early migration (1850-60), first to Salt Lake City and then to outlying communities. The move to Mexico by his grandparents took place in the late 1880s following the admission of Utah to the United States and the consequent persecution of those Mormons who refused to accommodate to the new state of affairs. Henry was born and raised as a Mormon and he remained a devoted follower of that faith throughout his life.

Henry's father was a successful cattle rancher and Henry was riding "as soon as my legs were long enough to straddle a horse." But the beginning of the Mexican revolution in 1910 destabilized the political situation to such an extent that 4,800 of the colonists migrated to El Paso, Texas, in mid-July 1912. They left behind them essentially all that they owned, expecting that conditions would return to normal. This did not happen; they spent a year of penury in El Paso. After another year struggling to make ends meet in small towns in Arizona Henry's father purchased a small farm near Pima. Hard work by all members of the family was required to clear the land, but in a few years they began to get back on their feet again.

Henry had finished the fifth grade by the time he left Mexico. A year of schooling was missed in El Paso, but he was able to skip several grades and graduate from eighth grade at Pima in 1914. He then attended Gila Academy, a church school near Pima, graduating in 1919. He did especially well in mathematics and science and was encouraged by one of his teachers to go into engineering at the University of Arizona. Winning a state fellowship there, he decided to study mining engineering. By assisting in classes and waiting on tables he was able to earn enough, not only to support himself, but also to send some money home "to help with payments on the farm."

During the summer after his junior year he gained expe rience in the mining profession working underground in a mine. Repeated exposure to danger, observation of a number of bad accidents, some of them fatal, and the realization that as a supervisor in a mine he would be sending others into dangerous situations (quite aside from the dangers to himself) caused Henry to change his field of specialization from mining to metallurgy. So, after graduating in 1923 with a B.S. in mining engineering he continued his studies at the University of Arizona and obtained a master's degree in metallurgy in the spring of 1924. A summer's exposure to the sulfurous fumes of a smelter, however, caused another decision to change careers; Henry returned to the University of Arizona as a chemistry instructor for the academic year 1924-25. Here his promise as a chemist was recognized by several faculty members and he was encouraged to go on for a Ph.D. He was accepted as a graduate student at Berkeley, where he received his Ph.D. in 1927. His thesis work was under the direction of Professor George F. Gibson and involved a study of the ionization of various gases by alpha particles from polonium, as well as the stopping powers of these gases.

He continued this work on the interaction of gases with alpha particles during the 1927-28 academic year as an instructor in the chemistry department at the University of Wisconsin. In his second year at Wisconsin he received a postdoctoral appointment in the laboratory of Farrington Daniels, where he studied the decomposition of N_2O_5 in various solvents. It was here that he began working in chemical kinetics, a field that was to remain so central to his interests and reputation for the rest of his life.

His work with Daniels resulted in the award of a National Research Council fellowship at the Kaiser Wilhelm Institute in Berlin from the summer of 1929 until the summer of 1930. His chief collaboration there was with Michael Polanyi.

This was shortly after Heitler and London had explained the covalent bond by writing a reasonably successful wave equation for the hydrogen molecule and Fritz London was close at hand, not to mention Eugene Wigner and others who were fully aware of the possibilities lying in wait for the quantum mechanical attack on basic chemical problems. The notion of describing a chemical reaction in terms of a potential energy surface had been formulated and Eyring and Polanyi decided to try to perform a quantum mechanical calculation of the surface for the reaction $H + H_2 \rightarrow H_2 + H$. This turned out to be a formidable problem, but by introducing clever and intuitive approximations a surface was produced. The following is excerpted from *Annual Review of Physical Chemistry* 28 (1977):1-13:

> This way we got an exciting, if only approximate, potential surface and with it gained entrance into a whole new world of chemistry, experiencing all of the enthusiasm such a vista inspired. We perceived immediately the role of zero point energy in reaction kinetics and our method . . . made it possible to extend our calculations to all kinds of reactions.

In the fall of 1930 Eyring returned to Berkeley on a one-year appointment as instructor in the chemistry department. He applied his method of calculating potential energy surfaces to the reactions of hydrogen with the halogens and was able to explain why hydrogen and iodine reacted by a bimolecular collision of H_2 molecules with I_2 molecules, whereas the reaction of hydrogen with bromine and chlorine involved an atomic mechanism. He also made the remarkable prediction that hydrogen and fluorine would be unreactive at room temperature, which was in conflict with the currently accepted observations. This work was reported at the Indianapolis meeting of the American Chemical Society. In the audience was Professor Hugh Taylor of the Princeton University chemistry department. Taylor was im mediately impressed by the enormous potential of Eyring's efforts. He was also aware of recent work that showed that the observed explosive reaction of hydrogen and fluorine with hydrogen was caused by surface catalysis, and that if the mixture was prepared in the absence of surfaces no reaction occurred at room temperature, just as Eyring had expected. Taylor invited Eyring to come to Princeton to give some lectures, and thus began his fifteen-year connection with Princeton University.

During the academic year 1931-32 Eyring held an appointment in the Princeton chemistry department with the title of research associate with rank of instructor, and from 1932 to 1936 his title was research associate with the rank of assistant professor. It was only in October 1936 that he was given a regular faculty appointment, but as associate professor with promotion to full professor following in April 1938. His research activities during this period were intense and it was at this time that his reputation became established.

Calculation of the potential energy surfaces was continued. The concept was applied, for instance, to problems in surface catalysis. The constant thinking in terms of these surfaces led in due course to what is probably Eyring's most important scientific contribution: the development of the notion of the activated complex as an entity controlling the rates of chemical reactions with a definite mean lifetime and capable of treatment in rigorous thermodynamic and statistical mechanical terms. As is well known the paper presenting this idea was first rejected when submitted to the *Journal of Chemical Physics,* but the editor was persuaded to change his mind and the paper appeared in 1935. It should be mentioned, however, that the validity of the basic assumptions of this theory frequently have been questioned and discussion on this continues to this day. Nevertheless, it is generally conceded that the theory provides a highly useful framework for the interpretation of chemical reaction rates.

Eyring became involved in a great many other activities that were taking place in the Frick Laboratory at the time. Hugh Taylor was engaged in the successful effort of preparing pure heavy water, and many papers appeared on its properties, many with Eyring as a co-author. He also became very actively interested in constructing models that would explain the existence and properties of the liquid state. The activated complex was applied to dynamic properties such as viscosity and diffusion. The preparation of heavy water in Frick was accomplished through electrolysis, and the concept of the activated complex was applied to electrolytic processes and to the phenomenon of overvoltage. The role of the zero point energy in the separation of isotopes was recognized. With Professor E. U. Condon of the physics department a new theory of the origin of optical rotatory power was developed. Seventy-five papers bearing Eyring's name and based on his work at Princeton appeared between 1932 and 1940.

Eyring's active mind was constantly coming up with interesting ideas, some of which did not work out. One of these is sufficiently amusing to deserve mention. Eyring came upon the fact that the onset of turbulence in laminar flow occurs when the Reynolds number reaches a value of 2000. It occurred to him that turbulence might be somehow a consequence of the uncertainty principle. He pointed out that in only one substance (hydrogen gas) is the ratio of the total mass of the electrons to that of the nuclei different from that in matter in general (nuclei other than hydrogen have roughly equal numbers of protons and neutrons, so that the ratio of the total mass of the electrons to that of the nuclei is half that in hydrogen, and this differ ence holds for deuterium as well as for hydrogen). Might it not be possible that turbulence in hydrogen sets in at half the normal Reynolds number for the onset of turbulence in deuterium? An appropriate apparatus was set up and measurements were made. Unfortunately no difference was observed. (These measurements were performed by Professor William Roseveare.) In my opinion shots in the dark such as this are the mark of a certain kind of genius that sometimes lead to very important scientific advances.

The war years brought Eyring into contact with still other areas of research. A major classified project dealt with the theory of detonation. The work of Professor Frank Johnson in the Princeton biology department on the effects of temperature, pressure, and narcosis on luminescent bacteria led to an extensive collaboration. In 1944 the Textile Research Institute was relocated in Princeton, and Eyring became heavily involved in its research program. A large number of papers on the mechanical properties of textiles came from this. Eyring was also involved in a heavy schedule of extracurricular teaching in several of the defense-related industries of North Jersey. The Princeton years from 1941 to 1947 resulted in fifty more papers bearing Eyring's name. Eyring was elected to the National Academy of Sciences in 1945.

In 1946 Eyring was approached by the University of Utah to explore his interest in becoming the dean of its graduate school, with the aim of building a major research activity at the university. The attraction of Salt Lake City was strong. While at Wisconsin Eyring had married Mildred Bennion, also a devout Mormon. The marriage had produced three sons, Edward Marcus, born in Oakland in 1931, Henry Bennion born in Princeton in 1933, and Harden Romney born in Princeton in 1936. Mildred especially was concerned about the problems of raising her sons in New Jersey, so far from the center of the Mormon faith. Professor Taylor was unable to convince either Eyring or his wife that it was in Eyring's best interests to remain in Princeton. And so began the final thirty-five years of Eyring's career, in which highly successful activities as an administrator were added to a continuing productive scientific output.

In Eyring's Utah years approximately 485 papers appeared bearing his name. The range of topics covered is astonishing. Areas that concerned him at Princeton continued to maintain his interest, but many other areas attracted his attention, involving pure, basic science, and highly applied matters. Eyring also did a great deal of consulting and traveled widely, giving many talks. He was a master at communicating his ideas, and a great many honors came to him.

In view of current controversies over the teaching of creationism and Darwinism, Eyring's intellectual interactions with the Mormon church, and particularly his opinions on the relationship of science to the scriptures, deserve special mention. It was his position that the interpretation of the scriptures was up to the individual members of the church, and it was not appropriate for the leadership to declare what positions on scientific matters were "correct" or "incorrect." The Mormon church had assumed a relatively liberal view of the relationship of science to the church, but in 1953 Joseph Fielding Smith, president of the Council of Twelve Apostles, began to express his opinion that the scriptures must be interpreted as literally true on scientific matters. In a number of meetings and writings Eyring tactfully yet forcefully engaged President Smith in the matter. His point is summarized in the following:

> The church is committed to the truth whatever its source and each man is asked to seek it out honestly and prayerfully. It is, of course, another matter to teach as a doctrine of the church something which is manifestly

> contradictory and to urge it in and out of season. The author has never felt the least constraints in investigating any matter strictly on its merits.

The consequence of this interchange seems to have been that Eyring's position received official support. (The fascinating story of this incident is told in some detail in the thesis of Steven H. Heath, which is referred to in the references section of this memoir).

One aspect of Eyring's personality that was particularly appealing was his thoughtfulness toward all who came in contact with him, regardless of their station in life. He has written of "how important it is to care about people even when they are small and may not seem very important." I myself observed numerous occasions in which he practiced this principle. He once remarked on the importance of being good to people whom you pass "on the way up," because you will want them to be good to you when they pass you on their way up and you are on the way down.

Eyring had a serious encounter with cancer in 1969, which was thought to have been successfully treated. Unfortunately the cancer returned and in his last years his health declined, though he continued to work hard and productively and maintained a cheerful outlook, undoubtedly strengthened by the faith that had sustained him throughout his life. He died in Salt Lake City two months after a large meeting was held in Berlin to celebrate the fiftieth anniversary of his famous paper with Polanyi, "Über einfache Gasreaktionen."

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Many of Eyring's students, friends, and collaborators have written about him. He had a colorful personality and some delightful stories will be found among the following references.

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Alfred Gilman

ALFRED GILMAN

February 5, 1908-January 13, 1984

BY MURDOCH RITCHIE

ALFRED GILMAN, AN INTERNATIONALLY renowned pharmacologist who died on January 13, 1984, will be remembered by countless scholars for his co-authorship of the seminal textbook on the pharmacological basis of therapeutics, by generations of medical students for his brilliant and inspiring lectures, and by his many friends for his warmth and concern for their personal problems. As for his scientific achievements, he played a key role in our understanding of how the ionic milieu of the body is maintained and he pioneered the first clinical trial that established the use of chemotherapy in the treatment of cancer.

Alfred Gilman was born in Bridgeport, Connecticut, on February 5, 1908. He graduated in 1928 from Yale College and remained as a graduate student in the Department of Physiological Chemistry, as many departments of biochemistry were called at that time. In 1931 for his dissertation on "Chemical and Physiological Investigations on Canine Gastric Secretion," he was awarded a Ph.D. degree in biochemistry. Although his heart was set on taking a medical degree to become a research clinician, the exigencies of the Great Depression led him to postdoctoral fellowships at Yale, first in the Department of Biochemistry for a year in 1931 and subsequently in the Department of Pharmacology. Thus, a career that changed the face of pharmacology was started.

The switch from biochemistry to pharmacology was one of those fortuitous events that was of no apparent significance at the time. G. R. Cowgill, Gilman's mentor in the biochemistry department, was studying the osmotic relations between blood and gastric juice. The main instrument used for this at the time, which depended on the depression of the freezing point, had a limited resolution and accuracy. By chance the young Gilman heard a seminar given by a visiting professor from Britain (A. V. Hill, Nobel prizewinner for his work on the mechanism of muscle contraction) in which was described the use of an extremely sensitive vapor-pressure method for determining the osmolality of very small volumes of fluid. Gilman soon arranged for A. V. Hill's technician in London (A. C. Downing) to make for him the sensitive thermopile and galvanometer that was needed, and then used the method with great success. It happened that the chairman of pharmacology at the time (H. G. Barbour) was also interested in osmotic changes in blood and body fluids that resulted from various procedures. As a result (at least according to Gilman) Barbour had little hesitation in offering this postdoctoral fellow in biochemistry with his state of the art methodology, first a postdoctoral fellowship and subsequently an assistant professorship in pharmacology. He remained in pharmacology for the rest of his career.

While still a postdoctoral fellow in pharmacology Gilman first met Louis S. Goodman, who had just completed an internship in medicine at Johns Hopkins University School of Medicine. Both had a compelling interest in pharmacology, which they began teaching jointly. It soon became clear to them both that the textbooks of the time were outmoded and inadequate for the teaching of medical students. They were either poorly written for the needs of medical students or else failed to reflect the latest findings in drug therapy—or sinned on both counts. By now assistant professors, the two young lecturers decided to write a text originally for the use of Yale students. Their aim was to correlate pharmacology with related medical sciences, to reinterpret the action and uses of drugs in the context of the important advances in medicine that were being made at the time, and to emphasize the application of pharmacodynamics to therapeutics—a momentous change to be undertaken by two junior faculty members, both barely thirty years old. Hearing of their project, John Fulton, professor of physiology, introduced the young authors-to-be to the Macmillan Publishing Company; the rest is history.

The editors at Macmillan, expecting a book of about 450,000 words, were aghast at the size of the manuscript that Gilman and Goodman submitted; it was nearly a million words long, all written in longhand. Despite heated discussion the young authors refused to make any cuts in the text; and, after a day of arguing, Macmillan agreed to publish the manuscript as submitted at the then unheard of price of $12.50 per copy (most medical books at that time sold for about $8.00). Needless to say, it became an immediate best-seller. It has remained so through nine editions.

Despite its length the book published in 1941 was not a compendium of drug data. Rather, it was the reinterpretation of the actions and uses of well-established therapeutic agents in light of recent advances in the medical sciences; and the time was ripe. The sulfa drugs had already been introduced, and over a hundred pages were devoted to them in the first edition. Modern chemotherapy had been born, to be followed by (and described in subsequent editions) the antibiotics, the antimalarials, and a host of chemotherapeutic agents that were specific against particular diseases.

The book was explicitly written as a textbook for physicians and medical students; and this was made clear in the first edition's subtitle. Emphasis throughout the book was clinical. Medical students, they believed, must be taught pharmacology from the standpoint of the actions and uses of drugs in the prevention and treatment of disease; and the practicing physician must be offered an opportunity not only to keep abreast of recent advances in therapeutics but also to acquire the basic principles necessary for the rational use of drugs in daily practice.

The book was an immediate success. Within three years 28,000 copies had been sold and by the time the second edition appeared in 1955 the first edition had sold more than 86,000 copies—a record for a pharmacology textbook. Subsequent editions have continued to enjoy record-breaking sales. Within a few years of the first edition, however, it became clear to the authors that the Blue Bible (after the color of its cover) needed thorough revision. The drug revolution was underway. Steroids, CNS compounds, new antimalarials, and antihistamines had been introduced, and the antibiotic era had been started with the introduction of penicillin. Any immediate revision, however, was prevented by the intervention of the Second World War so that the second edition did not appear until 1955. Much of the reason for the further delay after end of the war was the breakneck speed at which pharmacology and therapeutics were moving. Each time the last of the chapters had been rewritten, the earlier chapters were already out of date and had to be revised. By the time the new second edition eventually came off the press, it had become clear to both authors that the whole field of therapeutics had become too extensive and too specialized for two men to encompass all areas appropriately. The 1955 edition, therefore, was to be the last edition written by themselves alone. Two decisions were made: first, to revise the book every five years (a goal they met), and second, to call on other coauthors (forty-two in all) preeminent in their fields of specializations, most of whom were current or former associates of Gilman or Goodman. The fact that the book from the third edition on had multiple authors did not mean that either Gilman or Goodman abrogated their responsibilities. If the book was to maintain its readability, cohesiveness, organization, and most essentially, its philosophy and objective, strict editing by them was mandatory. This requirement was rigorously met; and, equally important, the timetable was kept. New editions appeared regularly every five years; in 1995 it was in its ninth edition. Gilman died before the seventh edition appeared (1985). Goodman relinquished his role prior to the eighth edition; Gilman's son, Alfred Goodman Gilman, who became the senior editor at the time of the sixth edition, will continue as the Blue Bible's consultant editor.

Gilman could not, from the very beginning, hide his obvious love affair with teaching and the responsiveness of those he taught, whether medical students or professional physicians. At Columbia University's College of Physicians and Surgeons in New York, which succeeded Yale as Gilman's academic home, the Department of Medicine had weekly staff conferences (grand rounds) always attended by the full faculty with the chairman of the department together with the senior professors in the front row and then behind them in due hierarchical structure, the associate professors, the assistant professors, the instructors, the house staff, as well as the senior and junior medical students in the rows behind. Discussion of patients' medical problems ranged from the commonest to the most perplexing and esoteric. Into this impressive arena Alfred Gilman appeared in 1946.

Not only appeared, but became a leading participant, commenting on all issues of pathophysiology and pharmacotherapy. He was soon counted on to add to the educational importance of these weekly meetings.

Wherever he taught he had tremendous impact, not just on the graduate students but also (and most particularly) on the much larger numbers of medical students. His lecturing style was leisurely. The facts and "information bits" presented during the hour were small in number, and they were often presented in the context of humor. But these "bits" had been carefully selected as the cardinal points at issue; and twenty years after such a lecture the students (by now practicing physicians or academic faculty) would still remember. As a lecturer Gilman was superb. He had an exceptional combination of modesty, good sense, and humor. For example, he gave the acid-base lectures in pharmacology. For this topic he would make sure to get to the lecture room well ahead of the students and fill the blackboard with the mathematical derivation of the Henderson-Hasselbalch equation. When the students arrived their jaws would drop visibly on seeing what they thought was in store for them. Gilman would then start his lecture by announcing that he was not going to go into the derivation of the equation, at which point he would erase most (but not all) of the blackboard. He would then continue with an hour of applied pharmacology, all beautifully illustrated by the final equation itself which had remained untouched. He taught the students how to *use* the equation rather than just how to derive it. What Gilman liked was actually teaching, not just talking about it, whether formally in a lecture or informally in a seminar. His office was always rather small and on the main corridor, and the door was always open, both literally and figuratively. Students would often drop in for a brief visit just to get something straightened out and the house staff would wander in just "to see what Gilman thinks."

In his teaching Gilman guided his students through the maze of pharmacology by selecting and emphasizing key issues and letting the unimportant ones take care of themselves. As John Kemeny, president of Dartmouth, said on the occasion at which Gilman was awarded an honorary degree, "Far more than an isolated exercise in pharmacology, your book has provided for generations of students and practitioners the essential but difficult bridge between the basic medical sciences and the practice of medicine. Indeed it could be said that long before the concept of an integrated curriculum became a popular educational philosophy in medical schools, it was a reality in the form of your textbook."

During the Second World War Gilman left Yale for army service as chief of the Pharmacology Section in the Medical Division at Edgewood Arsenal, Maryland, with the rank of major. These war years were largely spent in trying to develop antidotes for the nerve gas organophosphates and for the nitrogen mustards, both of which it was feared would be used against American troops. Even before he left Yale early in 1942 a contract had been signed between Yale University and the Office of Scientific Research and Development to investigate these chemical warfare agents. The study of the nitrogen mustards was assigned to Alfred Gilman and Louis S. Goodman. Early in the course of their study of the nitrogen mustards, it became apparent that the agents were cytotoxic following absorption; in particular they destroyed lymphatic tissue. After the nitrogen mustard treatments were shown to cause regression of experimental lymphoma in mice (and not many days after!) Gustaf E. Lindskog, an assistant professor of surgery, was persuaded to supervise a clinical trial on a patient in the terminal stages of lymphosarcoma which was resistant to X-ray therapy. The response of this first patient was as dramatic as that of the first mouse. Within forty-eight hours after initiation of therapy, softening of the tumor masses was detected. By the fourth day cervical masses were no longer palpable and a few days later the axillary masses had completely receded; however, as one might have anticipated from the mouse studies, the tumor slowly regenerated. A subsequent course of therapy resulted in only partial improvement and a third course had relatively little effect. That the treatment was only a partial success is irrelevant. The point is that tumor growth had been clearly shown to be susceptible to chemotherapy, and treatment was no longer limited just to radiation or to radical surgery. From this insightful beginning medical oncology grew and now is one of the recognized medical subspecialties.

Much of Gilman's immense contribution to the field of therapeutics is not immediately apparent, for it came outside academia in a lifetime of acting as consultant to several pharmaceutical companies (for example, Burroughs Wellcome and Smith Kline French) who sought his advice. Gilman did not see academia and industry as being completely independent entities or rivals. Rather, as far as therapeutics was concerned, they were quite complementary, each depending critically on the other. Academia was the basis for training each new generation of pharmacologists, but the final development of new agents was the province of industry—with, of course, the counseling of academia. One example comes from the field of diuretics in which Gilman had a lifelong, passionate interest. Following therapy with many diuretics there is often an increased secretion of potassium by the kidneys. In otherwise healthy patients, the consequent hypokalemia may be of little clinical consequence; however, it may cause cardiac arrhythmias and high-risk pa tients who have symptomatic coronary disease or congestive heart failure need to be protected. These considerations led to Gilman's participation with one pharmaceutical company (SKF) to develop a potassium-sparing diuretic. The first drug to be tested was a failure; the second taken orally worked excellently in all experimental animals tested, but, unfortunately, not in the human! The third drug (triamterene) was a success and it has become part of the present-day pharmaceutical armamentarium. Gilman continually cited this experience as an example of the value of the interdependence of industry and academia. The latter may provide sage counsel, but only the industrial company can gather multidisciplinary teams of appropriate magnitude that lead to ultimate success and afford the many expensive failures (of which the public usually remains unaware) on the road to success.

Among Gilman's scientific achievements were several important contributions to our understanding of kidney function. Exploring the effects of administering large amounts of urea to experimental animals, Gilman and his junior colleagues, Mudge and Foulks, were surprised at the excretion of large amounts of potassium that the diuresis induced. They were able to show that the rate of excretion was too great to be attributed to glomerular filtration alone and that potassium must, therefore, be actively transferred from blood to urine by the renal tubules. This was particularly surprising because the amount of potassium usually excreted in the urine is only some 5% or 10% of the amount in the glomerular filtrate so that filtration and reabsorption would alone easily explain potassium excretion under all but the most unusual circumstances. Nevertheless, Gilman suggested what seemed at the time an outrageous idea, namely, that all or most of the filtered potassium was reabsorbed and that what eventually appeared in the urine was added by secretion by the tubules. Subsequent work by others has shown that this insightful conjecture was indeed correct.

Another major finding was Gilman's demonstration with Brazeau that an increase in the CO_2 tension of the blood increased the capacity of the renal tubules to reabsorb bicarbonate. Such an increase in CO_2 tension occurs in many forms of pulmonary disease leading to the condition known as respiratory acidosis. It was well known that respiratory acidosis is associated with a compensatory increase in the concentration of bicarbonate in the blood, a compensation that raises the pH toward a more normal value. The finding of the effect of respiratory acidosis on renal bicarbonate reabsorption was a nice explanation for the phenomenon.

Alfred Gilman was truly a national and international figure. He was elected to the National Academy of Sciences in 1964. In 1967 he became chairman of the National Academy of Sciences's Drug Efficacy Review Committee and the work of the numerous expert panels that he oversaw was a landmark in modern therapeutics. No longer could claims of efficacy be made without the support of objective evidence. For example, at an international meeting in Geneva an irate clinician challenged Gilman for classifying the physician's treasured bioflavonoid as ineffective. Speaking as an experienced physician, the clinician assured Gilman that he knew that this drug was valuable in the treatment of asthma. Gilman listened attentively and then calmly noted that the bioflavonoid, which was given by mouth, was not absorbed at all from the gastrointestinal tract!

Gilman left Columbia University in 1956 to become professor and chairman of the Department of Pharmacology at the newly founded Albert Einstein College of Medicine. The young faculty that he attracted to the department was soon recognized for its excellence in research, however teaching continued to be of major importance. Gilman went to most lectures, as did all the faculty. It was not uncommon for the lecturer on finishing his didactic hour to be politely asked to come with Gilman to his office where it would be suggested, in the kindest possible way, that the lecturer had perhaps been inaccurate at some point or, much more likely, had laid insufficient emphasis upon a key point. Gilman's appointment as chairman coincided with the initiation of the medical scientist training program, the combined M.D./Ph.D. program. Einstein was the recipient of one of the first three grants from the National Institute of General Medical Sciences of the National Institutes of Health. Gilman was the creator as well as heart and soul of this program, which has since been running for three decades and which is considered one of the most successful in the nation. In 1964 he assumed the additional responsibility of becoming the first associate dean for graduate studies at Albert Einstein.

Gilman's dedication and devotion to pharmacology and science were passed on to all those fortunate to have been touched by him. He wanted students to know and understand how drugs acted in the whole animal, rather than just in isolated tissues in experiments or in the test tube. He was more than just a teacher or head of a department; he was a gentle, caring human being with whom one could share one's problems, whether career-related or of a personal nature. He would sit one down and give advice in a personal, fatherly manner, That he was nearly always available as mentor was considerably helped by the fact that unlike many scientists of his intellectual seniority, he spent most of his time on the home ground of his department rather than traveling worldwide to this or that "important" symposium. He served as an excellent role model for his students, postdoctoral fellows, and junior faculty. His keen intelligence, sharp wit, consummate ability to make complex problems simple, and his unexcelled teaching ability were greatly admired.

On retiring from Einstein, Gilman returned to Yale as a lecturer in his old Department of Pharmacology, where three of his "young men" from Einstein were now full professors. There, untroubled by any administrative responsibilities, he did what he always like doing best—talking to graduate and medical students about their problems and discussing the research problems of the faculty. His door was always open and his encyclopedic knowledge freely available. He volunteered his services to his alma mater joyously, out of his love for pharmacology and students, and entered into the regular teaching of the pharmacology course at Yale. He taught with vigor, enthusiasm, and great effectiveness into his seventy-sixth year. Indeed, on the very day he died he was to have given the concluding lecture in the pharmacology course, an overview of the status of pharmacology and therapeutics that was eagerly awaited by students and faculty alike.

Alfred Gilman had a love for life, and he did not neglect his family. He had great fun with music and would play the saxophone or violin or almost anything else, particularly at neighborhood parties. He could essentially pick up any instrument and play any tune without music. This interest and talent developed in early boyhood, undoubtedly stemming from the fact that his father, Joseph Gilman, owned the Gilman Music Store in Bridgeport. Music continued to be important throughout his family life. This love of music was shared by his wife Mabel (nee Schmidt) whom he married in 1934. Mabel, the daughter of a professional trombonist, was herself a talented pianist.

Gilman was extremely keen on fishing, which helped to nurture the collegial relationship he had with his son Alfred

Goodman Gilman (Nobel prizewinner in medicine in 1994), fishing together from a rowboat in Long Island Sound. Later, when the Gilman's acquired a place on Cape Cod, summer fishing and clamming was Gilman's joy, particularly when he could have his children along. Joanna, his daughter, tells how she learned from observation of her father how not to be lonely with oneself, for Gilman could sit quietly and not be idle because he was thinking and working things through. Even the book had a family basis. The dedication of Mabel, his wife, to the preparation of the second and subsequent editions of the textbook of pharmacology was awe-inspiring to those who appreciated the formidability of the task with its endless combing of the basic science and clinical literature, abstracting, typing, editing, and indexing as the editions of the classic began to take shape and grow. In this day of word processors, computers, Medline, and research staff it must be remembered that the *Pharmacological Basis of Therapeutics* was prepared by Alfred and Mabel on the East Coast by themselves together with Lou Goodman in faraway Utah.

HONORS AND DISTINCTIONS

DEGREES

B.S., Yale University, 1928
Ph.D. (biochemistry), Yale University, 1931

HONORARY DEGREE

D.Sc., Dartmouth College, 1979

UNIVERSITY APPOINTMENTS

Research fellow, biochemistry, Yale School of Medicine, 1931-32

Research fellow, pharmacology, Yale School of Medicine, 1932-35

Assistant professor of pharmacology and toxicology, Yale School of Medicine, 1935-43

Captain and major, Sn-C., A. U. S., chief, Pharmacology Section, Medical Division, S. W. S., 1943-46

Associate professor of pharmacology, College of Physicians and Surgeons, Columbia University, 1946-48

Professor of pharmacology, College of Physicians and Surgeons, Columbia University, 1948-56

Professor and chairman, Department of Pharmacology, Albert Einstein College of Medicine, 1956-73

Associate dean for graduate studies, Albert Einstein College of Medicine, 1964-69

Lecturer in pharmacology, Yale University School of Medicine, 1973-84

MEMBERSHIPS

Sigma Xi
American Physiological Society
Society for Experimental Biology and Medicine
Harvey Society
American Society for Pharmacology and Experimental Therapeutics
New York Academy of Sciences
New York Academy of Medicine
Honorary fellow, American Academy of Allergy
Fellow, American Association for the Advancement of Science

Honorary member, Alpha Omega Alpha
National Academy of Sciences
Fellow, American Academy of Arts and Sciences

PROFESSIONAL AND PUBLIC SERVICE

U.S. Public Health Service:

Member of the Pharmacology and Experimental Therapeutics Study Section, 1946-49 and 1950-55; chairman, 1956-60

Member, Pharmacology Training Committee, 1960-63

Member, Heart Special Projects Committee, 1963-65

Advisory Council on Research, New York Heart Association, 1958-64

Scientific and Educational Council, Allergy Foundation of America

Editorial Board of American Journal of Physiology and Journal of *Applied Physiology,* 1950-56; consulting editor, 1956-57

Editorial Board of *Pharmacological Reviews,* 1948-55

Advisory Council, Cystic Fibrosis Research Foundation, 1960-65

Advisory Council, New York City Health Research Council, 1960-65

President, American Society for Pharmacology and Experimental Therapeutics, 1960-61

National Academy of Sciences/National Research Council:

Member, Division of Medical Sciences, 1962-71

Executive Committee, Medical Division, 1962-64

Member, Drug Research Board, 1963-72

Chairman of Organization Committee, Drug Efficacy Review, 1966-67

Chairman of Executive Committee, Drug Efficacy Review Committee, 1967-69

Chairman, Drug Research Board, 1971-72

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1933 With G. R. Cowgill. Osmotic relations between blood and body fluids. II. The osmotic relation of blood and gastric juice. *Am. J. Physiol* . 103:143-52.

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With G. R. Cowgill. Osmotic relations between blood and body fluids. IV. Pancreatic juice, bile, and lymph. *Am. J. Physiol.* 104:476-79.

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1980 With A. G. Gilman and L. S. Goodman. *The Pharmacological Basis of Therapeutics,* 6th ed. New York: Macmillan Publishing Company.

HENRY GILMAN

May 19, 1893-November 7,1986

BY C. EABORN

HENRY GILMAN, one of the outstanding organic chemists of the century, and one of its best known chemical personalities, died on 7 November 1986. He was born in Boston, Massachusetts, on 9 May 1893, the third member of a family of six sons and two daughters. His father was a tailor, active in trade union affairs. He attended a high school in Boston and from there went on to Harvard University where he received the B.S. degree *(summa cum laude)* in 1915. His first acquaintance with research came during his final year as an undergraduate, during which he worked with Roger Adams on the synthesis of substituted phenyl esters of oxalic acids, demonstrating the use of the new reagent oxalyl chloride; an account of the results appeared in the *Journal of the American Chemical Society* in 1915 (1). This experience was of major importance in arousing Gilman's interest in research and in 1976 he recalled it in the following terms [1]: 'A sheer delight. Here I was, just a senior. We'd work at night until 11 or 12 o'clock, without

Reprinted with permission of the Royal Society, London, England. The original, in *Biographical Memoirs of Fellows of the Royal Society,* 1990, vol. 36, pp. 153-72, includes, on microfiche, the complete version of Gilmania, a full account of Gilman's research contributions, and a complete list of his publications

any compulsion—just for the joy of it.' And in the following year he wrote of it a little more fully [2]:

> In my senior year at Harvard I was interested in doing some research with Adams. This was done as an aside, and either carried no credit or only a small token of credit. Not a little of the work was done at night, and I recall how when the research was completed for the day, often near midnight, we would cross the street to a drugstore on Massachusetts Avenue for a chocolate malted milk.
>
> The experiments were a great delight for me, and he would come in somewhat frequently for chats. He was, of course, most friendly, interested, and helpful. The study was not 'monumental', but it was exciting for each of us; his first direction of research, and my initiation into research.

Gilman's performance was evidently good enough for him to be invited to stay on for postgraduate work with the renowned head of the department of chemistry at Harvard, E.P. Kohler, known widely as the King of Chemistry, or simply The King. Kohler is credited with having introduced the use of the Grignard reagent to the U.S.A., and there is no doubt that his interest in organometallic reagents had a formative influence on the young Gilman; the one paper they published together was concerned with the bromination of a-keto esters, and includes a description of the use of the Reformatsky reaction, which involves an intermediate organozinc compound closely related to a Grignard reagent (2). On the basis of this work he received an M.A. in 1917 (a year late, it is said, because his supervisor neglected to complete the relevant documentation on time) and the Ph.D. in 1918.

During his graduate work he was awarded a Sheldon Fellowship to travel in Europe, and spent periods with H. Staudinger at the Polytechnicum in Zurich and with W.H. Perkin, Jr., at Oxford. He also visited the Sorbonne. His experiences in Europe made a lasting impact on him and he would frequently recall them in conversation even into his 90s. (He took special delight in having met at the Sorbonne Marie Curie, the first woman to win a Nobel Prize for Chemistry, and during a visit to the Royal Society in 1975 was able to realize an ambition to meet Dorothy Crowfoot Hodgkin, the most recent female recipient of that award, and the only other woman to win it outright.) His contact with Staudinger probably stimulated an interest, which persisted for many years, in the reactions of ketenes, isocyanates, and cyanates. More importantly, during his stay in Paris (where he met Victor Grignard) he became fully aware of the great advances in organic chemistry made in France by the use of Grignard reagents and this, following his introduction to them by Kohler, fixed in him a determination to explore their chemistry when he was in a position to undertake independent research.

After completing his doctorate, Gilman accepted an invitation from Roger Adams to join him at the University of Illinois as an Associate Professor, but soon afterwards, in 1919, wishing to be his own master, he moved to Iowa State College of Agriculture and Mechanic Arts (ISC), as an Assistant Professor but in charge of organic chemistry there. His qualities were quickly recognized, and he was made a full professor in 1923, when only 30 years old. When he went to ISC it was a Land Grant college and by no means the major university it later became. Gilman has said [3] that when he arrived 'the chemistry program was very modest, fifteen or eighteen undergraduates and perhaps a dozen graduate students. But there was a nice *esprit de corps.* We were young and enthusiastic. We all worked quite hard. And we had a nice, easy relationship with the students.'

Ames itself was then a small town in the middle of farmland, a rural community with an atmosphere very different from that of Boston, but this was of little consequence to

Gilman, who gave almost all his time to chemistry (and most of the rest to keeping himself physically fit to do chemistry). Fortunately even then the college had an excellent chemistry library, so that there was no question of his being out of touch with advances in the subject. Moreover, Ames was on a main transcontinental railway line and only eight hours' journey from Chicago. This enabled him to attend meetings of the American Chemical Society and other such events without major inconvenience.

His arrival in Ames caused some excitement among the young women of the college, faced with a cultured, tall, upright, distinguished-looking, and athletic young man, with a Boston accent, a Harvard background, and experience of some of the great universities of Europe. And interest was all the greater because he seemed to show no awareness of female charms. A woman who was a student there then (and later became a member of faculty) recalls that she and a friend resolved to try to make his acquaintance and telephoned him at his boarding house, only to find themselves speechless when he was brought to the telephone, and they were never again able to summon the courage to approach him. A few years later, the woman who was to become his wife made contact with him as a result of a bet that she could not induce him to take her out and in winning her bet found that he was much less frightening than they had all imagined.

This young woman was Ruth V. Shaw, a native Iowan, born in 1901, who had attended Henry Gilman's first-year class in organic chemistry on her way to an AB in history, which she received in 1924. She subsequently took an AM in English and speech at Cornell University, and for three years taught speech at ISC before they were married in 1929. They were a devoted couple and Henry was most fortunate to have such a splendid partner when he became virtually blind, as will be described later.

Gilman began research at Ames without delay, in the early years mainly with Master's degree students, and as soon as 1920 published his first paper based on work there (3). Significantly, it was concerned with the reactions of Grignard reagents, in particular the course of their reactions with ketenes, and was his first independent step in his lifelong devotion to organometallic chemistry. His first student to receive a Ph.D. is said to have been R.M. Pickens, who was awarded the degree in 1925; his work was concerned with the chemistry of derivatives of furan, thiophene and pyrrole (in particular with their effectiveness as local anaesthetics), a field in which Gilman was to work extensively in the following years. Pickens later became director of research at Rayonnier Co., and was probably the first of the many of Gilman's students who went on to reach the highest ranks in industry.

Among Gilman's first doctoral students was W.B. King, who later himself achieved distinction as a Professor at ISC and Iowa State University (ISU), as it became in 1959. He worked with Gilman from 1923 to 1927, and has written of the great admiration he developed for him as a scientist and as a man. Gilman was then, and remained for many years until failing eyesight put an end to it, a highly enthusiastic tennis player, who played a hard competitive game, and took special pleasure in playing regularly against King, who was the state tennis champion. Gilman was interested in all aspects of the game (and for some years acted as an assistant coach in the sport at ISC), and in 1926 he and King went together to watch the Davis Cup matches in Pennsylvania. It is likely that none of his other students was ever as close to Gilman, but their relationship, although friendly, nevertheless had the degree of formality that Gilman thought appropriate between supervisor and student. Professor King relates that, although he knew Gilman well throughout his life from 1923, and regularly played golf, tennis, and card games with him, he could never, even as a senior professor at Ames himself, address Gilman as Henry, so great was his admiration for him.

It is clear from accounts by Professor King and others that even by the mid-1920s Gilman was regarded in the college as a great man, and certainly the outstanding scientist on the campus. It is remarkable that in the period 19201929, starting from nothing and with only the very limited resources of ISC available to him, he published 98 papers, mainly in the *Journal of the American Chemical Society*. The demands that Gilman made on research workers under his supervision in the 1920s set the pattern that persisted for the next 50 years or so. He expected total dedication: students were required to be in the laboratory working every day, including Sundays, late into the night, but there is no indication that at that time they resented this. In the 1930s, however, his demands gave rise to much discontent among the 30-40 members of his research group (an abnormally large number for those days), as several correspondents have made clear. To some extent this was probably a reflection of the general unhappiness during the Depression, when there was much unemployment, poverty and uncertainty, but the complaints of Gilman's students of those days centre on three specific aspects of his behaviour. First the unremitting pressure on them to work all and every day, and to produce results each day; second the low stipends he paid them; and third, and the most important, the length of time, and the uncertainty of its duration, that they were normally required to stay before receiving their Ph.D. degrees.

The students certainly had no opportunity to slack. Gilman went around his laboratories three times each day, in the morning, afternoon and evening, questioning each student in turn about what he had done since his last visit, what he was now doing, and what he intended to do next. When a project was at an especially interesting stage the student concerned would be questioned on all three visits. Gilman remembered exactly what he had been told on the previous visit and so the hours in between had to be accounted for. The initial questioning usually took the form 'What's new?' and the answer was followed by the question 'What else is new?' and the answer to that by the further question 'And what else?'. One of his students from those days has written: 'Always we would begin to think that we had done very little. The cunning ones soon realized that you should keep some results up your sleeve to report next week in order to make your achievements seem more impressive.'

He did not visit the laboratory every Sunday, but did so sufficiently often that the students could not risk being absent that day. At all times students in the laboratory were expected to be giving their full attention to experiments in progress, and those caught seizing an opportunity to study for graduate course or preliminary examinations were sharply reprimanded. They had some chance to get away with such study, however, because throughout his research career he tried to avoid coming upon embarrassing situations by shuffling his feet loudly outside the laboratory to give warning of his arrival. Gilman himself frequently stayed in the building until after 10 p.m. and then went home to work until after midnight reading or writing. During the whole of his career, except at the time of his eye operation, he was rarely, if ever, away from Ames for more than a week or so at a time, and when he did go away he told no one, not even his secretary, when he would be back, so that his students could not risk taking a day off.

As for the stipends he paid, he fixed the amounts by working out carefully exactly the minimum sum on which each student could manage. This had the secondary advantage of leaving them little to spend on leisure activities which might have kept them away from the laboratory. Someone who worked for him as postdoctoral fellow in the 1940s has told of how, in fixing his salary, Gilman asked him whether soap was provided free in his lodgings, and made an appropriate allowance when told that it was not.

The majority of the graduate students took 5-7 years to get their degrees, at a time when in other universities three years was still usual. Some were required to stay even longer; at least one is known to have taken ten years (and he, tragically, was killed in a fall from a ladder before leaving Ames) and some abandoned the attempt after a lengthy stay. One factor was that Gilman did not in most cases assign a graduate research project but instead directed the student to carry out a series of preparations, often unrelated, which could be expected to give rise to a number of short publications, and it required considerable initiative and ingenuity on the part of the student to devise and carry out additional experiments that would enable him or her to draw the material together into a coherent whole for a thesis. The preliminary examinations that had to be passed before the Ph.D. thesis could be submitted also presented a major hurdle, because they could be taken only with the supervisor's permission. This was normally not readily forthcoming from Gilman, and many students were too afraid of him even to seek it until he offered it. There is a story from those times, which, while undoubtedly apocryphal, and seemingly current outside rather than within Ames, reflects, with exaggeration, not only the distress caused by the length of time Gilman required students to stay with him but also his imperturbable and formal manner. In this story, it is said that a student who had been working for Gilman for 12 years, producing results for 15 publications, without having been allowed to submit for his Ph.D., burst into Gilman's office, placed a shotgun at his head and asked 'When am I going to graduate, Dr. Gilman?' To which Gilman is alleged to have replied 'Why Mr.—, I have been thinking that it is about time for you to get your Ph.D., and I appreciate your bringing the matter to my attention'.

It must be emphasized that not all of Gilman's students felt ill-treated, and it seems that towards a few, probably the hardest and most effective workers, he behaved rather differently, and they were able to regard him with awe rather than fear, and could question him and debate with him. A very select few, usually working in small laboratories, were rarely visited, and having been given on arrival a general indication of the problem they were to tackle were allowed to exercise their own initiative freely, and to work without interruption, subject only to the regular submission of satisfactory written reports. Dr. J.M. Straley, who took only the three years of 1933-1936 over his Ph.D., is one of those who had no complaints about the way he was treated by Gilman, partly because, he says, he had studied for a time under one of Gilman's former research students and so knew what to expect, and furthermore he had established a special relationship with Gilman shortly after arriving. He writes: 'I had been at Ames only for a week, working on a list of ten compounds HG had given me to prepare. I had four of them prepared when he demanded 10 g of one on which I had not even begun. I calmly told him that if he would inform me of his priorities I would arrange my activities accordingly. We understood each other completely thereafter.' Dr. Straley writes of his three years with Gilman as the most fruitful of his life; he developed a lifelong admiration for Gilman and remembers, as do many others, Gilman's kindness towards any of his students with personal, including financial, problems. (He recalls that one student, who had been badly injured, physically and psychologically, in World War I, and was unemployable and virtually incapable of research, continued to receive a stipend from Gilman until his death in the 1940s.)

But even those who regarded themselves as relatively well treated were aware of the unhappiness among other members of the group, and all of the following comments (some referring to the early 1940s rather than the 1930s) came from five such people:

> With most of his graduate students there was a strong undercurrent of fear: Gilman was capable of overwhelming criticism with which the students could not cope . . . A few of us who were treated differently were fully aware of the problems of the other students, but there was nothing we could do, since Gilman represented absolute authority in the Organic Chemistry Department.
>
> The attitudes of the graduate students under Gilman were based largely on overwork, underpay, uncertainty as to the future, and a constant prevailing fear of HG.
>
> Sad to say, we all claimed we hated him, but all of us mellowed in later years as realization grew of what a tremendous man and teacher he was.
>
> Most of us were united in our detestation of Gilman. Most of us later decided that he was not so bad after all.
>
> Everyone regarded his period at Iowa State College as a painful period necessary if one wanted a good job afterwards. Most of us, drinking laboratory alcohol and eating popcorn or potato chips, were united in our detestation of Henry, and wondered how people a few years after getting away could revise their memories and say: 'Well, he wasn't such a bad guy after all.' But most of us became revisionists in turn some years after leaving. Certainly Henry became very charming once we had left.

Some of the quotations above introduce an important aspect of Gilman's treatment of his students. However se verely he had dealt with them previously, once they had graduated his attitude changed completely, and all write of his unfailing courtesy, friendliness, and helpfulness once they had qualified. Moreover, because he insisted on complete commitment, and expected self-reliance, close familiarity with the relevant literature, the greatest care in experimentation, and absolute accuracy in observation and reporting, and would not allow them to submit for a Ph.D. until he regarded them as fully ready to work independently, those who did receive the degree under him were in great demand in universities and industry. Because of his reputation and that of his graduates, he could place them in the best companies in posts he judged most suited to their abilities. (It seems, however, that like many leading organic chemists of that time in Britain, he had a low opinion of the inorganic chemistry of the period, and one occasion, when one of his students decided to accept a post in a metallurgical company rather than the oil company Gilman favoured, he exploded 'But man, there's no *carbon* in it.' The student concerned later rose to be head of the company.) The discipline he had drilled into his assistants stood them in good stead, and a count in 1976 [1] revealed that his former students included more than 50 corporate research directors, nine corporate vice-presidents, one corporate president, and three university presidents, and more than 50 became professors of chemistry; these numbers would have been even larger later.

It was assumed by most of those aware of it that in his treatment of his graduate students in the 1930s and into the 1940s Gilman was driven solely by personal ambition. It can, however, be seen in a more favourable light. Thus his seeming parsimony with his research funds could have stemmed in part from his wish to give some useful employment and training to as large a number as possible at a time when many of them would otherwise have been unemployed or in menial posts. Moreover, it has been said that he kept them only until a vacancy became available for which he felt he could recommend them instead of releasing them into unemployment. There is also general agreement that although the stipends he provided were meagre even for those hard times, he never failed to come to the assistance of students who were in financial trouble, and in emergencies would commonly provide the necessary additional sums from his own pocket.

Through the efforts of his large and hardworking group, in the 1930s Gilman became one of the world's best known and highly regarded chemists. In the decade 1930-1939 he published 183 papers, probably more than any other chemists (though it has to be noted that many were very brief even by the standards of that decade, and by some authors would have been combined into a markedly smaller number of longer papers).* Through his work, especially that on uses of organometallic reagents, he had an influence on almost all organic chemists. He was offered numerous very highly-paid posts in universities and industry, and was a consultant to many leading chemical companies, from whom came a large part of the funds he used to support graduate students. His consulting activities were especially important to the meat-packing organization Armour and Company, and many of his Ph.D. graduates were employed there, largely on methods of utilization of waste fats. His advice and their efforts are said to have led to the establishment of a considerable industry based on those fats. The great admiration

* For a few years in this period he published a substantial proportion of papers in overseas journals, mainly *Recueil de Travaux Chimiques des Pays-Bas et de la Belgique,* following what he considered an unjustified rejection of one of them by the *Journal of the American Chemical Society.]*

and awe in which he was held in that company (and, indeed, by then throughout the U.S.A.) is illustrated by a communication from a chemist, Dr. S.H. Shapiro, who had a serious accident there in 1954 which resulted in his suffering third degree burns over some 70% of his body. There was doubt that he would live, but a few weeks after the accident Gilman, whom he had never met, appeared at his bedside to offer words of encouragement, and such was the effect of this action by the great man that the patient began from then on to recover, and was still with the organization (as part of the Akzo company) as a retired consultant in 1988.

Gilman also consulted for the Quaker Oats Company, and it was their interest in furfural that stimulated and financed his work on furan derivatives, and hence on other heterocyclic aromatic compounds, on which a good part of his research in the first half of the century was focused. He is known to have consulted also for the du Pont Company (and there is a report that very early in his career, in 1925, he and Roger Adams were invited, to become joint directors of the corporate research laboratory of that company) and for the Ethyl Corporation, Shamrock Oil, Metal and Thermit, and Parke-Davis.

World War II inevitably brought some changes in Gilman's research activities and in particular he was engaged on aspects of the Manhattan Project (the code name for the programme of work on the atom bomb), specifically the preparation of volatile uranium derivatives, mainly alkoxides. (Results of that work were described in papers that appeared in the 1950s) He was also commissioned to work on the synthesis of species with potential antimalarial activity and on other species of possible pharmaceutical interest, including organobismuth compounds. His status and his participation in the war-effort brought him the power to have experienced former students seconded to him from the posts to which they had gone, and one correspondent has written of his dismay at finding himself back at Ames after he thought he had at last escaped forever! Gilman still had a large research group of some 30 or more students and postdoctoral fellows, and still made the same demands on them as he had in prewar days, so that even though some of his work was necessarily secret he continued to publish extensively, and 156 of his papers appeared in the years 1940-1949.

That the students of the 1940s were driven as hard by Gilman as those in the 1930s, and required to take similarly long periods over their Ph.D.s, is confirmed by the appearance in 1947 of a satirical poem composed by one of his students J.W. Morton, Jr., and entitled Gilmania, with the subtitle 'Being a thesaurus of pictorial efforts on the part of divers organic-chymists as a brief commentary upon their experience in the laboratories of Iowa State College of Agriculture and Mechanic Arts'. Published anonymously, but purporting to be a hitherto unpublished section of the Prologue to Chaucer's Canterbury Tales, it was printed on high-quality paper, complete with illustrations in the form of pseudo-medieval drawings, and was in two parts. The first, 'The man of chemistry', read as follows:

Ther was also a man of chymistre,
I wot that HENRY GILMAN highte he;
He had grete lore of bodyes organeke,
And of the same ful longe colde he speke.
Of metall-carbon bondes and their waves
He mighte discours for fourty nightes and dayes,
Ere of his lerneynge cam he to the ende;
And noon his sayinges rightly colde amende.
To him ech yeere a dele of clerkes ther came,

To lere of him the carbon-chymists game;
Ech clerk abood with him ten yeers or so,
Then went his way with-outen wordes mo.
I mene, that this was trewe, but for the faster;
For some took fiftene yeers to get a Master.
Auctor he was, and Editor as wel;
His *Treatis* did lyke very hoot-cakes sel,
And specialy at *Ames,* in *Iouay,*
This boke founde market large, it is no nay.
A mery lyf this HENRY GILMAN lad;
He alweys was in finest clooth y-clad;
His coot was butoned with butones thre,
Nat even butones he lat idle be.
Advances grete in chymistrye he maked.
Bifor his anger al his clerkes quaked.

The second part, 'The carbon chemist', meant to be 'sung to the vulgar air, Strip Polka', relates the sad tale of an organic chemist who went to Ames to work with Gilman expecting to get his Ph.D. in nine quarters, was made to work for seven quarters on ethyllead, and eight quarters on phenyltin, then was assigned to butylzinc, tolylsilver, and xylylyttrium, but gave up, and became a garbage man, 'when Uncle Henry said to me one day, "You can start on bornylneon right away".'

The original edition of 50 copies, was printed at a press owned by the father of one of Dr. Morton's fellow graduate students, and sold within Ames at a price that just covered the costs of production. It was reprinted in 1954 and 1957. Its existence and general nature became very widely known in the U.S.A., though relatively few people seem actually to have seen it. It is often assumed that the publication was produced in anger by a seriously disaffected student, but Dr. Morton has indicated that it was meant as a good humoured, though pointed, satire of the type students have long been given to.

Although many of his students from the period before about 1950 do not have happy recollections of their time with Gilman, those in Ames who knew him other than as his students praise him without qualification as a gentleman of the highest quality. He had a friendly if very formal manner and treated those around him, including janitors and storekeepers, with courtesy and consideration, and always warmly welcomed visiting relatives of his students. Two secretaries who were with him in 1930s have written of him with great respect and affection. One, Mrs. A.S. Hull, writes:

> He was a fine gentleman, and I liked working for him very much. Even then he wore heavy lenses, and the long hours he put in must have put a great deal of stress on his vision. He was never impatient or temperamental in spite of his great intellect. He was most devoted to his lovely wife and daughter, who at that time was a toddler. When his daughter came on the telephone his face would light up, and I thought it very touching that this enormously dignified and prominent man became so like a delighted child himself when speaking to her. It was a great privilege to be associated with Dr. Gilman.

The year 1947 dealt Gilman a severe blow. He had always worn spectacles with thick lenses and even in the 1930s had obvious problems with his eyes, which were often inflamed**,** though the difficulties were evidently not such as to prevent him from playing tennis and handball. By the late 1930s, however, the difficulties had become so severe that much of the checking of the proofs of the major treatise on organic chemistry that he edited (see below) had to be carried out by his graduate students, who also took turns to read to him from current journals. In 1947 a combination of glaucoma and a detachment of a retina, which could not be remedied surgically, left him blind in one eye and with only about 10% vision in the other, and even that small residual vision deteriorated progressively, though with some periods of slight remission, during the remainder of his life. From 1947 on, although he could read to a limited extent by holding the page close to his eye, preferably with the aid of a large magnifying glass, he had to rely completely on students and his wife to read the literature to him. Selected students and postdoctoral fellows were called upon to write up their own work and that of others, though all their efforts had to be read to him, and he made many corrections and changes, so that his mark was placed firmly on the published versions even though the style varied somewhat from paper to paper. Although he was in great difficulty in unfamiliar surroundings without someone to guide him (which was rare because Ruth was almost always at his side when she was needed) he still behaved in exactly the same way in the chemistry building, making his rounds several times each day and rebuking severely anyone not at his bench when he should have been. It is said, too, that the students could never rely on the poorness of his sight, usually being detected if they tried to take advantage of it and frequently seeing him pick up a report and spot errors of chemistry, grammar, or spelling.

He still attended scientific meetings even into his late 80s, and such was Ruth's skill at acting as his eyes (for example, with a remarkable memory for faces, alerting him to the identity of persons approaching him) that many encountering him did not realize that he was almost completely blind. He travelled from time to time to conferences or on lecture tours abroad, and in visits in 1963 and 1971 was treated with great deference, and very warmly received, in the U.S.S.R., where organometallic chemistry was held in the highest esteem. It was a feature of his conference presentations, as of his publications, that he always gave generous credit to other workers in the field, and if he had to disagree with the conclusions of others he always did so in a gracious manner which made his comments seem compliments rather than criticisms.

The years following his loss of sight were, in fact, his most prolific in terms of numbers of publications, 312 appearing in the decade 1950-1959, and 198 in 1960-1969. As is evident from this rate of publication he continued to supervise the work of a fairly large, though steadily decreasing, number of research workers in that period; in the period 1960-1964, for example, he had at any one time about 8-10 graduate students and two postdoctoral fellows. In the later 1960s the proportion of postdoctoral fellows increased. Students with him in the 1950s and 1960s were under the same unrelenting pressure to work hard as those of earlier decades, but it is clear that they looked upon him then, as they remember him now, with real affection as well as respect. One change was that they now usually took only four years to get the Ph.D., the normal period in the U.S.A. by that time. (Gilman is on record as complaining in 1976 of the Ph.D.s of that date [1]:'Just when you're ready to get the maximum from them they stop', a not uncommon view among research supervisors in many countries!) A factor of some importance is that the Department of Chemistry at ISC underwent a rapid development after World War II and several very able organic chemists joined the faculty, so that Gilman's students no longer worked in isolation and he no longer had the absolute power that he had previously exercised.

Remarkably, he never retired from the Distinguished Professorship at ISU, but after reaching the usual retirement age of 70 in 1963 he received only a small salary to supplement his pension. He remained active in research until 1975, when he was 82 years old. Much of his support in the 1960s and 1970s came from the U.S. Air Force, but he received a substantial new grant from the National Science Foundation in 1972, at the age of 79. Even in the 1970s his research workers found him a hard task-master. Dr. M.T. Rahman, from Bangladesh, who was a postdoctoral fellow with him, relates that in 1974, when Gilman was 81 years old, he visited each of his four research assistants, all postdoctoral fellows, three times a day, questioning each of them in turn in the form 'What have you done since I was last with you?', 'What are you doing now?', and 'What do you intend to do?'; just as he had in the 1920s. Dr. Rahman writes:

> He expected us to have four reactions on the go, and to meet such an impossible expectation it had been known for a postdoc to put some liquid in a flask and have the stirrer going to make up the fourth, because although Henry could not see well his hearing was acute. The little noise would be heard, and had to be accounted for, and sooner rather than later the experiment would be asked after, and would have to be completed, despite another grace period when the postdoc claimed the compound was on the column for separation.
>
> He had a store of all the compounds ever made by his research associates, neatly arranged, and cross-referenced on index cards.* I once asked him the secret of his success, thinking that he might refer me to this systematic organization and hard work, but he replied 'The intelligent use of the chemical literature'.

Dr. Rahman goes on to say that Gilman was very annoyed with an American postdoctoral fellow who helped him to buy a car, since possession of this would encourage him to be away from the laboratory at weekends.

* The collection of chemicals was acquired by Dr. Alfred Bader of the Aldrich Chemical Company. There were about 20 000 of them, and after rejection of a large number for various reasons (many were by now common chemicals, some had decomposed, and others were available in only very small quantities) just under 3000 are available for purchase, as the Henry Gilman Collection, at prices intended to cover the costs of distribution.]

An important event in Gilman's career was the publication in 1938 of a two-volume treatise entitled *Organic chemistry, an advanced treatise,* which he had conceived and edited. This work consisted of chapters covering a wide range of topics in organic chemistry written by eminent authorities on them. It was the first publication of its kind, and for advanced students represented a considerable improvement on the general organic textbooks available to them at the time. It served as a model for many later multi-author publications, but judged in the context of the time in which each appeared was probably the best and most influential of them, since for every chapter Gilman had been able to get the leading expert in the U.S.A. Moreover, the topics were selected and the contributions carefully coordinated and edited by Gilman himself, who had an encyclopaedic knowledge of organic chemistry and a seldom-matched enthusiasm for it. Gilman's own chapter on organometallic compounds was a masterly summary of the state of the subject at the time, and played a major part in its development; it can still be read with profit. The book served as a standard text for several generations of graduate students in the U.S.A., and was much used also in other countries. A second, updated and expanded, edition of the two-volumes appeared in 1943, and two additional volumes in 1953. If there were any chemical laboratories in which Gilman's name was not already one of the best known in organic chemistry, there could have been few unaware of it after the appearance of this very influential treatise.

With M.S. Kharasch he was instrumental in creating the *Journal of Organic Chemistry,* which made its appearance in 1936, and for many years he served on its editorial board. At various times he served on the editorial boards of several other important journals.

Gilman had a major influence on many hundreds of chem ists through his undergraduate and postgraduate lectures. At some time during his first 25 years at Ames he gave all the organic chemistry courses at all undergraduate and postgraduate levels, and as the only senior organic chemist was also in overall charge of all the lecture courses and laboratory classes. A measure of his success is that a ranking of 14 Midwest universities on the basis of the quality of instruction in organic chemistry, conducted in about 1930, placed ISC first [4]. In the later part of that period, however, he concentrated mainly on the third and fourth year courses in organic chemistry for chemistry and chemical engineering majors. From all available accounts his lectures for most of that period were outstanding, and correspondents have written of how he came to classes admirably prepared, lectured without notes with great clarity, writing up all the essential material on the blackboard, and speaking firmly and slowly, with appropriate repetition, so that full notes could be taken; he insisted that these notes be taken in bound notebooks. The lectures were enlivened by numerous anecdotes. His air of authority and his enthusiasm made a deep impression on many of his audience, but his classes were by no means unalloyed pleasure for all of them. He began most lectures with at least a ten minute session in which he questioned members of the class, calling on them by name, in some years with the help of a plan showing the seats to which they had been assigned. He was severe on those who failed to answer satisfactorily and would persist in his questioning of them, so that they became more and more confused; it is said that female members of the class were frequently reduced to tears, and greatly feared his lectures. Sometimes the questioning would go on for the whole of the 50 minute period. In winter, when temperatures were often below zero on the Fahrenheit scale, if answers were unsatisfactory he would call for the windows to be thrown open in order to keep the class more alert and at that stage an assistant would bring in a fur-lined overcoat for him to put on. He was especially severe on chemical engineering students, maintaining a constant confrontation with them, in a tactic designed, as one who attended such classes has pointed out, to keep the attention of students for whom organic chemistry was not a topic to which they were naturally inclined. It is on record [4] that he once left two assistants in charge of his lectures to chemical engineering students when he went away to a conference. During his absence the engineers placed a notice in the student newspaper reading 'All is forgiven Henry, please return'.

There are indications that in the later 1930s and the 1940s Gilman's lectures were not always successful and one correspondent says that in the third year course in organic chemistry he attended in 1943, Gilman 'tried for six weeks in vain to impart information to the class. We could not establish any level of communication. The solution proved to be to assign a graduate student as the instructor!' (The same correspondent added that there was a belief at the time that Gilman made graduate students stay on without their Ph.D.s in order to maintain a cadre of experienced instructors!) It is possible that he became less effective as a class teacher in the 1940s as he became even more immersed in research and as his eyesight deteriorated, but some who attended classes at that time have written with much enthusiasm about them and it is evident that they appealed to some, probably the most dedicated, more than others. Several correspondents, probably representative of hundreds of their contemporaries, have stated that Gilman's undergraduate classes gave them an enthusiasm for organic chemistry that stayed with them throughout their careers.

Gilman was a determined opponent of discrimination against black Americans long before it was fashionable to be so, and his attitude was especially unusual in the Midwest. Even in the early 1930s he usually had at least one black graduate student in his group and one of them, N. Calloway, was the first black person to receive a Ph.D. in chemistry from a university west of the Mississippi: he went on to a distinguished career in medicine and became president of a company. A black student with him some years later, Dr. S.P. Massie, went on to become a professor, and Chairman of the Department of Chemistry, at the U.S. Naval Academy. Another black student, F.D. Patterson, became President of the Tuskegee Institute, for which Gilman served as an adviser for some years. Gilman also served on the Board of Trustees of the Carver Research Foundation. Mr. H. Oatfield, who joined Gilman's group as a Master's degree student in 1931 from the William Marsh Rice University in Houston, Texas, and went on to a distinguished career in industry, relates that on his arrival Gilman sent him to Calloway for instruction on how to go about tackling his first research assignment, and believes that this was done to test the attitude of someone who had just come from Houston, where colour prejudice was extreme. (As it happens Oatfield had himself been appalled by the treatment of black people in Texas.) There is much evidence that Gilman, whom some believe to have been the subject of prejudice himself, never allowed race, nationality, creed or political beliefs to influence his treatment of individuals. He certainly did distinguish between people, especially students, in terms of their personal qualities, and in particular their integrity and dedication to hard work.

Gilman's good manners included genuine consideration for others, not just formal courtesies, though the latter were also much appreciated by recipients of them. He always spoke firmly and softly, and was never known to show anger. Although he could on occasions, when especially dis satisfied with a student's attention to chemistry, deliver a cutting rebuke, his displeasure was normally indicated by no more than a long drawn out 'Oh', which left no one in any doubt about his feelings, or by striking his forehead with his fist and exclaiming 'What a blow!' In one of his more severe admonishments, to a student who had allowed lunch to interrupt his recrystallization of a product, he said 'What man, have you ice-water in your veins?' On more than one occasion, it seems, a student who (in common American style) reported that a reaction 'was allowed to sit overnight' was rebuked with the response 'No, it was allowed to *stand* overnight; nothing sits in this laboratory'.

An aspect of the good manners he acquired in Boston and which stayed with him all his life was that he was always neatly and appropriately dressed. Students with him in the 1920s and 1930s recall that they almost invariably saw him in a tweed suit, tailored for him in Boston, with three buttons (all fastened, as noted in *Gilmania!),* and with lapels, pockets, and cuffs sewn within about a quarter to half an inch from the edge; for a long time they thought he had only the one such suit, which he wore every day to the laboratory, but later realized that he had at least two identical ones that he wore in turn. That he was not prone to react rapidly to changes in fashion is revealed by the fact that a photograph taken in about 1968 shows him wearing just such a suit, similarly buttoned [5]. He greatly enjoyed conversation with friends and was always prepared to listen and learn; he gave his complete attention to anyone, of whatever status, speaking to him, often with his prominent chin cupped in his hand. He had a large fund of recollections and anecdotes, and a phenomenal memory for events in the lives of former students and colleagues whom he had not seen for many years. He had a warm, gentle sense of humour. An example is the following passage from a letter he wrote in 1962 [6] concerning one of the most hardworking students he ever had, a George F Wright, who was with him in the 1930s and later achieved considerable distinction as a professor at the University of Toronto: 'It was prohibition time. One weekend he needed some very good alcohol to purify a sensitive furan compound; the storeroom was closed, so he drove in his old car to one of Al Capone's caves in Boone and bought the alcohol. This is the only time I ever heard of a student bringing alcohol *into* the chemistry building.'

Throughout his life, up to the time of his final illness, Gilman maintained an extensive correspondence with surviving old classmates at Harvard (including J.B. Conant, who became a famous president of that University), former students and postdoctoral fellows, former colleagues, and numerous friends around the world. From time to time, when a certain issue, chemical or general, was on his mind, he would seek the view of a wide range of his correspondents; in his eighties, for instance, he became interested in cases in which the advance of chemistry had been delayed by the failure of a research supervisor to entertain good ideas for future work suggested by a student and sent letters to correspondents in several countries seeking accounts of examples of this. (His interest in the matter had been originally aroused during his visit to Europe as a postgraduate, where Grignard told him how Barbier had for some years prevented him from trying out the method which later became standard for the preparation of Grignard reagents.) An outstanding feature of his correspondence was his generosity towards other chemists whose work he admired. Young people, inside and outside the U.S.A., who had never met him were greatly encouraged by his letters expressing regard for their recent papers, and many an established chemist derived much pleasure from a short note from him saying how much he admired their overall contributions to chemistry.

A notable aspect of Gilman's character was that, even though he had for many years heard himself referred to as, for example, 'The American Liebig,' 'The Master Chemist', and 'The Father of Organometallic Chemistry', and in 1968 'one of the most inspirational and legendary figures to grace the chemical scene this century' [5], he never developed any trace of the pomposity and arrogance that is sometimes evident in those who have achieved such worldwide eminence. Although he was, for a professor, highly-paid, and received substantial additional sums for consulting, he and his family maintained an unostentatious, though comfortable, lifestyle. One indulgence he and his wife permitted themselves was a new home, outstandingly luxurious at the time for Ames, which they had built in 1936. One who was a student with him then, and was invited with his colleagues to a party to view the house, has told of the awe in which they looked around it, admiring especially the fully-fitted basement and attic, a feature he had never before encountered, and finding that the whole house had air-conditioning, something virtually unknown in Ames in those days. Several generations of students enjoyed parties there in later years, often given so that they could meet eminent visitors.

Gilman was rightly the recipient of much admiration for the way in which he responded to severe loss of sight which developed into almost total blindness. He allowed the handicap, which would have caused most people to give up, to make the minimum possible change in his behaviour, and this was all the more remarkable because if he had abandoned research in 1947 he would still have been one of the outstanding organic and organometallic chemists of the century. In the event, of his 1020 research publications, 584 appeared after 1947 compared with 436 up to then, and moreover these later papers were generally more substantial than those in the earlier years. That chemistry still held the same excitement for him in the years following the loss of most of his sight is illustrated by a story told by Professor R.A. Benkeser, who after taking his Ph.D. with Gilman joined the chemistry faculty at Purdue University. Gilman went there by train in the late 1940s or early 1950s to give a lecture and on arrival was taken by Benkeser to the Union Building where he was to stay overnight. At the reception desk the clerk handed him a telegram that had just arrived and this read simply 'The mixed melting point was undepressed'. It turned out that, although it was irrelevant to his lecture, Gilman had arranged for the student to wire him because he could not wait until the next day, when he would be back at Ames, to find out whether a synthesis had been successful.

One aspect of his life that was necessarily changed by the deterioration of his sight involved his previous commitment to keeping fit through regular exercise. He had always played vigorous tennis in the summer and four-wall handball in the winter, usually with graduate students; because of the time of day they took place the games were referred to for many years as 'five o'clocks'. He lived about a mile away from the campus and usually walked the distance briskly six times a day, often inviting a student to walk with him on the way home, and discuss the student's progress on the way so that, it is said, the walk commonly resembled an oral examination.* His son relates that each evening Henry and

* Most unusually for someone in his position in the U.S.A., he never learnt to drive a car, possibly because he did not trust his eyesight even before the major deterioration, and students with him in the 1930s relate that when driven by others he would keep the door slightly open with his foot, so that he could escape rapidly in an emergency!]

his wife would go for a final brisk walk before he continued working in his study well past midnight. It was one of the hardships he had to accept that, after first giving up his tennis and handball, as his sight became even worse he had finally to abandon even his vigorous walks.

He seemed hardly to change in appearance or attitude in the last 30 years or so of his life, and at a gathering in Midland, Michigan, in 1982 to mark the 21st anniversary of the creation of the Frederic Stanly Kipping Award of the American Chemical Society, of which he had been the first recipient in 1962, he was upright, gracious, alert and interested in the reports of others, still showing the same happy sense of curiosity and wonderment at an interesting new observation which had been a life-long characteristic. This was in spite of the fact that in the previous year, after some years of serious heart trouble, he had, at the age of about 88, been fitted with a pace-maker; of the event he recounted with delight that the surgeon had instructed him to return ten years later for a replacement battery. His composure in face of his loss of sight had been an example and inspiration to all who knew him. It was noted of him in 1977 that 'he speaks easily, casually of his "impaired vision", but he is wrong. His eyesight may have been dimmed, but his vision has not' [1].

Gilman was a well-informed, highly literate, and articulate person. In his later years became concerned about the lack of culture among science students (a matter he referred to in his interesting Priestley Award Address in 1977) and began to explore the possibility of making formal instruction in the humanities a required element of a postgraduate degree course in chemistry. He had a strong sense of history and it gave him very special pleasure that he was made an Honorary Fellow of the (British) Chemical Society, the oldest chemical society in the world, and took great delight in joining the Royal Society, the oldest scientific society, and in signing the Charter Book containing the signatures of those such as King Charles II, Newton, Priestley, Dalton, Faraday, Darwin and Ramsay. During his visit to Britain to be formally admitted to the Society he revealed a strong wish to visit Nelson's flagship H.M.S. Victory, and was taken there by Dr. D.R.M. Walton. Although he could see little of it, he derived great pleasure just from standing on its deck.

Gilman enjoyed a very happy family life. He had been brought up in the Jewish faith but upon marriage he joined the Episcopal Church, to which his wife belonged, and they remained active members of it throughout their married life. As has been seen, for the last 40 years of his life he was able to sustain his remarkable level of activity only with the help of his devoted and able wife. Sadly she died a little over two months after Henry, and she was followed almost six months later by their daughter Jane, who had returned to Ames in the latter part of their life and given them increasing support. A son survives them, as do four grandchildren, who brought them great pleasure.

The final word in this section can appropriately come from Professor W.B. King who, as mentioned earlier, was a research student with Gilman in 1923-27 and subsequently his colleague at ISC and ISU throughout their working lives. He writes: 'When he arrived at Heaven's gate, I can imagine St. Peter saying "Let this gentleman in; few appear with such an enviable record of dedication and discovery, honesty and courage!"'

HONOURS, DISTINCTIONS AND TRIBUTES

The main formal honours and distinctions bestowed on Gilman are listed below.

1945	Elected to National Academy of Sciences
1951	Iowa Award and Midwest Award of American Chemical Society
1961	Honorary Fellow of the (British) Chemical Society
1962	First American Chemical Society Frederic Stanley Kipping Award in Organosilicon Chemistry Distinguished Professor, Iowa State University
1974	Chemistry building, Iowa State University named 'Gilman Hall' Annual series of Gilman Lectures established at Iowa State University, endowed by gifts from friends and former students
1975	Elected Foreign Member of the Royal Society Distinguished Fellowship Awards, Iowa Academy of Sciences.
1977	Priestley Medal of the American Chemical Society
1982	Iowa Governor's Science Medals
1987	Gilman Graduate Fellowship Fund established in his memory at Iowa State University

Among published tributes are the following:

(a) article (with photographs) by R.A. Benkeser in the *Journal of Organic Chemistry* in 1968 on the occasion of Gilman's 75th birthday [5];
(b) tribute in *Chemical and Engineering News* in 1977 marking the award of the Priestley Medal [1];
(c) article by R.K. Ingham as the introduction to a special issue of the *Journal of Organometallic Chemistry* on the occasion of Gilman's 90th birthday [8];
(d) appreciation (with photograph) by J.D. Roberts in *Organic Syntheses* in 1987 [9];
(e) Appreciation (with photograph) by J.J. Eisch in the *Journal of Organometallic Chemistry* in 1988 [10].

On the occasion of Gilman's 90th birthday a celebratory dinner was held in his honour on the ISU campus and was attended by 278 members of his family, friends, and former students, a good number from abroad. He was presented with several volumes of congratulatory letters sent by 500 of his friends and colleagues. An account of the event mentions that he had supervised 242 graduate students [7].

Although Gilman was a dominant figure in chemistry in America in the 1930s, and was made a member of the National Academy of Sciences in 1945 (he was the first chemist at a Land Grant Institution to achieve this distinction), it can be argued that the importance of his overall contribution to chemistry was formally recognized sooner in Britain than in the U.S.A., in that in 1961 he was made an Honorary Fellow of the Chemical Society, the highest distinction the British chemical community can bestow, and in 1975 was elected to Foreign Membership of the Royal Society, the highest distinction the British scientific community can confer on someone from outside the British Commonwealth, and yet only in 1977 received the Priestley Medal of the American Chemical Society, its highest award. There are those who argue with some force that so influential was his work that he could appropriately have received the highest formal international recognition open to a chemist.

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The portrait photograph reproduced was taken in 1969.

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(1) (With R. ADAMS) *J. Am. chem. Soc.* 1915 37, 2716.
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(3) (With L.C. HECKERT) *J. Am. them. Soc.* 1920 42, 1010.

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1938	*Organic chemistry. An advanced treatise* (ed. H. Gilman), vols. 1 and 2. New York: Wiley.
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Photo by John M. [illegible]

Morris H. Hansen

MORRIS HOWARD HANSEN

December 15, 1910-October 9, 1990

BY JOSEPH WAKSBERG AND EDWIN D. GOLDFIELD

MORRIS HANSEN WAS THE most influential statistician in the evolution of survey methodology in the twentieth century. Early in his career at the Census Bureau he put together and directed a staff of mathematical statisticians and other survey methodologists whose aim was to systematically define the principal problems in the conduct of surveys, carry out research on these problems, and develop the statistical methods necessary to overcome them. This work included the development of sampling theory necessary for the efficient conduct of large-scale national surveys, the establishment of formal quality control methods for surveys, and the derivation of theory and models for analyses of nonsampling errors.

Hansen prodded the Census Bureau into accepting such innovations as the purchase of the first computer for statistical purposes, the development of optical scanning equipment, the introduction of self-enumeration and mail in both demographic and economic censuses, and other survey techniques now commonly used by both government and private organizations. He anticipated the concern that would arise over the completeness of coverage in decennial population censuses and, long before the current interest in the subject, he persuaded the bureau to adopt procedures de signed to improve coverage; and he directed research on coverage problems. Both the statistical methods he and his staff developed and the form of research staff he organized had a profound effect on statistical agencies all over the world.

Morris H. Hansen was born on December 15, 1910, in Thermopolis, Wyoming. His parents, Hans C. and Maud E. Hansen, lived in the nearby town of Worland, Wyoming, where Morris was raised. Although Morris left Wyoming soon after graduating college, he retained fond memories of Worland, and would talk about his childhood and adolescent experiences to his friends. At the time of Morris's birth Worland had a population of 265 and Wyoming's population was 145,965. In the eight decades following, Worland's population grew twenty-fold and Wyoming tripled its size; Morris's stature increased much more. He became one of this country's, and the world's, most prominent statisticians.

He majored in accounting at the University of Wyoming, obtaining a B.S. degree in 1934. His stay at the university was interrupted in midstream while he took a year's absence in order to work to earn some money. The only statistics courses the university offered were a couple of courses in economic statistics. Morris took the courses, taught by a very good teacher, Forrest Hall, who stimulated Morris's interest in statistics. He decided that he would attempt to make a career in statistics, not a common profession in those days. Morris's subsequent formal training in statistics consisted of after-hours courses taken at the Graduate School of the U.S. Department of Agriculture and at American University, where he received a master's degree in statistics in 1940. (He was later granted an honorary doctorate by the University of Wyoming in recognition of his contribution to survey research.) By that time he had made himself into a highly skilled statistician, learning his trade both at school and at work. Throughout his career he continued to learn from those he worked with, from those he conversed with, and most of all, from himself. Much of what we now know about sample survey theory and methodology was developed by Morris Hansen, using his great ability to absorb and develop and apply.

When Morris graduated from college in 1934 there were few jobs available. He found some temporary employment with the Wyoming State Emergency Relief Administration, and took federal civil service examinations. His high ratings on the examinations and the fact that Wyoming was below its quota for federal jobs brought him an employment offer from the Bureau of the Census in Washington. He began work in the bureau's Personnel Division in 1935, and the following year was transferred to the Statistical Research Division, then headed by Calvert L. Dedrick. This marked the real beginning of what was to be a long and productive career in statistical surveys and censuses.

During the depression of the 1930s emergency relief agencies were handicapped by a lack of good information about the extent of unemployment and the characteristics of the unemployed. In August 1937 the U.S. Congress, with the strong support of President Roosevelt, authorized a national voluntary census of the unemployed and partially unemployed. The census was conducted in the fall of 1937. Postal workers delivered to every household on postal routes a questionnaire to be filled out and returned.

It soon became evident that there were problems in getting complete and accurate response in the census. Knowledgeable statisticians, including Dedrick and project consultants Samuel A. Stouffer and Frederick F. Stephan, recommended that a follow-on sample survey be conducted by direct person-to-person interview to check on the validity of the census. The administration endorsed the sugges tion. Dedrick, who was serving as chief of the technical staff of the unemployment census, directed the project, called officially the enumerative check census. He brought Morris, then not quite twenty-seven years old, into the project. Morris helped design the sample and the statistical procedures to generate estimates of unemployment and estimates of the standard errors of the unemployment estimates, and to project sample estimates to smaller areas through regression relationships. All of this, in fact the very idea of sample surveys for official governmental guidance, was quite new.

On a very fast track, the survey was conducted in mid-November 1937. The census itself produced three large volumes of detailed data, reporting for the nation as a whole 7.8 million totally unemployed and 3.2 million partially unemployed. The sample survey results were published as Volume IV, "The Enumerative Check Census," written by Dedrick and Hansen. It reported estimates of 11.0 million totally unemployed and 5.5 million partially unemployed. The volume IV statistics were generally accepted and used much more than the three volumes of the census itself, which were referred to mainly for the geographic detail they provided. This experience significantly contributed to the acceptance of sampling by the government. Ingram Olkin in an interview with Morris commented,[1] "This was really a major innovation in the philosophy of sampling and censuses." This experience encouraged the Works Progress Administration to sponsor the development of a monthly sample survey of households beginning in 1940 to provide estimates of employment and unemployment, which under the Census Bureau became known as the Current Population Survey.

In 1941 Morris became the assistant chief of the Statistical Research Division at the Census Bureau. William N. Hurwitz had joined the staff in 1940, beginning a close collaboration with Morris that lasted for almost thirty years and produced a steady stream of notable contributions to statistical science and practice. Hansen-Hurwitz put together and directed a staff of bright young statisticians that led the way in developing modern scientific techniques in sampling and other survey-related methodology.

When the Census Bureau became responsible for the government's monthly household sample survey that had become relied upon as the barometer of employment and unemployment, Hansen and his team set about to improve it and to put it on a sound basis, both in terms of mathematical theory and in terms of the efficiency of field and processing operations. In so doing, they made it into a model for other government agencies, the private sector, and many other countries whose statisticians came to the Bureau of the Census for training.

In examining the methodology for conducting sample surveys that was considered state of the art in the 1930s, Hansen and Hurwitz realized that the mathematical-statistical theory underlying sampling methods was inadequate. They started to work on the development of the necessary theory. The seminal paper that resulted from this work was one of the first articles on the general theory of finite sampling[2] As stated in the book *Revolution in United States Government Statistics (1926-1976),* [3] "[T]he importance of the article goes well beyond the one survey . . . [I]t represented a major breakthrough in the theory as well as the practice of finite sampling in the social and economic fields." This paper was followed by a number of others which developed sampling theory in more detail. The papers included the extension of theory to cluster sampling in which the number of elementary units is not the same in all clusters. The work pointed out the advantages of very large primary sampling units in a multistage sample when costs and adminis trative restrictions are taken into account. The statistical properties of sampling units at various stages of a multistage sample drawn with probability proportionate to some measure of size of the sampling unit received attention. Emphasis on the acceptability of an estimator was shifted from unbiasedness to consideration of consistency and mean square error. Hansen was an advocate of the principle that in most cases inference from sample surveys should be based on the design of the surveys rather than on assumed models of the population, and co-authored a number of papers on the subject; however, he had an open mind on this topic (as well as on most other statistical issues) and recognized conditions under which models were useful.

In 1953 there was issued what quickly became, and remained for a long time, the bible for sample survey practitioners: the two—volume work *Sample Survey Methods and Theory.*[4] It is still considered a standard reference on the theory and application of probability sampling. The publisher, Wiley, reissued the book in 1993 in the Wiley Classics Library. The distinguished British statistician T. M. F. Smith, in a lecture on sample surveys in 1991,[5] likened the contribution of Morris and his colleagues to sample surveys as equivalent to R. A. Fisher's contribution in other areas of statistical science, saying, "I now realize that the contributions of Morris and his colleagues to sample surveys represent an equivalent contribution from statistics to social science and more generally to all forms of observational study."

Even the relatively advanced western European countries were fallow ground for education in the new techniques, some developed by Hansen and Hurwitz, and others by the staff they had put together and who considered themselves disciples. One of the authors of this memoir recalls that in the early 1950s he (E.D.G.) traveled to the western European countries to spread the gospel and then organized and directed an extended training course for senior European government statistical officials at the Bureau of the Census in household sample survey techniques and the use of such surveys to produce current labor force statistics. A book published by the Bureau of the Census included the lectures given in the course by Morris and others and a subsequent book, *Labour Force Statistics: Sample Survey Methods,* was published by the Organization for European Economic Cooperation. The seeds thus planted yielded a crop of sample survey programs, modeled after the United States, in many European countries and later in other countries.

With the established success of sampling in population surveys Morris turned his attention to business surveys. He encountered the attitude that, while sampling would work with populations that were relatively homogeneous, it would not work with business surveys, where the units were very diverse in size and characteristics and the distributions were very skewed. Hansen and his associates were able to show how to take advantage of this skewness with differential sampling rates and approximately optimum allocation of the sample. Sampling was successfully extended, with new principles, methods, and theory developed as needed, to data gathering on manufacturing, retail trade, wholesale trade, agriculture, government units, and other subjects, making it possible to compile more and better information on the state of the economy.

A parallel development was the introduction of sampling into the major censuses. Until 1940, all items of information in the decennial censuses of population and the more frequent censuses of the various economic sectors were collected on a 100% basis. This was costly, burdensome to respondents, time-consuming, and imposed constraints on the number of questions that could be included in the censuses.

Morris's first contribution to census-taking was to work with W. Edwards Deming, Philip M. Hauser, and Frederick F. Stephan in developing the sample to be used within the 1940 decennial census of population. Some of the questions in that census were asked of only a sample of the population. Morris and his team later were major participants in the redesign of all the major censuses. Sample schemes were devised not only for collecting some of the information on a sample basis, but also for tabulation and for quality control of the census operations. Studies by Hansen and his staff led to the conversion of the decennial census from door-to-door canvassing by enumerators to a census now largely done by mail with self-enumeration by the household respondents. The change in procedure not only introduced operational gains, but also gains in the quality of the data. The Hansen team's experimental studies for the control of measurement errors in the decennial census had demonstrated that correlated errors within the work of enumerators constituted a serious problem (e.g., an enumerator who consistently misinterpreted a particular question could destroy the validity of that item for the entire area that he or she covered), and that self-enumeration substantially reduced this kind of bias and also improved accuracy of response on most items. The innovations under Morris's leadership radically changed census procedures that had been in effect for 150 years.

The revolutionary changes in census methodology came about as a result of two approaches that Morris and his colleagues pioneered. The first was the introduction of the concept of total survey design. This meant the incorporation of nonsampling error into the consideration of choices among alternative survey designs. In other words, this implied recognition that when the errors resulting from simple response and interviewer variance and the biases resulting from the use of relatively untrained interviewers were already large, sampling errors could be introduced, even in small areas, with very little additional impact on quality. On the basis of this research it became clear that better quality would result from resorting to sampling, and using the cost savings to improve interviewer training, supervision, and quality control.

The second approach was related to the importance of research in the work of statistical agencies. Morris emphasized the need to conduct both short-term research to solve immediate problems and long-term research required to identify issues and to look for solutions. An implicit assumption in total survey design is that data are available to analyze components of error and unit costs. Morris's intellect and drive led to a tacit acceptance of the fact that research and development are essential parts of a census program, and that a reasonable proportion of the total budget should be assigned to these functions. Hansen and his associates developed theory that reflects the contribution of data collectors and data editors to the total mean square error of an estimate. The theory is widely known as the Census Bureau model of survey error. On the basis of that model, the Bureau of the Census has implemented the estimation of the error contributions of enumerators in the population census and current population survey, as well as from coders of occupational categories, and in the economic censuses.

An illustration of how this approach was applied in practice occurs in the considerations that led to the decision to extend the use of sampling to most items collected in the 1960 U.S. census. (The wide use of sampling was continued in subsequent U.S. censuses and has also been adopted as an almost international standard.) The studies of sampling, response, and interviewer errors carried out on the 1950 census indicated that for most census items the total mean square error was only slightly affected by the introduction of a moderate amount of sampling error.[6]

Morris was a leader in bringing the electronic era into statistics. J. Presper Eckert and John W. Mauchly, who were involved in the building of ENIAC, an electronic computer built for specific military purposes in the early 1940s, saw the possibility of adapting the technology into the design and construction of a large-scale general-purpose electronic computer with the capacity to process large quantities of data such as those collected in a census. They recognized that the Census Bureau might be interested in having such a computer and initiated discussions with Morris, who quickly realized the potential importance to statistical applications. Together they formulated a plan for the design and building of a data processing computer with the sponsorship of the Bureau of the Census. With the assistance of the National Bureau of Standards (now called the National Institute of Standards and Technology) negotiations were conducted that led to the building by Eckert and Mauchly of Univac I, with input into the design decisions by the Hansen staff. Univac I was the first electronic statistical computer. The first of what became a series was received by the Bureau of the Census in early 1951 and was put to work twenty-four hours a day, seven days a week on parts of the data processing of the 1950 census.

The next major electronic advance that the Hansen team undertook was the development of mark-reading electronic equipment that could replace manual card punching and handle the massive data conversion job for the 1960 census. The result was FOSDIC—Film Optical Sensing Device for Input to Computers—invented and produced by the joint effort of the staff of the Bureau of the Census and the Bureau of Standards. The equipment was designed, tested, and constructed in time for its highly successful use in the 1960 census. With successive improvements it has been used in every decennial census since.

Hansen was the dominant force in the design of sample surveys in the Bureau of the Census. His first major contribution was the design of the Enumerative Check Census of unemployment in 1937 which involved a probability sample of postal delivery routes and ratio estimators. Sampling for some items was introduced in a decennial census in the 1940 census of population and housing and has been an integral part of decennial censuses ever since. The 1943 redesign of the labor force survey, now known as the Current Population Survey, is largely based on the theoretical developments mentioned above. This is also true for the bureau's retail trade survey and the other periodic sample surveys conducted by the bureau. Both the methods of integrating sample data with 100% data in censuses and the design of intercensal sample surveys have been adopted by national statistical agencies worldwide.

Upon his retirement from the Bureau of the Census in 1968 Hansen was invited by Edward C. Bryant, president and one of the founders of Westat, Inc., to join the company. Westat was at that time a fairly small statistical research company specializing in U.S. government contract work. It was established a few years earlier by Ed Bryant, at that time chairman of the department of statistics at the University of Wyoming, and several colleagues at the university. Ed knew Morris personally as well as professionally and, of course, by 1968 Morris's reputation was well-established. In addition, the two had University of Wyoming connections. (Another alumnus of the University of Wyoming was W. Edwards Deming. It is curious that the University of Wyoming, a relatively small institution that did not have a particularly strong department of statistics, produced three distinguished statisticians. The university was apparently a close bond among the three—Hansen, Bryant, and Deming—and they were good friends as well as professional colleagues.)

Morris accepted Ed Bryant's invitation and was appointed senior vice-president of Westat. Later, when Ed Bryant retired from active participation in the company management, Hansen was elected chairman of the board of directors. In the next few years a number of Morris's former associates from the Census Bureau joined Westat, primarily because of the intellectual stimulation of working with him. His insistence on exacting standards, his leadership in inspiring the professional staff, and his personal involvement in the design of some of Westat's major projects were important factors in the company's success. At Westat he had a lead role in the design of many important national surveys carried out by U.S. government agencies and by Westat, such as the consumer price index (CPI) and the national assessment of educational progress (NAEP).

He was particularly proud of his contribution to the CPI. The prevailing philosophy regarding the CPI was similar to earlier beliefs with respect to sampling from highly skewed distributions for business establishments, that is, "you can't do probability sampling; it applies to other areas but it doesn't apply here." With Benjamin J. Tepping, a frequent collaborator, Morris proposed procedures for selecting samples of establishments and of items to price within the establishment that followed principles of probability sampling. Because of changes that constantly take place and the need to keep the same sample in operation constantly, the sample could not be kept up to date in a probability sample sense. Nevertheless, it was a substantial advance over procedures used previously. The Bureau of Labor Statistics adopted the new methods and later extended them to other areas, the producers price index and the international trade price index.

While at Westat, Morris made important advances in methods used for quality control on welfare programs run by the states including aid to families with dependent children (AFDC) and food stamps. Hansen, working with Ben Tepping, proposed federal monitoring programs using subsamples of cases that had been examined by state quality control reviewers. Estimates of errors were then prepared from both the statedata and those reported by the subsequent federal monitors using a double sample regression estimator. The procedure got the maximum information possible from the data available in the estimation of overpayments to recipients. It also made the sanctions on the states depend on the federal investigations, not the states. These procedures were soon adopted by the agencies responsible for the programs.

He continued his interest in sampling theory as well as in practical application of accepted methods. A paper he prepared with Ben Tepping and William Madow was an important contribution to a controversy among researchers in sampling theory and estimation on the role of models in making inferences from survey data.[7] He wrote a number of papers relating to historical developments in sampling theory and, more generally, to survey methods. He also continued his role as advisor to a number of government statistical agencies (both in the United States and Canada) and activities in statistical societies.

A special feature of Morris's approach to his work, which helped make him not only respected but beloved by those who worked with him, was his strong belief in teamwork. He did not merely direct; he collaborated. Ideas were crystallized in one-on-one encounters and in group discussions. Morris was exceedingly generous in sharing credit and in acknowledging contributions. He was modest and a good listener. He earned, and received, the steadfast loyalty of all who worked with him. He was a major influence in the direction of the professional careers of his staff and was a personal friend to most of them.

Morris was much in demand as an advisor and teacher. He taught statistics courses at the Graduate School of the U.S. Department of Agriculture. He was a member of the Advisory Committee to the U.S. Office of Statistical Policy, an advisor to the Program for National Assessment of Educational Progress, a member of the Committee on Statisticians in Governmental Service (advisory to the Civil Service Commission), a contributor to the report of the President's Commission on Federal Statistics (1971), and a consultant to UNESCO and to the U.N. Food and Agriculture Organization. He visited a number of countries on consultation missions, including Canada, Sweden, Japan, and India. In India on one occasion he met with Prime Minister Nehru and on another occasion with Chinese Premier Chouen-Lai, who was visiting India at the time.

Morris was one of the founders of the International Association of Survey Statisticians and, virtually by acclamation, its first president.

Among the many honors and awards Morris received are the following:

Elected to National Academy of Sciences, 1976

Honorary LL.D, University of Wyoming, 1959

Rockefeller Public Service Award, 1962

Department of Commerce Distinguished Service Award

Fellow of the American Statistical Association (vice-president, 1951-53; president, 1960)

Honorary fellow of the Royal Statistical Society

Honorary member of the International Statistical Institute

Fellow of the Institute of Mathematical Statistics (vice-president, 1947; president, 1953)

First president of the International Association of Survey Statisticians, 1973-77

Morris was also a member of AAAS, the Inter-American Statistical Institute, the Population Association of America, and of the Sigma Xi, Alpha Tau Omega, and Phi Kappa Phi fraternities.

Morris's principal services to the National Academy of Sciences and the National Research Council were:

Member-at Large of the NRC Division of Behavioral Sciences, 1968-70

Member of the Advisory Committee on Problems of Census Enumeration, Division of Behavioral Sciences, 1969-71

Member of the Committee on National Statistics, 1972-76

Member of the Panel on Incomplete Data, Committee on National Statistics, 1977-84

Member of the Committee on Ocean Science Manpower Trends and Curriculum Needs, Ocean Sciences Board, 1978-79

Member of the NAS Report Review Committee, 1978-82

Member of the Board on International Comparative Studies in Education, Commission on Behavioral and Social Sciences and Education, 1988-90

Morris had remarkable energy, most of which he devoted to his work, but he found time to be a good friend to many and a good family man, and to get some recreation in boating and hiking. He married Mildred R. Latham in 1930, and they had four children: Evelyn Maxine, Morris Howard, James Hans, and Kristine Ellen. (With good statistical tech nique, they were stratified evenly into two boys and two girls.) Mildred died in 1983. Morris married Eleonore Lamb in 1986; she survives him and remains as stepmother to Morris's children.

Morris never retired. He continued to be active as a company executive, working statistician, consultant, author, and member of advisory committees to the time of his death. His last paper (with Benjamin J. Tepping) appeared only a week before he died in the September 1990 issue of the *Journal of the American Statistical Association.*

His death was followed by memorials throughout the statistical world. The March 1991 issue of the *International Journal of Official Statistics* was dedicated to his memory. So was the December 1990 issue of the Canadian journal, *Survey Methodology,* whose dedication said, "This issue is dedicated to the memory of Morris H. Hansen, a pioneer, innovator and leader who made fundamental and lasting contributions to many aspects of survey methodology." An international conference on measurement errors in surveys and a book[8] containing the invited papers presented at the conference were dedicated to his memory. Westat provided funds to the Washington Statistical Society to establish an annual Morris Hansen lecture, featuring eminent statisticians from the United States and other countries.

THIS MEMOIR IS BASED on the personal recollections and files of the authors; the files of the history branch and of Mary Ann Cochran of the Bureau of the Census; brief biographies in standard reference volumes and in *The American Statistician* of February 1991; an article of reminiscences by Morris Hansen and an interview of Morris Hansen by Ingram Olkin, both in *Statistical Science* of May 1987; an unpublished interview of Morris Hansen by James L. O'Brien conducted on June 22, 1983; *Revolution in United States Government Statistics 1926-1976* by Joseph W. Duncan and William C. Shelton, published in October 1978 by the Office of Federal Statistical Policy and Standards, U.S. Department of Commerce; various statistical reports of the Bureau of the Census; records of the National Academy of Sciences; and other sources.

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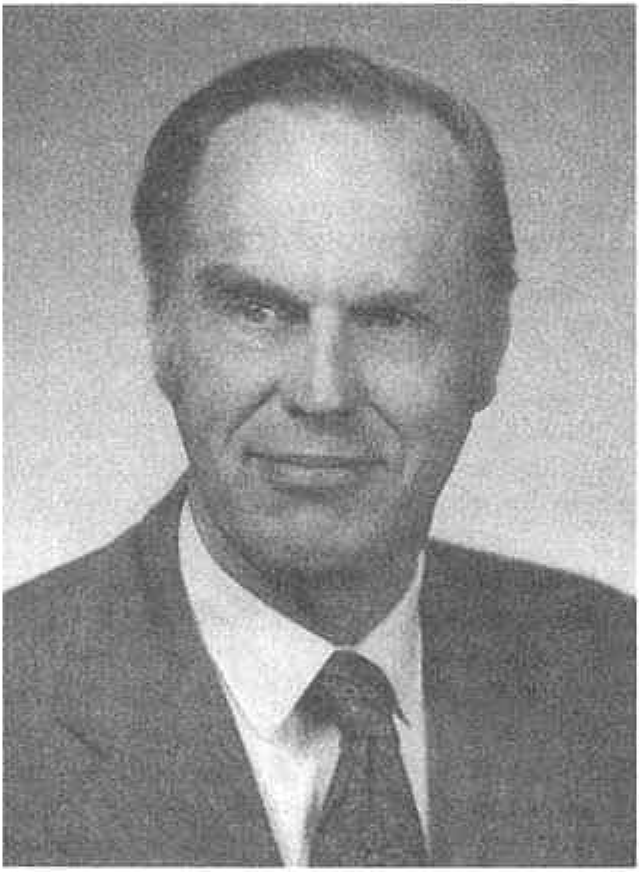

Courtesy of Institute of Environmental Studies, University of Wisconsin—Madison

Robert P Hanson

ROBERT PAUL HANSON

May 14, 1918-July 27, 1987

BY THOMAS M. YUILL AND B. C. EASTERDAY[1]

THE PUBLIC, AS WELL AS the scientific community, is surprised and concerned about the periodic emergence of new infectious diseases. This concern has been fueled by recent outbreaks of Ebola virus disease in people in Zaire, Venezuelan equine encephalitis and fowl plague in Mexico, "mad cow disease" (bovine spongiform encephalopathy) in Europe, and the spread of avian cholera in wild waterfowl in North America. Bob Hanson would not have been surprised at all by these disease trends. He was familiar with "surprise" disease outbreaks. His career was built on determining where these surprise diseases came from, how they spread, and how they might be controlled.

Bob Hanson insisted that we should *expect* to see the emergence of new diseases as ecological and environmental conditions and host populations change. He operated on the basic premise that infectious agents must be viewed and attacked holistically because they are components of the ecosystems in which their hosts live. He recognized that no one person had all the expertise required to understand the natural history of diseases, and he was a champion of interdisciplinary team approaches. The problems on which he focused were always real ones that originated in nature.

He recognized the need for close laboratory and field interaction. He was a master at getting people to work together on a common, often "new," problem. For example, he was the glue that united laboratory virologists, wildlife ecologists, medical entomologists, veterinarians, and public health physicians in their successful efforts to discover the mechanisms for maintenance and spread of La Crosse virus, a cause of encephalitis in children mainly in the upper midwest. He countered the decline in the preparedness of the scientific community to deal with disease recognition and control by continuing to train students to look beyond their subspecialties and see the bigger picture of which their narrow areas were a part.

Bob recognized that not all viruses are created equal, even though they appear to be quite similar. His focused curiosity led to the recognition that, although Newcastle disease virus strains from the field were antigenically identical, they produced a range of disease severity in poultry from mild to highly fatal. He spent much of his career answering the questions: Why does a given virus produce different disease states in the same host or in different hosts? Why are lethal virus strains favored in some situations and mild ones in other circumstances? How do environmental factors affect the host-virus relationship and resulting disease? How do viruses move within and between populations of animals?

He recognized the growing need to unite molecular and epidemiological concepts and techniques as a seamless continuum in the study of the host-virus-environment interaction. His students and postdoctoral fellows worked at this interface. Were he with us today, he would still be arguing for more support for research and training in "landscape" epidemiology, pointing out that if we are willing to spend tens of millions of dollars on the control of disease in an emergency situation, it would be wise to spend consistently a reasonable fraction of that on understanding how the agents of those diseases are maintained and spread in nature, to prevent the surprises that will surely come again in the absence of adequate knowledge.

PERSONAL HISTORY

Robert Hanson was born on May 14, 1918, in Sarona, Wisconsin. He was the first of five children born to Fred and Marion (Bergquist) Hanson. His Scandinavian roots were deep, his father's family having left Norway to settle in Illinois in 1839 and his mother's family from Sweden to Minnesota in 1888. His youth was spent on a small northern Wisconsin family farm, and he graduated from Ashland High School in 1936. His rural early environment in Wisconsin's North Woods kindled a lifelong interest in nature that provided the ecological vantage point for his later microbiological research and teaching. From his youth onward Bob Hanson was fundamentally a naturalist. His parents encouraged Bob and all the children to pursue their own interests. His brother Elmer recalled that ". . .Bob was inherently a scientist from the very beginning, empirically investigating everything from bugs to flowers as a child. When he was in high school he transformed Mother's flower beds into an elaborate arboretum, building a rock garden, an ornamental fish pond and replanting wild flowers."[2] He earned his B.S. degree at Northland College in Ashland, majoring in biology and writing a popular local newspaper column entitled "In Bloom this Weekend" in which he described the flowers that would be seen during the coming weekend. The Second World War introduced him to research on exotic foreign animal diseases. He was stationed at the joint U.S. and Canadian Army Gross Isle Biological Research Station from 1943 to 1945. This experience and the contacts he made there would shape his subsequent career. He married Martha Goddert following the war, and they had two children, Allen Neil and Diane Gail. Martha Hanson shared her husband's interest in natural history, and in the last decade of his career they became partners in science, publishing and teaching together on the ecology and control of animal diseases.

Bob's wartime animal disease contacts with Richard Shope, James Baker, and Fred Maurer led to graduate training at the University of Wisconsin, Madison, and then a faculty position there in the departments of veterinary science and bacteriology. His unfailingly friendly, unassuming personality coupled with keen scientific insight and imagination attracted students and fellow faculty members and scientific colleagues from around the world. Early in his career his mentors Carl Brandly and Sam McNutt sparked his interest in poultry diseases and viral pathogenesis. His interactions with colleagues from many disciplines were essential for the development of a career that was a remarkable combination of depth and breadth. His collaborators were as varied as they were numerous, and included David Berman and J. B. Wilson (bacteriology and epidemiology), Elizabeth McCoy (virology), William Hinshaw (vesicular diseases and Latin American contacts), Jacob Traum (foot-and-mouth disease epidemiology and virology), Duard Walker (slow virus diseases, medical virology), Gabriele ZuRhein (neuropathology, slow virus pathogenesis), and Robert McCabe (wildlife ecology), to mention a few. He was part of the faculty team that offered the first virology course on the Madison campus, with colleagues Dennis Watson and Elizabeth McCoy. Many of his scores of graduate students went on to become active colleagues and collaborators later on, and their names appear with his in the many jointly authored research papers and reports published over the years.

His achievements and contributions garnered considerable recognition. He found particular satisfaction in three honors. On the university level he was selected for a coveted named professorship in 1975, which he chose to designate the S. H. McNutt Professorship in honor of his highly respected mentor and colleague. Nationally, in 1975 he was granted honorary membership as a non-veterinarian in the American Veterinary Medical Association for his contributions both to animal health and to the veterinary medical profession. His greatest recognition was election to the National Academy of Sciences in 1979.

Even though he was a resident of the city of Madison throughout his career, the pull of rural Wisconsin was ever present. He and his wife purchased a small farm in the hilly country 25 miles southwest of Madison, selected because of its woods, natural beauty, and the presence of a log cabin constructed by Norwegian settlers a century before. Later the adjoining farm was purchased, too. The farm became Bob's and his family's weekend escape from the city and a place to return to nature throughout the year. It became the site of important field work that led to an understanding of the natural history of La Crosse virus. It was also the place where he died unexpectedly in his sleep the night of July 27, 1987. He had never complained, but a few of us realized that he had a heart problem—just an odd comment a few months before that he would not join a group from the university going to The Gambia for an animal health and production workshop because "his doctor wouldn't let him go." He was full of life right up to the end, and was planning an active retirement project to establish an inter-university group for international animal health and disease control. That idea lives on.

PROFESSIONAL HISTORY

Although Newcastle disease was the major theme that ran throughout his career, there were interesting and productive "side trips" that came along and were fruitfully pursued. When these side efforts grew too extensive or complex for him to continue he cheerfully and selflessly passed them along to others. If Bob had a standard modus operandi it was to recognize a new disease problem in the field, find out what was causing the disease and contributing to its transmission with carefully conducted laboratory studies, and then validate the experimental results and put them in their proper biological perspective with additional field observations. Graduate students were given a remarkably free rein to follow their interests and intuitions in the laboratory or field. His guidance and stimulation were more subtle than overt. Often, the student did not recognize that a key concept or experiment that arose from a discussion was more Bob's than the student's. Ownership or origin of the idea was not important to Bob—the idea itself was. Students were allowed (often encouraged) to set out on an interesting scientific side trip, and the journey was usually followed with encouragement. As one of his former students has written, "Bob Hanson had a way of making scientific inquiry exciting. He accomplished that by his words of advice and guidance, but more importantly, by his example. Bob wasn't one to say, 'that approach won't really get you anywhere' or 'that direction will be a dead end.' He would listen to you and respond by pulling from his vast memory the work of others that was relevant to your idea, but I never heard him put an idea down as not being appropriate or not being likely to provide useful information."[3] Which was a good thing, or he might have told one of the students that it was a waste of time to look for La Crosse virus in mosquito larvae (per the conventional wisdom of the day), and the discovery of transovarial transmission of the bunyaviruses would have come along much later, and from another institution. The virus-containing mosquito larvae came from tree hole water from the woods on Bob's farm. Of course, some of the side trips did turn out to be dead ends. Bob recognized that those dead-end trips were the price of encouraging imagination and also taught very important lessons about taking risks, dealing with failure, and cutting losses.

Bob Hanson's career does not fall into neat periods. His career was a combination of specific focus—mainly on Newcastle disease—and taking advantage of unexpected targets of opportunity as they arose. His career can be summarized in approximately five year periods.

1946-49: THE FORMATIVE YEARS

His graduate training in the departments of bacteriology and veterinary science at the University of Wisconsin, Madison, began his career-long exploration of Newcastle disease virus (NDV). He developed laboratory diagnostic procedures, including virus cultivation in embryonated chicken eggs.

1950-55: CONTINUED LINES OF RESEARCH PUNCTUATED BY SOME SURPRISES

Work continued on NDV diagnostic test development. Studies were begun on NDV pathogenicity in chickens and susceptibility of other avian hosts as well as a mouse model. Vaccine development continued, various virus strains and isolates were compared, and a world repository of these strains and isolates was begun. Two surprises occurred in Wisconsin—vesicular stomatitis (VS) and eastern equine encephalitis (EEE) appeared for the first time, and studies on their epidemiologies were done utilizing both field ob servation and laboratory experiments to determine the roles of domestic and wild vertebrate hosts and arthropod vectors. His field investigations of VS and EEE virus epidemiology kindled his interest in wildlife diseases, as he and his research teams looked for the virus in the Wisconsin River bottoms.

1956-60: ONGOING DIRECTIONS MIXED WITH NEW OPPORTUNITIES

Research on Newcastle disease, vesicular stomatitis and eastern equine encephalitis viruses and their structures, pathogenesis, epidemiologies and possible control continued. His interest in host-virus relationships prompted him to organize a highly successful symposium on virus latency. The VS and EEE field studies were extended to Georgia through collaboration with the health department of that state. Swine influenza appeared in Wisconsin and its occurrence and transmission were studied, and reconnected him with his Gross Isle mentor Richard Shope. A new target of opportunity appeared through collaboration with the Wisconsin Department of Natural Resources on an evaluation of the role of white tail deer in the maintenance and spread of leptospirosis. Fibroma virus was found fortuitously in local cottontail rabbit populations, and its transmission and host-virus relationships were studied.

1961-65: HOST-VIRUS-ENVIRONMENT INTERACTIONS AND VIRUSES IN NEW PLACES

The effects of air pollution and environment on host susceptibility and pathogenesis of NDV were studied. VSV cattle vaccine development progressed to tests in endemic areas in Georgia and Panama. Additional EEE virus vector and avian host studies in Wisconsin were completed. New diseases appeared in Wisconsin and other places and were studied to determine their patterns of transmission and disease. The epidemiology of California and western equine encephalitis viruses were studied in wildlife in Alberta, Canada, as part of collaborative studies to determine factors that regulate wildlife populations there. Avian botulism appeared in Wisconsin waterfowl and laboratory investigation of toxin transmission was done. Mink enteritis (ME) was recognized as an increasingly important disease in Wisconsin (and other fur-producing states), and Bob responded to a request for help from the fur breeders with studies to determine ME epizootiology and host-virus relationships.

1966-70: OLD DISEASES, MORE NEW DISEASES

Investigation of NDV transmission and air pollution effects continued, a new line of research on nutritional status and NDV susceptibility was begun, and the world NDV repository expanded with the addition of new isolates. VSV immunity in cattle and laboratory animals challenged conventional wisdom about lifelong immunity following infection. The geography and epidemiology of California (La Crosse virus) and other bunyaviruses in Wisconsin were expanded to include an assessment of the effects of infection by these viruses on their wildlife hosts in Alberta. Arthropod-borne virus (arbovirus) research was expanded to include laboratory studies on St. Louis encephalitis virus in a mouse model. Another new disease appeared in the Wisconsin fur industry—transmissible mink encephalopathy (TME)—and the host range, neuropathology, and physiochemical properties of this scrapie-like, slow, and unconventional presumed viral agent were begun. Avian mycoplasmas were recognized as important pathogens in commercial midwestern poultry operations, and diagnostic systems, including isolation and characterization of these agents, were developed. An unexpected finding of a chlamy dial agent in wildlife in western Canada led to its isolation and characterization.

1971-75: ENDEMIC VIRAL DISEASE PROBLEMS AT HOME, EPIDEMIC EMERGENCIES AWAY

Bob Hanson and his laboratory were called on to support the U.S. Department of Agriculture's emergency program to combat an extensive, costly outbreak of highly virulent Newcastle disease in southern California. His contributions were recognized in a citation awarded by the Secretary of the U.S. Department of Agriculture. More routine NDV work continued on nutrition, environmental factors and host susceptibility, virus strain differences and mutability, and host immunoresponsiveness to vaccination. Further work was done on TME neuropathogenesis, host range, characterization of the agent and its comparison with the scrapie agent, and mechanisms of transmission. Significant progress was made on the development of laboratory diagnostic tests and structural study of avian mycoplasmosis. Work on La Crosse virus by the interdisciplinary team provided new insights into its epidemiology in Wisconsin (wildlife hosts and mosquito vectors); transovarial transmission in mosquitoes was discovered and its role in overwintering was described. The close collaboration developed with the Colombian animal health agency and with the USDA Plum Island Laboratory led to collaborative work on foot-and-mouth disease antigenic analysis. He concluded a nine-year stint as chair of the NAS-NRC Subcommittee on Poultry Diseases (1963-72).

1976-80: A TIME OF CONSOLIDATION OF RESEARCH PROGRAMS

Steady progress continued with NDV, including factors affecting transmission, continued strain characterization, additional pathogenesis studies, and antigenicity and vac cine effectiveness. Important advances were made in the understanding of TME pathogenesis, comparative studies with the scrapie agent, and the development of the hamster model. Experimental studies of infection of rabbits with chlamydia were completed. Bovine immune responsiveness to Colombian isolates of foot-and-mouth disease was assessed. He completed a four-year term as a member of the NASNRC Board on Agriculture and Renewable Resources (1973-77).

1981-87: REFLECTIONS OF THE "BIG PICTURE"

Bob Hanson's career concluded on a reflective note of holistic approaches to animal health. He blended his years of field and laboratory investigations and participation in significant disease control programs—especially Newcastle disease in California and foot-and-mouth disease in South America—into insightful writing, speaking, and teaching about biological, social, and economic elements essential to deal effectively with diseases in animal populations. He and his group recognized early on the power and utility that new molecular tools emerging from basic virology brought to epidemiology, pathogenesis, and control or prevention of the economically important diseases that he had been studying over the years. Although his sudden and unexpected death cut off his research contributions, in this last stage of his career he particularly addressed the need to apply his and others' research results to what ultimately matters—a reduction of disease impacts in the real world.

CONCLUSION

Many mentors admonish their graduate students to "focus, focus, focus!" Bob never did that. Rather, he urged his students to go where the interesting ideas led. He recognized that specialty was necessary to achieve essential basic competence, but that a high degree of specialization could limit the pursuit of a problem to its final conclusion. The solution to dealing with the risk of getting out of one's depth in a new area was to enlist the collaboration of a congenial colleague who could bring the needed expertise to the enterprise. It was natural for him and his students to work with others as teams. The arbovirus group (to evolve into the La Crosse virus group), the slow virus (transmissible mink encephalopathy/scrapie) group, the wildlife diseases group, and the animal disease control group all came together around Bob. His quiet demeanor and total lack of egocentricity made it natural for him to establish the trust needed for an open and collaborative environment for these groups to come together and generate a high level of enthusiasm about the problems they were involved in solving. It was his personality and operational style, in addition to his keen intelligence and holistic vision, that made it possible to contribute to so many areas in the infectious disease arena.

These contributions made him a superb trainer of young scientists, advisor to governments and international agencies on animal health problems and policies, and a stimulating teacher. In speaking of Bob Hanson, colleagues would comment on what an insightful scientific thinker he was and then invariably add, "and such a fine human being."

NOTES

1. We were privileged to have Bob Hanson as our major professor and mentor for our Ph.D. degrees granted in 1961 (Easterday) and 1964 (Yuill). We were also privileged to return to the university and become faculty colleagues and personal friends for those many years before his death in 1987. We recall with fondness and respect the sessions of scientific "stargazing" with him in the office, the laboratory, and in seminars. And we recall the smell of wood smoke

and the taste of cider on crisp autumn days as he shared his "Pinnacle Farms" hospitality with us, other colleagues, and students. During the course of writing this profile we had abundant opportunity to reflect on the influence Bob Hanson had on us. We reaffirmed our conclusion that whatever scientific and professional opportunities, adventures, contributions, and successes that we have enjoyed during our own careers we owe in great measure to the guidance, example, and caring given to us by Robert Paul Hanson.

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Courtesy of Stephen Heizer

Robert F. Heizer

ROBERT FLEMING HEIZER

July 13, 1915-July 18, 1979

BY THOMAS R. HESTER

ROBERT FLEMING HEIZER WAS one of the preeminent archaeologists of the twentieth century. A longtime professor of anthropology at the University of California, Berkeley, Heizer made scholarly contributions to archaeology, anthropology, ethnohistory, and history. Much of his research and the many publications that followed dealt with the prehistoric and historic Native American peoples of the western United States, particularly Nevada and California. He was a pioneer in the field of scientific applications to archaeology, principally in research dealing with radiocarbon dating in its early phases in the 1950s and then with trace element analysis of obsidian (volcanic glass) artifacts in the 1960s and 1970s.

Heizer also was deeply involved in the early application of cultural ecology in North American archaeological sites. Much of this research stemmed from analyses of preserved materials from ancient Nevada caves, primarily coprolites (fossil feces) that were a direct reflection of human diet

This work draws heavily on Hester (1982). Some portions originally appeared in *American Antiquity* 47:99107. Copyright 1982 by the Society for American Archaeology. Reprinted with permission and dietary change through time. Heizer became active in fieldwork in Mesoamerica in the 1950s, continuing up to the time of his death. His excavations provided insights into cultural evolution of the Olmec civilization, and collaborative efforts with geologists and chemists provided new data on trade patterns in prehistoric Mexico and Guatemala. Heizer's prodigious publication record leaves a tremendous resource for future generations of archaeologists and anthropologists in the study of many facets of ancient and early historic human life ways.

Heizer was born on July 13, 1915, in Denver, Colorado, the son of Ott Fleming (a mining engineer) and Martha Madden Heizer (a nurse). He married Nancy Elizabeth Jenkins in 1940 (they were divorced in 1975); they had two sons, Stephen and Michael, and a daughter, Sydney. It was during Heizer's youth, much of it spent in Lovelock, Nevada, that he developed his lifelong interest in the culture of the American Indian. He was able to observe the surviving remnants of the northern Paiute peoples and to collect artifacts from prehistoric sites in the area. Curtice (1981, p. 2) reports that Heizer's uncle learned to make chipped stone projectile points from local Indians. Heizer did a lot of reading about Indians and archaeology, and he once told me that his father contacted an acquaintance (perhaps a relative) who worked in Washington, D.C., to secure copies of Smithsonian Institution publications for Bob. Shortly thereafter, a whole set of Bureau of American Ethnology Bulletins, Smithsonian Annual Reports, and related publications were dumped off, in crates, at the Lovelock train depot! At the age of fourteen Heizer worked as a volunteer at the Smithsonian Institution (Curtice, 1981, p. 2).

After graduation (in a class of eleven) from Lovelock High School in 1932 Heizer had wanted to go to the University of California, Berkeley, to study archaeology; how ever, the Lovelock school had an enrollment of only seventy students and several subjects required for admission to the university were not taught there. Heizer later said (here and in some later passages I quote from unpublished reminiscences that he sent me in July 1973) that he "failed the College Entrance Examinations and was advised to go to a junior college" prior to entering what was then called the "State University."

He thus enrolled in Sacramento Junior College in September 1932. During registration an important event took place:

> Along the line, when someone noted that I wanted to be an archaeologist, I was pulled out of line and escorted to meet the president of the institution, Jeremiah Beverley Lilliard—a sweet and dear person who had worked his way up through the city school system until he achieved the presidency of the junior college. From that time on, I became a special ward of his, and I can say that his interest was an important determinant in my life yet to come.

Through the common interest in archaeology that Heizer and Lilliard shared a great deal of fieldwork was done (and the basis laid for later investigations) in the Sacramento Valley and much of this was published. Heizer provides these insights:

> Lilliard, untrained and uninformed, was at the same time extraordinarily perceptive and energetic, and he saw in some fashion which I could not, and can never divine, that one could recover the story of the Indian past by digging and studying the materials recovered. He was absolutely indefatigable, and on Saturdays we would go dig for Indian relics. . . . Always we dug where we hoped to find some poor old buried Indian whose grave would produce some interesting thing. . . . This was pothunting, pure and simple. But (this is not intended as an excuse), we were all interested in Indian culture, though not in the way contemporary archaeologists say they are.

After the experiences in Sacramento Heizer went on in his junior year to Berkeley, where he received an A.B. de gree (with highest honors) in 1936. There was little interest in local archaeology in the department, and to gain additional field experience he dug with Waldo Wedel, then the only archaeology graduate student (Kroeber, 1981, p. 209). When he began graduate school Heizer got a wide exposure to various subfields of anthropology from such teachers as A. L. Kroeber, Robert Lowie, E. W. Gifford, and Ronald Olson. He also carried out fieldwork with Alex Krieger and others in Nevada with funding from Francesca Blackmer Wigg (Heizer, 1970, p. 210); but, when it came time to write a dissertation Kroeber did not want it done on an archaeological subject, and a study on aboriginal whaling in the Old and New Worlds resulted. Another key part of Heizer's training was his work in 1934 and 1935 with Ales Hrdlicka in the Aleutian Islands. Heizer liked to talk about those experiences with Hrdlicka; more importantly, he saw through to publication many years later the findings of some of those investigations.

Heizer received his Ph.D. in 1941. His graduate school experience led to a long professional relationship with Alfred L. Kroeber for whom he had a tremendous respect, speaking of him fondly as "my teacher." Heizer and his family were living in Paris, where he was doing research at the Musée de l'Homme, at the time of Kroeber's death there in 1960.

Heizer's academic career began with a one-year appointment for 1940-41 at the University of Oregon. During World War II he worked for four years and four months as a marine pipe fitter in the Richmond (California) Kaiser shipyard. Following the war he resumed teaching, this time as an instructor at the University of California at Los Angeles in 1945-46. In addition to his full-time teaching (paid at $1,800 per year), he worked with sociologist Edwin Lemert at the tank site in Topanga Canyon. His thirty-year career in the Department of Anthropology at the University of California, Berkeley, began in 1946 with an appointment as assistant professor; in 1948 he was promoted to associate professor and in 1952 attained the rank of professor. He worked closely with many students, both undergraduate and graduate. He particularly enjoyed working with a group of students that he had in the late 1940s and early 1950s (Martin A. Baumhoff, James A. Bennyhoff, Albert B. Elsasser, Clement A. Meighan, Francis Riddell, and others). Collaborative efforts among students and professor led to the first version of *A Guide to Field Methods in Archaeology* (1949) as well as the popular volume, *The Archaeologist at Work* (1959). Other student groups, beginning in the late 1960s and continuing through the 1970s, did extensive fieldwork in the Great Basin. Heizer, in an unpublished note, wrote that this was his happiest period as a teacher and researcher. A great deal of fieldwork and, of course, subsequent publication was done by students like C. William Clewlow, Jr., James F. O'Connell, Richard Ambro, Richard Cowan, Lewis K. Napton, Karen M. Nissen, James Bard, and Colin Busby. My own experiences as a student under him were particularly enlightening. I successfully resisted his efforts to steer me into the coprolite analysis program, and while I did my share of Great Basin research, we began to work more in the area of obsidian analysis, lithic use-wear and functional studies, typology, and research on Olmec and Egyptian monuments.

Indeed, Heizer's strength in teaching was in working with students through research projects, by steering them into new methodological and theoretical approaches, putting them into contact with colleagues in other disciplines, and sharing with them his enormous knowledge of the archaeological literature.

In addition to full-time teaching and instructing summer field classes, Heizer organized and directed the University of California Archaeological Survey from 1948 to 1960, when it was disbanded. The survey was responsible for many major excavations and field studies in California, and Heizer personally saw to it that the results were promptly published in the seventy-five volumes of its Reports series. The style of the Reports was an inexpensive mimeographed one, as Heizer was more interested in rapid and wide dissemination of information than in a fancy format. The survey was succeeded in 1960 by the Archaeological Research Facility, a research unit with broader goals. Heizer became coordinator of the facility and served in this capacity until his retirement. A major publication series, the Contributions, was initiated and more than thirty-five volumes were published under his supervision. Several volumes dealt with Mesoamerican topics and resulted largely from the collaborative efforts of Heizer and John A. Graham. As with the Survey Reports, publication in the Contributions was quick; upon completion of a manuscript Heizer saw that it went immediately into production. This often meant that he had to raise the money for printing through his friend Dean Sanford Elberg.

Heizer retired from teaching and administration in 1976; however, he continued to be involved in archaeological projects up to the time of his death.

During his career Bob Heizer received numerous research grants. He took enormous pride in obtaining what he felt was just enough money to carry out a project; he then stretched every dollar in terms of fieldwork and publication. He received grants from the National Science Foundation, the National Geographic Society, the Wenner-Gren Foundation, the American Philosophical Society, and from a variety of private funding sources. I cannot find one of these which did not result in substantive archaeological rewards.

Let us look at Heizer's record of publication. It is staggering in terms of numbers, creativity, and anthropological diversity. An almost complete bibliography of Robert F. Heizer's works appears in the *Journal of California and Great Basin Anthropology* (Clark, 1980), with a set of comments by Philip Wilke (an extensive list of citations is also found in Hester, 1982). My inventory of his publications is somewhat at variance with figures published earlier; this doubtless reflects the use of different publication lists and my inclusion of publications that appeared after his death. Heizer's scholarly output totaled at least 500 items: 415 papers, reprinted papers, reports, prefaces; 30 books (authored, co-authored, or edited); 53 book reviews; and 2 films.

It is impossible within the space limitations of this memoir to attempt any sort of detailed analysis of his publications. I offer the following observations and refer the reader to other evaluations by Clark (1980) and Baumhoff (1980). Heizer's publications are dominated by archaeology, principally on the prehistory of the Great Basin and California. Also figuring largely are works on Mesoamerican archaeology, especially on the Olmec, stimulated initially by his research with Philip Drucker at La Venta in 1955. Interdisciplinary research (much of what would now be called archaeometry) is also very important in his publications. This is especially true of his collaboration with physiologist Sherburne F. Cook. Their innovative studies on the chemical analysis of human bone, midden constituent analysis, and on quantitative methods are still important references.

Among the other scientists with whom Heizer worked, and often published, were Howell Williams (geology; extensive studies of the stone sources for Olmec monuments); Robert N. Jack (geochemistry; trace element research on Mesoamerican and Californian obsidian); Fred Stross, Frank Asaro, I. Perlman, and Harry Bowman (nuclear chemists at the Lawrence Berkeley Laboratory; Mesoamerica, Egypt); W. I. Follett (studies of fish remains); and Rainer Berger (radiocarbon analyses).

Heizer's publications also reflect a continuing interest in archaeological methods, and he was well known for the six editions of his text on archaeological field methods. His main approach to fieldwork, from my perspective and based on his own comments, was one of flexibility and adaptability; use what is best for the problem at hand and, above all, be observant and diligent in record-keeping. As he wrote in the introduction to the 1975 edition of *Field Methods in Archaeology* (Hester et al., 1975), "There is nothing very exciting about techniques for recording data during an excavation; one simply has the obligation to record whatever information is there." Many solid site reports resulted from his field investigations.

A review of Heizer's publications reveals this sample of some of his other archaeological interests: the archaeology of hunters and gatherers, principally in California and the Great Basin; early human occupation of the western United States; rock art; historical archaeology; prehistoric technology and material culture studies; archaeological bibliography; history of archaeology; dating techniques; cultural ecology, especially as learned through coprolite analysis; general archaeological issues and theoretical concerns; and, experiments in archaeology.

Heizer, working with Frank Hole, co-authored an introductory text in archaeology that was one of the most widely used from the 1960s to the 1980s; it went through three editions, and a restructured, briefer version was published in 1977.

An interest in the transport of ancient heavy monuments was kindled in Heizer through his work with Olmec colossal sculpture. He began a research program that was world wide in scope and later led to a project in Egypt in 1971 and 1972, funded by the National Geographic Society. On the west bank of Thebes at Luxor, Heizer and his team studied the New Kingdom statues known as the Colossi of Memnon. Trace element analysis using the techniques of nuclear chemistry was employed to determine the quarry origins; then the aim was to study the attendant problems of transport and placement of these 720-ton monoliths. Through the efforts of Stross, Perlman, Asaro, and Bowman this project was successfully completed and remains, as far as I know, the only research of its sort yet carried out with Egyptian monuments. The methodology developed through the work of the project still has tremendous potential. As was typical of most Heizer projects the Colossi investigations had spinoffs, particularly an ethnoarchaeological study of contemporary Egyptian stone-vase manufacture, carried out by Heizer, John Graham, and the author.

Thorough reports on a number of major sites are found among Heizer's publications. These include Humboldt Cave, Lovelock Cave, and the Eastgate shelters, Nevada; La Venta, Tabasco; Uyak, Alaska; and Hum-67, California. In addition to the geographic areas represented by these sites Heizer also did fieldwork in Peru, Egypt, Guatemala, and Texas. In the year prior to his death he worked with his friend Giancarlo Ligabue and the Centro Studi e Ricerche Ligabue (he was a member of the Centro's Scientific Committee) on the planning of fieldwork slated for the Maya site of Colha, Belize. His deteriorating health kept him from going into the field during the first season at the site in the spring of 1979.

Throughout his career Heizer published extensively on the ethnology and ethnohistory of the Indians of California and Nevada. Baumhoff (1980) has dealt with this research facet in some detail. His interest in California Indians went beyond the scholarly level. This included his work with

Kroeber on the Indian claims cases in 1954-1955 and his personal involvement as a consultant to the legal assistance program for California Indians. He also saw to it that many of the documents in the C. Hart Merriam collection were published (Simmons and Bickel, 1981). He wrote eloquently, and often bitterly, about the treatment of the native peoples of California. He has been honored since his death by a symposium held in October 1980 by the American Society for Ethnohistory. This organization has also established an annual Robert F. Heizer Memorial Prize for the best annual article using an ethnohistorical approach.

Aside from writing, which he loved and which he seemingly did almost continuously, Bob Heizer greatly enjoyed the chores of editing. This was expressed in several books, the *Archaeological Survey Reports,* most of the Archaeological Research Facility *Contributions* up to 1976, and as one of several editors for the *University of California Publications in American Archaeology and Ethnology* (as well as in the *Anthropological Records* also published by that university). He was also on the editorial board of a number of journals. With the help of Gerald O'Neal he started the *Ballena Press Publications on Archaeology, Ethnology and History* (fourteen volumes had been published at the time of his death). He assembled and edited the California volume of the *Handbook of North American Indians* (Volume 8); typically, the task was done efficiently and was the first volume to be issued (1978) in this major publication series (a library journal described it as one of the ten most important references of 1978). Heizer had long maintained that there was a need for a statewide anthropological journal for California and he was thus one of a group of people who helped start the *Journal of California Anthropology* (and its successor, the *Journal of California and Great Basin Anthropology*).

Finally, in regard to Heizer's extensive publications, it should be noted that much of his work was collaborative. He took much pleasure from such research and the exchange of ideas that it engendered. Most such collaboration was with students and colleagues; for example, there were five to ten (or more) papers and books each with M. A. Baumhoff, C. William Clewlow, Philip Drucker, Albert Elsasser, Franklin Fenenga, John A. Graham, and Lewis K. Napton. I worked with him on more than twenty papers and books. An equal amount of collaborative research was done with colleagues from other scientific disciplines, all of whom have been mentioned earlier; at least sixteen papers resulted from his joint research with S. F. Cook.

Numerous honors came to Robert F. Heizer as a result of his distinguished career. These included an honorary Doctor of Science degree from the University of Nevada (1965), two Guggenheim fellowships (1963 and 1973), a year as a fellow in the Center for Advanced Study in the Behavioral Sciences (1972-73), an award for "distinguished scholarly contributions" from the Southwestern Anthropological Association (1976), the Henry R. Wagner Medal of the California Historical Society (1977), and repeated listings in *Who's Who in the World.* He was elected to the National Academy of Sciences in 1973.

Those are the basic statistics. What lies behind them is a brilliant anthropological career and a record of accomplishments that few can hope to attain. Heizer as a person was most typified by his incredible energy. He was competitive, aggressive, and did not shrink from controversy. He tremendously enjoyed fieldwork and the challenges and prospects of discovery that it presented. If conditions deteriorated he responded to the challenge and worked harder, as I well remember in two quite different environs: recording southern Nevada rock art in a snow flurry and documenting the Colossi of Memnon in 122° heat! His energy ex tended, as we have already seen, to publication, for in his view fieldwork, a laboratory project, or any type of archaeological endeavor was worth nothing unless it was published.

To say he was energetic about archaeology is indeed an understatement. He was obsessed with archaeology. Lunches on Telegraph Avenue, airplane journeys, sitting in a bar in the Winter Palace Hotel at Luxor, all were locales for Heizer to take pen in hand, get out one of his ubiquitous yellow note pads, and proceed to outline a paper, develop some ideas for a future project, or jot down another reference to be squirreled away until the next revision of one of his many books.

From a personal perspective, I knew Heizer for ten years, first as his graduate student (1969-72) and then as a colleague, co-researcher, and close friend tip to the time of his death. He had a complex personality, and this led to a number of stormy relationships with graduate students, not to mention some of his professional colleagues (see Baumhoff, 1980; Curtice, 1981). While I am sure that many such encounters and feuds grew from a variety of causes, some stemming from his own personality, Heizer's primary explanations were that such persons were "not serious about archaeology" or that they did not "produce." I found him to be a very generous person, particularly with his time, his knowledge, and his experience. He had a keen wit and an engaging, albeit sardonic, sense of humor. I more than once expected that we would be tossed out of a restaurant or bumped off an airplane as he mumbled out incisive commentary on an inept waiter or offered his pointed appraisal of some fellow traveler. And many situations called forth a joke or anecdote derived from his fondness for the writings of Mark Twain.

Heizer died on July 18, 1979. He was initially hospitalized with cancer in June 1978, although he had been in poor health for several months prior to that time. He waged a determined battle against the disease and despite its debilitating effects continued research and teaching until a few days before his death. Elsasser (1979, p. 151) notes that Heizer had twelve manuscripts in preparation or in press at the time.

Robert F. Heizer leaves a considerable legacy to archaeology, in terms of fundamental and often far-reaching research and more than three decades of teaching and lecturing. He made a number of gifts of books and special literature collections to several institutions; collections on rock art went to UCLA and to the University of Texas at San Antonio. At the latter there is also a special collection of publications on Olmec archaeology. His personal notes and records on La Venta and on other facets of his research went to the National Anthropological Archives at the Smithsonian Institution. Heizer's personal and professional letters as well as assorted research notes are now in the archives of the Bancroft Library at the University of California, Berkeley.

I AM GRATEFUL, TO a number of persons for providing information on Robert F. Heizer. An insight has been provided by others, including J. Desmond Clark, A. E. Elsasser, Lynette Curtice, Karen Nissen, Colin Busby, and L. K. Napton. Giancarlo Ligabue, who first met Heizer through a mutual interest in duck decoys (Nevadan and Venetian), encouraged him in many projects and insured that an Italian version of Heizer's ancient heavy transport book was published.

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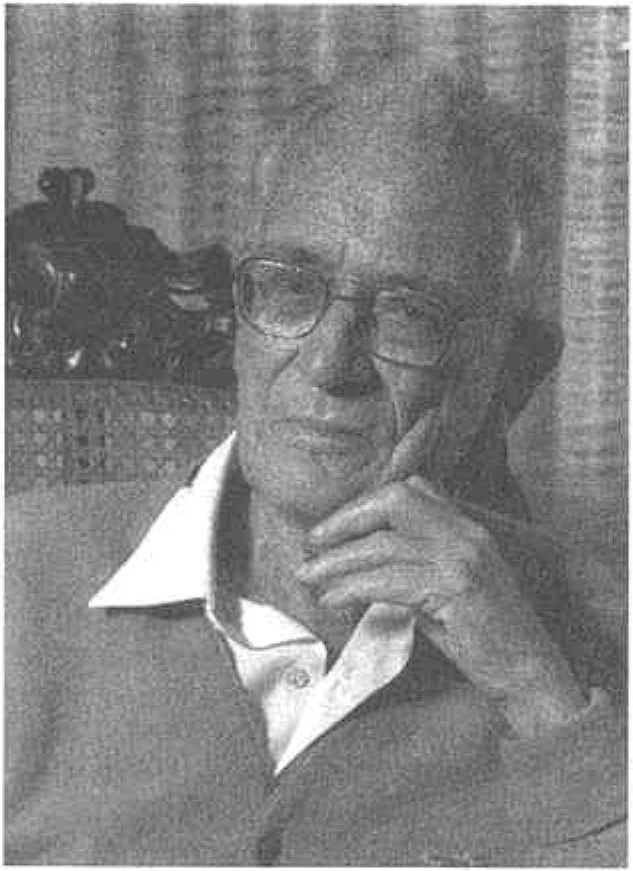

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Frederick S. Hulse

FREDERICK SEYMOUR HULSE

February 11, 1906-May 16, 1990

BY EUGENE GILES

WORLD WAR II and its timing impacted Frederick S. Hulse's intellectual trajectory in physical anthropology in a clearer way than it did perhaps any other of the more than two dozen doctoral students mentored in this field over four decades (1913-54) by Harvard University's extraordinarily influential Earnest A. Hooton. Hulse's graduate work was entirely standard in its primary focus on the notion of race being a nominal reality that can be parsed into its historical constituents by a series of observations and measurements on peoples' heads and bodies. He learned, but did not particularly like, another favorite endeavor in the field, human osteology. His only prewar publications[1] were a description of skeletal remains from an archeology site (1941) and an account of the racial origins of the Japanese (1943). Who would have imagined that from this traditional and tardy beginning—his Ph.D. was awarded in 1934—after a brief, war-induced sojourn into sociocultural anthropology, there would emerge from the conventional physical anthropology encrustment, chrysalis-like, another Fred Hulse.

The new Hulse, the one honored by membership in the National Academy of Sciences, saw the integrated way genetics and culture shaped individual human beings that expressed both. He became an early, and certainly dominant, practitioner of anthropological genetics, conducting research in the United States and a half dozen other countries (mostly Western, a bit unusual for an anthropologist). In his fieldwork he sought and found small human populations that he could use as natural experiments to study, as he once summed up his life work, "the effects of culture . . . upon human biological characteristics and evolution."[2]

EDUCATION

Hulse was born in New York City in 1906, a son between two daughters, Mary and Charity, of Hiram Richard Hulse and Frances Barrows Seymour Hulse. His father was rector of St. Mary's Episcopal Church. New York was his home and his early education was in its public schools, with summers spent at his maternal grandparents' house in Northampton, Massachusetts, until he was nine-years old. The family moved to Havana in 1915 when his father became Missionary Bishop of Cuba. He continued his education at the Cathedral School and then Candler College in Havana, but in 1918 he was sent back to Ridgefield School, a boarding school in Ridgefield, Connecticut. His effort to enter Harvard College on graduation was thwarted by poor scores on his Latin examination, so he entered Williams College. Two years later, unhappy at Williams and having endured a total of seven years of Latin, he was able to transfer to Harvard as a member of the class of 1927.

A pre-Harvard reading of Roland B. Dixon's *The Racial History of Man* persuaded Hulse to give anthropology a try, and, because of his late start, he needed to take all available courses. This led him into the realm of the trio constituting Harvard anthropology in the 1920s, the archeologist-ethnographer-linguist Alfred M. Tozzer, the physical anthropologist Hooton, who was an exceptionally popular undergraduate teacher, and Dixon (despite its title, Dixon's curious book—he later called it "my crime"—reflected his primarily ethnographic interests). Hulse shared an anthropology major with only one other classmate, Milton Katz, who abandoned anthropology after surviving a Peabody Museum-sponsored expedition through central Africa and went on to a distinguished career as Harvard's Henry L. Stimson Professor of Law. Hulse, however, was captivated by Hooton's intellectual arena, and immediately upon receiving his bachelor's degree *cum laude* entered Harvard's graduate program in physical anthropology.

Hooton initially considered Hulse extraordinarily shy, even drab, but warmed to him in time, and acknowledged that his general examination for the Ph.D. was one of the best in many years. From the beginning Hulse needed financial support for his graduate studies, and although he never held a teaching assistantship at Harvard, Hooton provided him employment in a variety of projects. In 1927 Hooton had persuaded Harvard to enter into an arrangement with Pathé Exchange, Inc., for editing and advice on their educational films involving anthropological subjects. Hooton employed Hulse both as a film laboratory assistant in the basement of the Peabody Museum, the home of the Division of Anthropology (as it was then called), and as a sort of traveling salesman for the Pathé anthropological films in the East and Midwest.

A wealthy Cuban, Madam Rosalia Abreu, developed a primate colony at her estate, Quinta Palatino, near Havana which had reached something close to 100 animals, including chimpanzees, orangutans, and gibbons by the late 1920s.[3] Hooton, with support from Pathé, gained Abreu's permission to film her primates for the purpose of better understanding the functional anatomy of locomotion, particularly in chimpanzees. With his Cuban connection through his parents Hulse was a natural for Hooton to send out to Cuba to supervise filming the primates in a variety of locomotor activities when they were let out of their cages, and he did so.

Cuba also became the focus for Hulse's doctoral dissertation, "The comparative physical anthropology of Andalusians and Cubans," completed in 1934. In his research, which was conducted first in Cuba and then in Spain between 1928 and 1930, Hulse examined some 200 Cubans and 500 Andalusians, taking 17 traditional anthropometric measurements and 40 morphological observations, for the purpose of determining the hoary "racial" categories of the different social and geographic groups in Andalusia and the determination of the effect of the large migration from Andalusia to Cuba. Although contemporary white Cubans were larger than Andalusians, most likely because of environmental changes, he believed that their greater physical similarity to upper rather than lower class Andalusians was due to the effective prohibition of migrants with evidence of Jewish or Moorish ancestry during Spanish colonial times. Hulse received his degree in 1934, but his dissertation remained unpublished until a summary of his conclusions about migration to Cuba appeared 45 years later.[4]

THE EARLY CAREER

To support himself while writing his dissertation Hulse accepted a job working for Harry L. Shapiro, the first of Hooton's many doctoral students. Shapiro, affiliated with the American Museum of Natural History in New York, had Rockefeller Foundation support through the University of Hawaii to study the effect of migration on the physical characters of population groups in Hawaii. Hulse's assignment was to collect the entire anthropometric database for Shapiro's investigation of Japanese immigrants to Hawaii and their relatives at home in Japan. This daunting task ended up requiring more measurements (43) and more observations (41) on more individuals in both countries (2,594) than his thesis research. The Hawaii portion examined 2.5% of its Japanese population! He completed the field work in 13 months beginning in January 1931. Hulse's data, when analyzed and published by Shapiro,[5] became one of many studies confirming the environmental plasticity of anthropometric measurements demonstrated years earlier by Franz Boas.

Through Hooton's good offices Hulse had come somewhat under the wing of the eminent anthropologist Alfred L. Kroeber at the University of California, Berkeley. On finishing his dissertation, and having been influenced by his work for Shapiro, he applied for and received a National Research Council fellowship in 1934 to examine Japanese in California to compare with the Hawaiian Japanese. In California he had met and soon married Leonie Robinson Mills in Pasadena on August 26, 1934.

In January 1935 Kroeber placed him on an archeological expedition mounted under the auspices of the university's Institute of Social Sciences to Culiacan, Sinaloa, Mexico, and directed by Isabel Kelly, who had received her Ph.D. only three years earlier under Kroeber and Carl Sauer.[6] It was the first time in Mexico for Kelly, for Hulse, and for his new wife, who continued to accompany him on all his field trips. As soon as he returned in June Kroeber sent him off to Bishop, California, for the summer to collect oral histories about European contact from the older Native Americans. All of this provided Hulse with valuable archeological and ethnographic research experience. In the fall he went back to Hawaii for a year as a research associate to investigate dietary and other potential differences between Japan-born and Hawaii-born Japanese. By his own admission, how ever, the work with the Japanese did not go well either in California or in Hawaii. The good news was the birth of the Hulses' first son, Richard Seymour Hulse, in Honolulu on May 29, 1936.

Hulse got his first chance to teach in the fall of 1936, and with a vengeance. He became an acting instructor (replacing Verne Ray for a year) at the University of Washington in Seattle. Each quarter he was required to teach three introductory courses: ethnology, archeology, and physical and linguistic anthropology. The department chair, Erna Gunther, tried to expand the department and keep him on, but during the depression that was not in the cards. Joining a teacher's union didn't help Hulse with the administration either, and he became unemployed at the end of the academic year. Gunther was able to get him only a couple of inconsequential bits of work and didn't really know how he managed to exist. During this grim period his father died and his second son, Christopher Robert Hulse, was born (March 17, 1938).

Both Hooton and Kroeber as well as Gunther tried assiduously to find Hulse an academic position. It was probably Hooton's urging the Works Progress Administration in Georgia to hire Hulse that finally turned the trick. That one of Hooton's doctoral students, Arthur R. Kelly, had been involved with federal archeology projects in Georgia since 1933 and had just been made chief archeologist of the National Park Service[7] probably didn't hurt. In any event, in late 1938 Hulse joined the team excavating the Irene Mound site about 5 miles outside Savannah and was principally responsible for the analysis and description of the human skeletal remains uncovered. After two years in Savannah Hulse moved to Atlanta with a promotion to state supervisor of various Work Projects Administration projects. He remained in that position until World War II intervened.

From 1942 until 1945 Hulse served in the Office of Strategic Services, which used his Japanese expertise. In October 1945 he became a member of the Civilian Morale Division of the U.S. Strategic Bombing Survey. The team spent four months in Japan collecting information on morale and attitudes during and after the war by means of polling a random sample of 3,000 Japanese, interviews with selected individuals, and review of available documentary evidence. Analysis took the next five months. Hulse made the most of this opportunity to utilize and expand on his Japanese anthropology. A half dozen articles published between 1946 and 1948 were exclusively cultural anthropology, with titles like "Status and function as factors in the structure of organizations among the Japanese" and "Convention and reality in Japanese culture." [8] Quite a leap for a physical anthropologist. The articles were read and used in their day, and were thoughtful and balanced, in no way marred by racism or other manifestations of wartime feelings as was some—not most—social science writing at that time.[9]

THE LATER CAREER

Employment opportunities widened for Hulse in the early postwar period. The most solid of these offers were from the University of Arizona and Colgate University. Although he was supported for both by his mentors Hooton and Tozzer at Harvard, they recommended Arizona; ironically, he chose Colgate. In 1948, after two years as an assistant professor at Colgate, Hulse accepted an offer to return to the University of Washington in Seattle, where he was promoted to associate professor in 1949. He found that state law targeted a portion of liquor-licensing fees for medical research, and he was able to tap this source for funding to collect blood samples from several thousand Native Americans for genetic studies. It also supported his later genetic research among the Italian Swiss in California, a follow-up of his examination of Swiss from the Canton Ticino during his first sabbatical in 1952-53.

A promotion to full professor was among the enticements that lured Hulse to the University of Arizona in 1958, and there he remained until his retirement. Although he had worked with a doctoral student earlier, it was at Arizona that his success in graduate training became evident. He maintained that during his time at Arizona he raised the number of physical anthropology faculty from one to eight, but particularly for the early years, his Ph.D.s in that field were the lion's share. Surprisingly, given his own keen interest in anthropological genetics but only disinterested competence in human osteology, eight of Hulse's nine Arizona Ph.D.s became forensic anthropologists, an applied subfield in physical anthropology that stresses osteology. Only one followed Hulse's own academic leanings. In fact, at the time of his death, Hulse was second in the country to William M. Bass in actually producing forensic anthropologists,[10] and influenced others in that direction. Why this was is not clear. Although, when feasible, he would attend meetings of the American Academy of Forensic Sciences after its physical anthropology section was established in 1972 and he helped frame the bylaws and qualifications of the American Board of Forensic Anthropology (a professional certifying body) some five years later, he belonged to neither. It may have been a combination of his informal but infectious interest in forensic anthropology, Arizona having a very large human skeletal collection, and the prior baccalaureate or master's level tutelage of some of his doctoral students.

Hulse was warmly regarded by his graduate students. They called him "Pappy," particularly the later ones, and enjoyed his hospitality at Friday afternoon seminars at his Ridge Road home in Tucson, complete with front yard desert fauna, backyard swimming pool, wicked El Presidente cocktails, and exotic foodstuff. He could be frugal: for a new Ph.D. celebration party he gave a student ten dollars with orders to buy one bottle of scotch and bring back the change. He was modest: at the 1977 gathering of the American Association of Physical Anthropologists in Seattle, where he was feted with a day-long symposium in his honor, he stayed in the hall outside the meeting room while his accomplishments were being extolled by the lead-off speaker. And he was demanding of but confident in his students, expecting them to carry out lecturing and editorial tasks during his sabbatical absences.

A few years after his retirement Hulse and his wife finally accepted what he considered an outlandish sum for their Tucson home from a real estate developer and prepared to move into a new home they had purchased on Crest Road in Pebble Beach, California, adjacent to the famous 17-Mile Drive between Monterey and Carmel.

As they made their departure from Tucson in March 1982 with Leonie driving, some 100 miles west of Tucson, not far from Gila Bend, their vehicle left Interstate 8 and rolled over twice. Leonie died at the scene of the accident and Hulse was hospitalized for a time. He had to take up residence at Pebble Beach without the wife who, as one of his students wrote, had been "wonderfully warm, supportive, and understanding; an exceptional companion, [providing him] with artistic, clerical, and secretarial support for virtually his entire career." [11] Hulse continued his research and writing, but although emphysema progressively slowed him down, he attended the San Diego meeting of the American Association of Physical Anthropologists in 1989, a year before he died of cancer.

ACHIEVEMENTS AND RECOGNITION

Hulse's emergence as an intellectual leader in anthropological genetics in the postwar period was not entirely unforeshadowed by his prewar training and research. He was, he believed, the first anthropology doctoral student that Hooton insisted take courses in genetics, and his examinations of Japanese in Hawaii, California, and Japan inculcated in him the integrated role of culture and biology in the study of human evolution. His first foray into population genetic research, however, was with Native Americans in the Northwest. His felicitous term, ripples on a gene pool, reflected his demonstration during his years at the University of Washington of the various cultural influences that had altered, and continue to alter, the genetic composition of Native American groups. His elegant study of the people of the Swiss canton of Ticino, in which he compared offsprings' stature in outgroup (between villages) and ingroup (within village) marriages both in the canton and among the Ticinesi who migrated to California (based on their village origin back in Switzerland), demonstrated a genetic effect independent from changes due to environment and nutrition. In later years he sought and found similar culture-mediated explanations for genetic-based variations, such as in skin color and sexual dimorphism, between related populations in Japan, England, and elsewhere.

Hulse's importance as a scholar and teacher went beyond his innovative research to incorporate the authority and elegance with which he expressed his ideas and conclusions. His compassion for his fellow humans and his ability to convey it in scientific papers that used hard words ("ethnic, caste, and genetic miscegenation") and in his popular undergraduate textbook, *The Human Species,* joined as it was with the clarity and sensibleness of his explanations of hu man physical differences, led to an influence wider than the anthropological community and more extensive than the number of his publications might suggest.

Throughout his later career the focus of Hulse's professional activities was the American Association of Physical Anthropologists (AAPA), an organization that was founded in 1930 and continues to be the largest of its kind. Hulse was chosen to be the managing editor of its flagship publication, *American Journal of Physical Anthropology,* for a six-year term (1964-69). During his editorship the journal increased in size by more than 50%. Hulse was also, while he was editor of that journal, elected to a two year term as president of the AAPA (1967-69). The AAPA's 1977 annual meeting was dedicated to him for his many contributions and, in addition to the full-day symposium arranged by his students in his honor, he was the guest speaker at the annual luncheon, inquiring "A rose by any other name smells as sweet, or does it?"

In 1968, as a vice-president, he was the highest-ranking American official of the Eighth International Congress of Anthropological and Ethnological Sciences, held in Tokyo and Kyoto, an honor bestowed no doubt in part because of his long-term interest in Japanese anthropology. He made clear, however, that among all the recognitions of his achievements he felt most honored by his election to the National Academy of Sciences in 1974.

In December 1988, ten years after he became professor emeritus, the University of Arizona provided a capstone to Hulse's distinguished career. It conferred on him the degree of doctor of laws, *honoris causa.* The citation read, in part, "Son of Harvard and adopted son of The University of Arizona, for 20 years you devoted your energies to the Department of Anthropology in this institution. In so doing, you contributed in major fashion to its national and inter national reputation." True enough, but only one of many legacies of this humane and farsighted scholar.

THIS MEMOIR IS BASED ON information in Hulse's autobiographical statement in the archives of the National Academy of Sciences, materials contained in the archives of Harvard University's Peabody Museum, an interview **I** had at Hulse's Pebble Beach home in August 1986, my own reading of his research, and the generous assistance of his son Chris and many others, including K. A. Bennett, W. H. Birkby, H. K. Bleibtreu, T. Burke, S. M. Garn, R. Heglar, K. A. R. Kennedy, C. F. Merbs, F. P. Saul, D. R. Swindler, R. H. Thompson, C. G. Turner II, and C. W. Wienker.

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Leon Orris Jacobson

LEON ORRIS JACOBSON

December 16, 1911-September 20, 1992

BY EUGENE GOLDWASSER

IN BIOLOGY AND MEDICINE there are occasionally singular personalities, people who affect the lives of students, colleagues, and patients to a memorable extent. One such singular person was Leon O. Jacobson, M.D., who combined clinical practice, teaching, academic administration, and innovative investigation to make an indelible impact on hematology and on all who knew him.

Leon Orris Jacobson was born on December 16, 1911, in Sims, North Dakota, a town that exists today only in memories because the Northern Pacific railroad changed its route leaving Sims an abandoned village which now doesn't even appear on maps. He died of complications of lung cancer in Chicago on September 20, 1992, after a rich scientific and personal life. His first wife Elizabeth died in 1983, and he is survived by his second wife Elise, his son Eric, his daughter Judith Bonacker, and their children. Dr. Jacobson was known to his friends and colleagues as Jake and it is fitting to refer to him that way in this memoir.

EARLY LIFE

Jake's family was made up of Norwegian immigrants, and he frequently and proudly showed his ability to speak Nor wegian and to sing Norwegian folk songs while accompanying himself on the piano. His early intention was to study agriculture and follow the family tradition of ranching in North Dakota, but, due to economic necessity during the depression, his education had to be interrupted during his second year at North Dakota State University. He then taught eight grades of elementary school in Sims for the next three years. During this period of teaching a number of grades in a one-room schoolhouse and observing the children getting a variety of illnesses he became interested in medicine and decided to forego ranching. He returned to college and eventually received a B.A. from North Dakota State University in 1935. To embark on a career in medicine he chose to apply to only one medical school, the University of Chicago.

POSTGRADUATE EDUCATION AND PROFESSIONAL CAREER

Jake completed medical school at the University of Chicago in the canonical four years and proceeded to spend the rest of his professional life at that university, never leaving even for a sabbatical. He was both an intern and resident at Chicago and became an instructor in the Department of Medicine in 1942. He was an assistant professor of medicine from 1945 to 1948 and associate professor from 1948 to 1951.

Although Jake professed not to enjoy administrative work, his intense loyalty to the institution did not permit him to decline such positions. He served as associate dean of the Division of Biological Sciences and the School of Medicine at the University of Chicago from 1945 to 1951.

World War II saw the establishment at the University of Chicago of war-related research and Jake was involved in two secret projects: the Toxicity Laboratory, where he served as consulting physician working with chemical warfare agents and protection against them, and the Metallurgical Laboratory. The latter was the local code name for the nationwide Manhattan Project, work that resulted in the first chain-reacting atomic pile, which was designed and built by Enrico Fermi and his colleagues in a squash court under the stands of Stagg Field. In short order Jake became associate director and then director of the Biology and Medicine Branch of the Metallurgical Laboratory.

In 1951 Jake was appointed professor of medicine, head of the hematology service, and director of the Argonne Cancer Research Hospital. This latter was a direct postwar outgrowth of the Metallurgy Laboratory. It was established on the University of Chicago campus, was completely funded by the Atomic Energy Commission, and had a loose connection with Argonne National Laboratory some 30 miles away. The establishment of Argonne Cancer Research Hospital was due largely to Jake's efforts. He, in essence, designed the hospital as a research institute which included research beds and ample laboratory space. It was devoted to the use of the products of the atomic era in research on cancer-related problems and therapies. He staffed the hospital with members of the various departments in the Division of the Biological Sciences and included investigators in fields ranging from protein chemistry, steroid biochemistry, and experimental and clinical hematology to the use of high-energy radiation (an intense ^{60}Co source, a Van de Graff generator, and a linear accelerator) for cancer therapy. The staff consisted of physicians, surgeons, chemists, biochemists, physicists, and radiologists. The synergy resulting from this mixture of disciplines and the complete freedom to follow interesting phenomena led to many important advances in these fields and made the Argonne Cancer Research Hospital known throughout the world.

Jake continued to rise through the academic ranks, be coming chairman of the Department of Medicine in 1961, the Joseph Regenstein professor in 1965, and dean of the Division of the Biological Sciences and Pritzker School of Medicine in 1966, a position he held until 1975. During this period he also returned to direct, for a short time, the Franklin McLean Institute, which was the Argonne Cancer Research Hospital under a new name. He became professor emeritus in 1976 but continued an active interest in research and in taking care of patients. He did not actually retire from the faculty until 1981 and even then remained very active in a number of extra-university activities until his death. He also devoted time to making wild-fruit jellies and to sculpting hardwoods from trees on his country place after chemically curing the wood.

His activities included a number of national boards, most especially those connected with the National Academy of Sciences and the Institute of Medicine. He was past chairman of the Section of Medical Sciences as well as chairman of the Committee on the Study of Postdoctoral Fellowship and Traineeship Programs in the Biomedical Sciences, chairman of the Committee on Science Policy for Medicine and Health. He also served on the Committee on Cancer Diagnosis and Therapy. In addition he was a member of the Committee for Radiation Studies (USPHS) and the Advisory Committee on Isotope Distribution (USAEC) and was the U.S. representative at the First and Second International Conferences on Peaceful Uses of Atomic Energy. Jake also was a member of thirty-seven other committees and boards.

HONORS

Jake was honored many times by many institutions; a partial list follows:

- Election to the National Academy of Sciences (1965), the American Academy of Arts and Sciences (1967), and the Institute of Medicine (1970) ;
- Election as a master of the American College of Physicians (1968);
- Recipient of the Janeway Medal of the American Radium Society (1953), de Villiers Award of the Leukemia Society (1956), American Nuclear Society Award (1963), Phillips Award of the American College of Physicians (1975), Theodore Roosevelt Rough Riders Award and Hall of Fame from the State of North Dakota (1976), Gold Medal of Merit and Knight of the First Order from King Olaf V of Norway, and the Borden Award of the AAMC;
- Honorary Sc.D. degrees from North Dakota State University (1966) and Acadia University, Nova Scotia (1972); and
- Election as a laureate of the Lincoln Academy of Illinois and recipient of its Order of Lincoln in Medicine and Health (1979).

RESEARCH ON THE START OF CHEMOTHERAPY

Jake's wartime association with the Toxicity Laboratory led him to the study of nitrogen mustards and their effects on hemopoiesis. In collaboration with Clarence Lushbaugh he studied dose effects of these toxic compounds in causing severe decrease in white cell number of experimental animals. In 1943, based on these experimental studies, he started to study the clinical efficacy of one nitrogen mustard [methyl-bis (ß-chloroethyl) amine hydrochloride] as an anticancer agent. The first patient treated had lymphatic leukemia and had not responded to any of the then therapeutic regimens. Treatment with nitrogen mustard did cause a partial remission although the side effects were very severe. A quotation from Jake's article "From Atom to Eve" gives a sense of what it was like to embark on such a bold adventure in an unprecedented therapy of a devastating disease:

> It may be difficult for many to understand the deep concern one has when one is giving an extremely toxic but potentially therapeutically effective chemical to a patient for the first time. True, one has the advantage, in a deliberately planned human experiment such as this, that the dose is controlled or calculated from experience with animals and from knowledge of all the specific organ and systemic effects of a wide variety of dose schedules. Human beings generally, but not always, respond to a drug or to a toxic substance in a way similar to animals. Therefore the first trial is inevitably a time of great concern. Obviously, to proceed with this clinical trial, we had to obtain the permission of Dr. George Dick, chief of medicine, as well as of Franklin McLean, the director of the Toxicity Laboratory. Dick was experienced as a clinical investigator, and his cautious supportive role in the venture cannot be overemphasized. The participation of Dr. Charles Spurr and Dr. Taylor Smith as part of the clinical research team was essential. Lushbaugh, with his vast biological and pathological experience with the nitrogen mustard gases in general, and with the particular one we employed (methyl-bis), was a constant observer and advisor and, in fact, must be credited not only with the idea to proceed but with invaluable suggestions on dose schedules and possible toxic manifestations of the drug.
>
> After I gave the injection, I remained with the patient for 24 hours. Within 15 minutes the patient became extremely nauseated and for several hours had severe vomiting; but about 8 hours after the injection, he was able to drink water, although he had no appetite. All vital signs were normal and remained so. Two and 4 days after the first injection, the same dose was repeated. Each time severe nausea and vomiting followed. But the high blood count came down, and the leukemia-infiltrated lymph nodes and spleen became smaller. The patient definitely had a remission.

At about this time investigators at Yale were also using these compounds for the same purpose and the studies at both the University of Chicago and Yale University initiated the present era of cancer chemotherapy. Soon after the patient was treated Jake used nitrogen mustard to treat a second patient, with Hodgkin's disease, who showed a lasting remission. These results were so promising that an extensive series of clinical trials were undertaken. Wartime restriction prevented publication of the results of these trials and by the time the censorship was lifted the Chicago group had treated close to fifty patients with long-term remissions in about two-thirds of them. Treatment of leukemias remained a lasting interest of Jake.

In addition to nitrogen mustards Jake explored the treatment of several neoplasias of the blood-forming system with radioisotopes with indications of success, especially with ^{32}P.

RESEARCH ON RECOVERY FROM RADIATION INJURY

Another outgrowth of Jake's wartime work derived from the Manhattan Project (Jake wrote an engaging account titled "From Atom to Even" in 1981 of how he became involved in that project and what ensued). He had done an extensive study of the effects of ionizing radiation on hemopoiesis and found that the blood-forming system was among the most sensitive to radiation. In an attempt to determine the relative importance of spleen versus bone marrow in overall hemopoiesis he subjected mice to an otherwise lethal dose of X rays to destroy the bone marrow while the spleen was exteriorized and shielded from the radiation. Surprisingly the mice survived the radiation. This was true even when the shielded spleen was removed a few minutes after the radiation or when spleen cells were injected into nonshielded, radiated mice. A large number of experiments led him to the tentative conclusion that a blood-borne, humoral factor was responsible for the recovery from severe radiation injury. The conclusion that the effect was due to a humoral factor was largely based on Jake's observations that heterologous spleen cells (e.g., rabbit) were as effective as isologous cells in promoting the survival of the irradiated mice. Since he expected that an immune response to heterologous cells would have eliminated them from the irradiated host mouse, it seemed reasonable to infer that the effect was due to a noncellular substance that was not immediately inactivated by antibodies. At the time these experiments were done it was not widely known that ionizing radiation severely damaged the immune system, so that antibodies to foreign cells would not be made. Eventually the humoral hypothesis was shown to be wrong; the effect was found to be due to repopulation of the blood-forming system of the radiated animal with either cells coming from the shielded spleen or exogenous cells. Even if the first interpretation was wrong these experiments led directly to the concept of hemopoietic stem cell repopulation of radiated hosts and to the whole field of bone marrow transplantation as it is now practiced therapeutically.

RESEARCH ON ERYTHROPOIETIN AND ERYTHROPOIESIS

In the midst of these experiments while Jake was carrying out his clinical duties (he always considered this his most important work), his administrative duties as head of hematology and chair of medicine, and while being an inspiring teacher of medical students and administering a research institute, he made time to embark on a new field of studies in hemopoiesis. In collaboration with two medical students and the writer of this memoir he started to study the role of the substance termed erythropoietin in the regulation of red cell formation. Evidence for the possible existence of this substance and its nature was, at best, sparse. His intuition, guidance, and unstinting support led to major advances in the study of erythropoietin including the findings that it was made in the kidney and that its production was regulated by the need for oxygen relative to the availability of hemoglobin. Jake's leadership and initia tive in erythropoietin research led eventually to the purification of human erythropoietin, its later cloning and expression in "commercial" quantities, and its present use in the therapy of anemia of chronic renal disease.

My own interaction with Jake during many years of joint research, especially on erythropoietin, was characterized by an interesting duality in approach. Jake's vast knowledge of biology, derived from his deep study of medicine, permitted him to use an intuitive approach to research. He "knew" aspects of biology that he could not explain, especially to a literal biochemist who felt more comfortable with data. Surprisingly, we collaborated easily—without friction—and complemented each other's approach. In addition, his support both intellectually and with research funds was unstinting. This was especially important when the work went slowly, when we had to develop a large-scale source of erythropoietin and new assay methods, and when publications were not numerous.

Those of us who worked on the second floor of Argonne hospital will always associate Jake with noontime music. His two longtime technicians, Edna Marks and Evelyn Gaston, spent their lunch hours practicing flute and recorder, filling the hall and Jake's office with music.

Despite his great achievements Jake remained a humorous, compassionate, and generous person who took great pleasure in being the catalyst for other people's successes in science. His devotion to his patients was legendary and his influence on clinical and experimental hematology will be lasting, especially through the continuing contributions being made by the large number of eminent hematologists and other scientists who trained with him or who had the great good fortune to be part of his staff in the Argonne Cancer Research Hospital.

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Warren K. Lewis

WARREN KENDALL LEWIS

August 21, 1882-March 9, 1975

BY HOYT C. HOTTEL

WARREN KENDALL LEWIS, through his coordination of chemistry, physics, and engineering into an independent discipline to serve the chemical industry, has been called the father of modern chemical engineering. Although his contributions to basic chemical engineering principles and to chemical processing during a life of ninety-two years were many and solid, his hallmark was intense stimulation of hard thinking in others—his students, research associates, and industrial contacts. His applied chemical research and books were important, but aggressive teaching and demand of straight thinking were Lewis's characteristics most remembered by his associates of two generations. "Doc" would bring to the solution of a problem, whether educational or industrial, a sound, well-organized knowledge of physics and physical chemistry. His capacity for expression was superb and his dedication to the objective of finding the answer was obvious and intense. In any discussion, whether on science or engineering or social problems, he loved to lecture and to question.

Born on a farm in Laurel, Delaware, on August 21, 1882, Lewis transferred in his high school days to Newton, Massachusetts, for better schooling. There he met Richard C.

Tolman, later to become as able and original in his area, statistical mechanics, as Lewis would become in his; friendship and mutual respect developed. (In London during World War II Dr. Tolman spoke to me with warmth and admiration about Lewis.) In 1901 Lewis entered the Massachusetts Institute of Technology, intending later to improve farming with his engineering background. His association with William H. Walker, head of the chemical engineering option in MIT's Chemistry Department, changed his objective. He was awarded a fellowship for study in physical chemistry in Breslau, Germany. On the award of his Sc.D. degree in 1908, he returned to MIT for a year as a research associate in applied chemistry; this was followed by a year as a chemist for a tannery in New Hampshire. Then, in 1910, Lewis accepted an MIT appointment as assistant professor. His son, H. Clay Lewis, tells the story of a conversation he had years later with his father's former employer: "I told Lewis I would double his salary if he stayed in leather. He said, 'No'. I then said I would triple his salary. He said 'No'. I then said to him, 'I suppose there is no amount I can offer that will keep you from going to MIT'. Doc said, 'I guess so!'" In 1914 Lewis became full professor under Walker, and in 1920, when the Chemistry Department's engineering option of thirty-two years' standing was split off, Lewis was made head of the new Department of Chemical Engineering at MIT. After thirteen years as head he resigned in order to have more time for his teaching and research. In 1948 he became professor emeritus.

BOOKS

In the period before 1920 Lewis recognized the need for a more unifying philosophy of education in chemical engineering. Stimulated by Arthur D. Little, he worked with Walker and William H. McAdams in identifying and quanti fying what they called the "unit operations" of the chemical industry—distillation, vaporization, separation processes, heat transfer, combustion, absorption, fluid flow, filtration, and so on. In 1923 that effort produced the classic *Principles of Chemical Engineering* by Walker, Lewis, and McAdams (Edwin R. Gilliland was coauthor of the third edition in 1937). The book powerfully stimulated the evolution of chemical engineering as a profession, and it encouraged the creation of new chemical engineering departments worldwide. Parallel to that effort was the publication by Lewis of many papers on unit operations. How more effectively to use material and energy balances on a single chemical species was the motivation for Lewis's next book, *Industrial Stoichiometry* (1926), written jointly with A. H. Radasch; in 1954 the book was expanded, with co-authorship by H. Clay Lewis. Lewis's early work on leather tanning and on the vulcanization of rubber got him interested in colloidal phenomena, and his later research on clay, textiles, and plastics expanded that interest and produced a book, written jointly with L. Squires and G. Broughton, *The Industrial Chemistry of Colloidal and Amorphous Materials* (1942).

SOME OF LEWIS'S RESEARCH ACTIVITIES

So many areas of applied research interested Lewis that it is difficult to guess which interested him most; distillation was certainly high on his list. He had early become a consultant on petroleum problems and was soon aware of the higher level of sophistication on distillation in the alcohol industry than in petroleum. Existing patents on separating petroleum fractions showed a gross deficiency in the basic physics and physical chemistry of separation, and Lewis typically dedicated himself to putting fractionation by rectification on a sound basis. One story of events in that area in the 1920s is typical Lewis: The department's Applied

Research Laboratory had contracted to expert a lawsuit involving fire in piping carrying oxygen, and had got Lewis to agree to be an expert witness. Studying the problem, he found that a side issue unimportant to the subject case presented a distillation problem he could not solve. He buried himself in the problem, and on the train to where the case was to be tried he was so lost in his new problem that the laboratory staff began to worry, and assigned to one of its younger members the full-time job of keeping Doc from working on his distillation problem rather than studying the coming court case. Lewis published thirteen papers on distillation and nine on evaporation; nineteen of his eighty-one patents were on distillation.

The movement of underground oil to a well's borehole excited Lewis's imagination, and he made valuable contributions to the modeling of flow through oil sands, the prediction of oilfield life, and methods of increasing oil recovery. But his most important contribution to the petroleum industry, measured either in dollars or in military value, came out of his interest in fluidized powders and the control of their movement in a chamber. Lewis in his early days in petroleum had contributed to thermal cracking as a means of increasing the fraction of crude petroleum that ended up in the volatility range of gasoline. Then came catalytic cracking, which enormously increased the gasoline yield and its antiknock quality but was plagued with troublesome problems such as control of temperature of the massive bed of pellets on the surface of which the chemical reaction generates heat and loss of catalytic activity due to the deposition of carbon on the pellet surface. Lewis saw that preparing the catalyst in fine-particle form and suspending it in the petroleum vapors to be cracked could solve the problems. In a continuous process he saw that the catalyst could be partially removed at one point, cleaned by oxida tion, and returned at another point, and that the temperature could be far more readily controlled. The first full-scale unit began operation in 1942; fluid-bed cat cracking produced high-octane aviation fuel, giving Allied planes greater speed than Axis models with engines designed for lower-octane fuel. Substantially all cracking today uses the fluid bed, representing a multibillion dollar investment. Many other chemical reactions are today carried out in fluid-bed systems.

WARTIME ACTIVITIES

Lewis's high ethical standards and deeply religious temperament did not prevent his large effort in both world wars. In World War I, first in the Bureau of Mines and later in the Chemical Warfare Service, he was in charge of research on gas defense, correlating and directing the work of various laboratories and aiding in the reduction to practice of the results in the manufacture of protective devices by the Gas Defense Production Division. In World War II Lewis served as executive officer of the Chemical Engineering Department's many military research activities. He was also a consultant to the federal Office of Scientific Research and Development and an advisor to the Office of Production Research and Development. In April 1940 Arthur H. Compton was asked by Vannevar Bush, director of the wartime Office of Scientific Research and Development, and the National Academy of Sciences to chair a committee to assess the military value of uranium. Two academy reports and indecisive discussion of the consequence caused Bush and Conant in the autumn of 1941 to enlarge the Compton committee by two members, "this time to include W. K. Lewis, a chemical engineer with an outstanding reputation for estimating the potential success at industrial scale of laboratory processes"[1] and George Kistiakowski, a Harvard chemist and expert on explosives. By early spring of 1943 Los Alamos physicists were busy laying plans for their laboratory work, and "(General) Groves appointed a review committee—W.K. Lewis again, an engineer named E. L. Rose, who was thoroughly experienced in ordnance design, van Vleck, Tolman, and one other expert—to follow planning and to advise."[1] In January 1943 Philip Adelson and Ross Gunn had proposed thermal-diffusion as a partial enricher of uranium, and a year later construction finally began on a small plant. By the spring of 1944 Oppenheimer had become aware of the effectiveness of thermal-diffusion as a complement to gaseous diffusion, had convinced Groves, and was contrite for having lost time. In May "Groves appointed a committee of men thoroughly experienced by now in Manhattan District troubleshooting: W. K. Lewis, Eger Murphree" (an expert on thermal diffusion who had served at MIT under Lewis) "and Richard Tolman."[1] Clearly, in both wars Lewis's engineering know-how and decision-making ability were highly prized by many organizations.

HIS AGGRESSIVE TEACHING

Lewis's contributions to the chemical industry loom large but they pale in comparison with his Herculean capacity to teach, whether students, faculty, or industrial associates. His teaching methods were phenomenal, varying greatly but always stimulating and intense, sometimes dramatic, and not infrequently showing a talent for acting. But they drove home Doc's demand for clear analysis, his impatience with sloppy thinking, and, if a problem involved industrial application, the importance of action. Sometimes Doc would come into the classroom, take off his coat, roll it into a bundle, deposit it, and turn to a student with a riveting glare and ask a broad question. To a poor answer he might reply, "You damned dumb-bell, don't you see that," and so on, sometimes with a little sermon. Among countless stories told about Doc's teaching, here is one told a generation after it happened by the last recipient in a class period of Doc's succession of put-downs of student answers to his questions, shouted from a raised platform in a large classroom: "Can anyone here name a single infallible law of Nature?" Silence, then a pointed finger, "You there, first man in the first row! Can't you name one?" Pause. Then, "Conservation of matter." Doc, "Cosmic rays blow your law of conservation to Kingdom Come. Next man!" Finally the pointing finger got to the teller of this story, who blurted out, "The law of constant proportion." Doc, "Did you ever hear of isotopes?" Then he leaned down and forward until his face was within 12 inches of the student's and shouted with such intensity that his mouth was not under proper control, "Isotopes are things that *spit* at the law of constant proportion." In telling the story the teller did not forgive; instead, he thanked Doc for a lesson never forgotten. Some comments from Walter G. Whitman, who followed Lewis as chemical engineering department head at MIT: "There was nothing more important (to Dr. Lewis) than kindling the spark for accomplishment. . . . (His) methods have seemed unorthodox and even harsh to many on first acquaintance. . . . But as the student learns to meet that challenge to his intelligence and imagination, he acquires unsuspected powers and confidence. He also learns that Doc can become a patient guide . . . ." One more story: When a lecturer of students gets badly mixed on a quantitative derivation, the gracious and student-time saving action is to admit being mixed, promise a straight derivation next session, and go on with the lecture. Not for Doc; he feared an apology would be misinterpreted as indicating being wrong rather than just temporarily being confused. Doc's teaching assistant, Robert L. Hershey, later a faculty member and superb teacher and still later a du Pont vice-president, was sitting in the back row of a large classroom where Doc, at the blackboard on a thermodynamics derivation, got going in a circle. Hershey left through the rear door, entered the front one, and handed Doc a paper as he commented, "A telegram for you, Dr. Lewis." Doc glanced at the paper, pocketed it, and went on, with his derivation clarified. Doc's use of strong statements to make a point often got him into an argument with students or faculty associates. Characteristically, he would say, "I'll bet you a dollar to a doughnut" (five cents in the early 1920s). R. E. Wilson, one of Doc's faculty associates and later president of Standard Oil Company of Indiana and still later Atomic Energy Commissioner, would set scientific traps for Doc and sometimes win. Every spring he would send Doc a copy of a page out of his IRS return in which he reported "$xx won from W. K. Lewis on dollar-to-doughnut bets." Sixty years after the event causing its transfer, an old-fashioned dollar bill, larger than the present one, was received by MIT from a surviving relative of a student who had won a dollar-to-doughnut bet from Lewis and carried it in his wallet throughout his life.

CONSULTING

Lewis's teaching ability was a main factor in his consulting contacts with industrial researchers or planners. Harold C. Weber, a faculty associate of Doc, told a story of their joint visit to National Carbide Company, where they discussed several problems related to carbon, including B batteries. Lewis dominated the discussion. As they left the conference, Doc turned to Harold and said, "Weber, what the hell is a B battery?" Weber later said that Doc could stretch a small bit of factual knowledge about a problem farther than anyone he knew. That was illustrated in a 1928 conference at Humble Oil Company which I had the honor to join. Doc and Robert T. Haslam took me with them to Texas for consulting on several problems in their area and one in mine. At the conference were several young men I knew who had studied under Doc and were now on Humble's research staff. I expected the chairman, Mr. Wise, general manager and later on the board of Standard Oil Company of New Jersey, the holding company for Humble and other subsidiaries of Standard Oil, to dominate the meeting. He outlined the first problem, and Doc immediately made some comments that got a lively discussion going. Mr. Wise presented the second problem area, and Doc at once spoke up and said what the problem was about. One of his former students immediately replied, "Doc, that isn't it. The problem centers on (so-and-so)." Mr. Wise encouraged the younger man, and discussion went well. On the third problem Doc again started the discussion, and again one of the younger men disagreed. The whole conference went that way, with Doc occasionally supplying inputs but always supplying stimuli. With the fairly prevalent youngster's misconception of how big corporations become big I had expected Mr. Wise to dominate the conversation. Instead, he stayed in the background. It was also clear that the Humble staff knew far more about what inputs were important to the discussion than Doc, and even clearer that by any standards Doc was the most valuable man at the conference.

THE ENGINEER IN SOCIETY

Taking a strong position on the problems of society was characteristic of Lewis. His belief in the important role the engineer had played over the centuries in raising the living standards of humanity was expressed frequently; he saw engineering as a noble activity. Such a discussion might lead to his almost religious fervor in defending the principle of a free competitive economy, or profit as the measure of industrial success. He could become eloquent on the validity of that measure, pointing out that the principle of free competition combined with profit to measure success promoted individual contributions to the common welfare at minimum cost. Then he would add, "I see no viable alternative to the profit system; I have nothing but contempt for the profit motive." Lewis was devout, high principled, a deacon in his church, but not outwardly religious. He never hesitated to mix his beliefs about ethical principles or free industrial competition with his teaching of engineering. He believed the engineer, by not taking more of a lead in society's problems, was overlooking an opportunity to render a public service greater than any he had contributed in the past.

At the end of World War II, MIT appointed a Committee on Educational Survey. With Lewis as chairman and Jay Stratton one of the members the committee made a long-range study of the MIT's curriculum and educational policy. One of the significant results was the establishment of the School of Humanities and Social Sciences, a deficiency about which Lewis had commented for decades and had the support of Stratton. His legendary ability for clear and forceful report writing was one of the reasons for the committee's success in guiding the evolution of MIT during the critical years after World War II and for the next two decades. In the nearly fifty years since the committee's formation two similar ones have written reports which refer to the principles enunciated by the Lewis committee.

Warren K. Lewis was elected to the National Academy of Sciences in 1938. He was the recipient of the many honors listed below; but he is chiefly honored by the vivid memory of his personality and principles in the minds of engineers and others who had the privilege of knowing him.

AWARDS AND HONORS

Perkin Medal of the Society of Chemical Industry (British), 1936
Member, American Academy of Arts and Sciences
Member, National Academy of Sciences, 1938
Lamme Medal of the American Society of Engineering Education, 1947
Priestley Medal of the American Chemical Society, 1947
President's Medal for Merit, 1948
Gold Medal of the American Institute of Chemists, 1949
New England Award of the Engineering Societies of New England, 1950
First American Chemical Society Award in Industrial & Engineering Chemistry, 1956
American Petroleum Institute Gold Medal for Distinguished Achievement, 1957
Founders Award, American Institute of Chemical Engineers, 1958
Member, National Academy of Engineering, 1965
John Fritz Medal of 5 Engineering Societies, 1966
Honorary Member, Institute of Chemical Engineers (British)
President's Medal of Science
Warren K. Lewis Award of the American Institute of Chemical Engineers
Establishment of the Warren K. Lewis Professorship in Chemical Engineering at MIT, 1969
Honorary Sc.D. degree, University of Delaware, 1937
Honorary D.Eng. degree, Princeton University, 1947
Honorary Sc.D. degree, Harvard University, 1951
Honorary Sc.D. degree, Bowdoin College, 1952

NOTE

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CHOH HAO LI

April 21, 1913-November 28, 1987

BY R. DAVID COLE

FOR A HALF CENTURY Choh Hao Li studied peptide and protein hormones, especially those of the anterior pituitary gland and for much of that time dominated the biochemistry of the field. He was first, or among the first, to purify and determine the molecular structure of adrenocorticotropin, lutropin, follitropin, growth hormone, lipotropin, prolactin, endorphin, and melanotropin. The biological properties and clinical applications of these hormones were also extensively studied in his laboratory. Li was a pioneer in the synthesis of biologically active peptides and proteins and analogs of the natural species and accomplished the chemical synthesis of melanotropins, corticotropins, endorphins, lipotropin, and growth hormone. Since peptide and protein chemistry was in its infancy when Li began his studies, he also contributed heavily to the development of techniques and methods for protein chemists.

Recognition of Li and his work was extensive abroad, as well as in the United States. Among his ten honorary doctorates were ones from the Catholic University of Chile, the Chinese University of Hong Kong, and the University of Uppsala. Li was an honorary member or fellow in fourteen societies and academies, including ones in Argentina, the

Republic of China, Chile, Israel, and India. He was elected to the American Academy of Arts and Sciences in 1963 and to the National Academy of Sciences in 1973.

An international scope also is evident in Li's twenty-six awards and medals which include the Gold Medal of the City of Milan; the University Medal, Liege; the Science Award, Academia Santa Chiara, Genoa; and the Heyrovsky Gold Plaque of Honor for Achievement in Chemistry, Czechoslovak Academy of Sciences. His major American awards are the Lasker Award, the Lewis Prize of the American Philosophical Society, and the Nichols Medal of the American Chemical Society. In 1947 the Endocrine Society gave Li his first award, the Ciba Award, to acknowledge his outstanding promise as a young researcher and then in 1981 recognized his vast record of accomplishments with the society's senior honor, the Koch Award. Li was particularly gratified by the Scientific Achievement Award of the American Medical Association in 1970 because it marked the far-ranging ramifications of his basic research—throughout medical research and practice.

Even in a brief sketch of the accomplishments that lie behind Li's honors it can be seen that a salient characteristic of his was an alertness to opportunity, even when opportunity was at the very periphery of his field of activity and even when it was risky. This might have made him ineffective by scattering his efforts were it not that once he decided to grasp a new opportunity he creatively wove it into the fabric of previous work. He had the capacity to envision what was actually possible long before most of us saw it. Therefore, Li had a knack for gambling on winners and he backed his bets with unstinting commitment of resources and personal energy.

His resourcefulness and a capacity to concentrate his effort may have been necessary for intellectual survival as

Choh Hao (C. H. to his friends) grew up in Guangzhou, China, with ten siblings, all of whom became successful scholars. One brother, Choh Ming Li, played a key role in determining where C.H.'s career would be lived out. After earning a B.S. degree in chemistry at the University of Nanking in 1933, C. H. stayed on as instructor. He took the opportunity to do research with F. H. Lee, who had just returned from the United States with a newly earned Ph.D. The research completed a project begun by Lee with W. V. Evans at Northwestern University and led to a publication in the *Journal of the American Chemical Society,* authored by the three men. This publication was another factor that determined the place where C. H. would live the rest of his life and develop his career.

His application rejected by the University of California at Berkeley, C. H. was on his way in 1935 to the University of Michigan, where his application for doctoral studies had been accepted. When C. H. stopped in Berkeley to visit his brother, a graduate student in business administration, Choh Ming advised C. H. to show the paper from the *Journal of the American Chemical Society* to Gilbert Lewis, who ruled the College of Chemistry at the university. The college had rejected C. H.'s application because they knew nothing of the University of Nanking, and had not had any graduate students from China. Lewis knew Evans, and was impressed by C. H.'s scientific paper, and admitted him to the doctoral program provisionally. Financing this opportunity was a challenge because there was severe competition for the few jobs available during the depression. Generally in the competition being from China was a disadvantage, but C. H. found an opportunity to turn it to advantage by teaching Chinese at the Chung-Mei Home for boys, about 5 miles from campus. To do this $30-per-month job C. H. invested in a jalopy and learned to drive. He took to the streets after a one-hour driving lesson with characteristic determination and concentration, but with uncharacteristic fear. Fortunately, he did survive and by 1938 had earned a Ph.D. studying chemical kinetics with Thomas Dale Stewart.

Academic and industrial positions were extremely scarce in 1938 for chemists with fresh Ph.D. degrees, and Li felt himself very lucky to secure a position, even though it was an unusual one. It was an opening on the Berkeley campus for a chemist in the Institute for Experimental Biology, headed by Herbert Evans, a professor of anatomy and a preeminent endocrinologist. This was a high risk opportunity because it had almost nothing to do with the kind of chemistry for which C. H. had been trained. The members of the institute used various biological approaches to study endocrinology of the pituitary gland. Evans's vision of a chemical approach to endocrinology was novel, so much so that the nature of such chemistry was unclear at Berkeley and even elsewhere. As foreign as this sort of research was, Li poured himself into it wholeheartedly, grateful for the opportunity. The hormones to be studied were proteins, and protein chemistry was in its infancy. The state of the art was crystallization, centrifugation, and moving-boundary electrophoresis. Amino acid analysis, amino acid sequencing, and X-ray crystallography were only dreams, and chromatography, gel electrophoresis, and circular dichroism were not even dreamt. To make matters more bleak, hormones occurred in tissues in minute amounts and had very crude assays, whereas the little protein chemistry being practiced in 1938 was done with abundant proteins and usually with enzymes that had fairly precise assays. Only one of the pituitary protein hormones, prolactin, seemed to be pure at the time, and it was not even clear how many other pituitary hormones there were. C. H. came to see the potential of a field he might have somewhat to himself if he was lucky, smart, and hardworking. In any case he took the gamble and won because he was lucky, smart, and hardworking.

Although Li collaborated some with another chemist who joined the group, Heinz Fraenkel-Conrat, for the most part during the first decade he worked alone in a basement lab. For the biological aspects of his research he had strong support from the institute's biologists such as Evans, Miriam Simpson, William Lyons, William Reinhardt, Willet Asling, and Leslie Bennett. In the first decade Li published 126 papers, including announcements of the preparation of highly enriched extracts of corticotropin (ACTH), lutropin (LTH or ICSH), and follitropin (FSH), and the purification of somatotropin (STH or growth hormone). He also served on the faculty of the university as lecturer in chemical morphology (1942-44), and as assistant professor (1944-47) and then associate professor (1947-49) of experimental biology.

In 1949, as a new generation of techniques for protein chemistry was aborning, Li took advantage of a Guggenheim fellowship to study peptide chromatography with Arne Tiselius in Uppsala. He arrived in Europe at an opportune moment. In conversation at a conference C. H. heard of promising experiments on the determination of amino acid sequences in insulin, and so he changed his plans to spend a month with Fred Sanger and Rodney Porter at the University of Cambridge. These experiences in Sweden and England set the research approach of Li's lab for years to come.

Returning to Berkeley in 1950, Li established the Hormone Research Laboratory and was appointed its director and professor of biochemistry and experimental endocrinology. The establishment of the Hormone Research Laboratory separate from the Institute of Experimental Biology was instigated by Robert Sproul, president of the University of California, who was always eager to forestall the loss of rising stars to other institutions. The chance to build a large research team was rare in those days and to do so would severely reduce Li's time at the lab bench, as well as turn him to the unfamiliar tasks of administration, political maneuvering, and raising financial support. C. H. was an instant master of those unfamiliar tasks. Initially the university provided all the support for the laboratory, but the need for major outside support arose almost at once. The extensive array of sources for financial support that we know today was not available in 1950, but C. H. was quite effective, and before he retired he had tapped thirty-two public and private sources. Of extraordinary help to C. H. was Mary Lasker, who not only gave generously from her own foundation, but introduced Li to productive contacts in government agencies and to Maxwell Geffen and Charles Allen, Jr., who became major, long-term benefactors of the laboratory. Mrs. Lasker made annual visits to the lab with her scientific advisor. On those occasions, one by one, those of us in the lab joined C. H. in his office with the visitors. I clearly recall nervously taking my turn, chatting over a cup of especially delicious jasmine tea, and then presenting my most exciting recent findings. After being in the lab somewhat longer it dawned on me that this choice tea was not served on any occasion other than Mary Lasker's visit-that jasmine tea was her favorite. C. H. was attentive to detail in all phases of his work.

In 1952 when I joined the Hormone Research Laboratory (HRL) as a graduate student it was already thriving in rooms scattered from the basement to the fourth floor of the Life Sciences Building on the Berkeley campus. The research staff consisted of David Chung, Peter Condliffe, Jonathan Dixon, Irving Geschwind, Ieuan Harris, George Hess, Anthony Levy, Harold Papkoff, Ning Pon, and Jerker

Porath. Through the years many famous researchers visited and many worked for a while in the lab. Li made a point of generously sharing the time of the famous ones with all of us; it was a heady experience for a graduate student. By 1983 when C. H. retired the HRL had moved (in 1967) into contiguous space that C. H. designed at the San Francisco campus of the university, and it had been the training ground for more than 300 visiting scholars, postdoctoral associates, and graduate students. C. H. never gave up experimenting with his own hands, and his unbounded enthusiasm for working at the lab bench infected us all. Li was a good mentor with a style that used modeling and encouraging rather than explicit instructing. Indeed, C. H. was especially effective as an encourager and he consistently took pains to enhance morale in other ways as well. The HRL in San Francisco was even well decorated. When I visited there Li proudly showed me the paintings he had hung in the lab as well as in his office. Although my recollection might be colored by nostalgia for the sweet simplicity of graduate years I recall a lot of laughter and excited discussion; it was fun and hard, driving work. My impression is that C. H. was successful in maintaining an ambience of joy, excitement, and strong friendship within the HRL throughout its history. The family-like affection of the large numbers who were associated with C. H. in the HRL was evident in enthusiastic reunions on the twentieth and thirtieth anniversaries of the founding of the HRL and on the celebration of C. H.'s sixtieth birthday. Many who attended came from the far ends of the world; the HRL was a markedly cosmopolitan (and democratic) community. The HRL had a personality —in many ways the personality of C. H. himself.

The research productivity of Li and his HRL was so vast and multifaceted that severe selectivity is needed to fit a discussion of it into this memoir. Such a selective story could be told from many different angles, but I choose to tell it along lines that C. H. himself used on occasion. This traces the trail from corticotropin to lipotropin to endorphin and it has the advantage of showing the progression in Li's research program over the whole history of the HRL. Moreover, the story illustrates Li's alertness to opportunity.

Although an extract had been greatly enriched in adrenal stimulating activity while C. H. still worked with Herbert Evans, by 1950 C. H. was in a race with three pharmaceutical firms for the complete purification of corticotropin (ACTH). Two of the pharmaceutical firms published the purification of a large active fragment of pig corticotropin (Armour) and intact pig corticotropin (American Cyanamid) before the HRL published its isolation of intact sheep corticotropin in 1953. Actually, pure corticotropin had been obtained by the HRL well before the publications of the pharmaceutical firms, but Li's team delayed publication while they attempted the replication of their initial success. Unfortunately, replication was delayed because the sheep pituitaries used in the first several months of frustrated attempts had been previously stored by the supplier in a warehouse next to one that burned down, evidently allowing the frozen pituitaries to thaw and refreeze. When a new batch of starting material was used the initial results were replicated. While C. H. was disappointed to be late in publishing he took this bad luck stoically. Since C. H.'s competitiveness is commonly recognized, I note to his credit that he never publicized the spoiling of the starting material as an excuse for failing to purify corticotropin before the others.

In 1955 the amino acid sequence of corticotropin was published by Li's lab. C. H. saw in it an opportunity to test two notions that had fascinated him for several years despite their unpopularity with most other protein chemists. One of these notions was derived from the observation that even the purest lab preparations of hormones generally had biological activities overlapping those of other hormones. Although most workers explained this by cross-contamination, C. H. felt that the activities overlapped because portions of the structures were homologous from one hormone to the next. The second notion that fascinated C. H. was that biologically active fragments could be derived from hormones because qualitatively the basis for activity resided in a limited region of the molecule, while other regions modulated the basic activity. Many of us in the lab in the early 1950s, myself included, looked at this notion askance, but this notion and the first one became major themes in the program of the HRL. It can be seen that Li's position was vindicated as a generality for peptide/protein hormones. The first notion was confirmed in 1956 when Ieuan Harris in Cambridge and Li's lab in Berkeley learned the amino acid sequence of the melanocyte-stimulating hormone, melanotropin (MSH). Corticotropin preparations also stimulated melanocytes to some extent and it became evident that the MSH-like activity of ACTH preparations was inherent because the 18-residue sequence of MSH was found within the 39-residue sequence of corticotropin.

The notion that biological activity could exist in a fragment of a hormone had been tested for several years by attempting the isolation of active fragments from enzymic digests of hormones. The results, however, were vulnerable to the criticism that the isolated fragments were contaminated with minor amounts of intact hormone—such was the reliability of peptide separations then. Li saw that a rigorous proof could be obtained by chemical synthesis of peptide fragments. Moreover, he innovatively envisioned that the synthesis of analogs would permit correlations of structure with biological activity that could reveal general principles and that some might be useful in drug design. Work ing out the vision, however, would be a high-risk commitment. Nobody in the HRL knew about peptide synthesis and the best that was done anywhere else produced meager yields of relatively short peptides. Nevertheless, C. H. gambled and took a sabbatical leave in 1957 to learn peptide synthesis with Robert Schwyzer in Basle. In 1958 with Schwyzer's help Li established a synthesis team in the HRL at Berkeley, a team consisting of Eugen Schnabel, Tung-Bin Lo, Johannes Meienhofer, Janakiraman Ramachandran, and David Chung. By 1960 this team had synthesized longer and longer peptides until they made one containing the first 19 residues of corticotropin and were delighted to find that this peptide had adrenal-stimulating and melanocyte-stimulating activity, just as did the intact hormone of 39 residues. Peptides shorter than 19 had MSH activity, but no ACTH activity. Many 19-residue analogs were studied and detailed correlations were made between structure and activity.

Immediately after Bruce Merrifield introduced solid-phase peptide synthesis in 1963 James Blake and Donald Yamashiro put it to work in the HRL. After making some significant modifications in the method they were able to synthesize corticotropin-related peptides and ultimately to accomplish in 1973 the synthesis of the entire hormone. One of the things that was learned as this work progressed was that the addition of residues between 19 and 26 increased the ACTH potency of the peptide progressively up to the natural level. The experience gained in synthesis of ACTH was valuable in subsequent syntheses of other protein hormones—lipotropin, endorphin, and a growth hormone.

Attempting to improve yields of naturally occurring ACTH, Yehudith Birk and C. H. made a minor change in the purification procedure and, among other things, this resulted in the appearance of a new peak in one of their chromatograms. The peptide represented by the new peak was iso lated and found to be chemically different from all the known hormones. Since analogs of ACTH fragments were then being assayed for lipolytic activity the new component was also tested. It proved to be much more potent in lipolysis than corticotropin and so it was named ß-lipotropin (ß-LPH). It lacked ACTH activity but had an MSH activity equivalent to that of corticotropin. Normally the discovery of the activity of a protein precedes its purification and characterization, but Birk and Li, with some combination of luck and intuition, had reversed that order. It will be seen shortly that luck and intuition continued to play a role as research on lipotropin progressed.

When the amino acid sequence of lipotropin was deciphered in 1965 it was clear why ß-LTH had MSH activity. The 18-residue sequence of MSH was contained within the 91 residues of ß-LTH and, therefore, lipotropin can be viewed as a prohormone from which MSH is derived by protein processing. Always alert for active fragments and intrigued with the implications of protein processing, Chrétien and Li studied a side fraction from ß-LTH preparation and in 1967 found a new lipotropin, which they labeled ?-LTH. It consisted of the first 58 residues of ß-LTH and contained the MSH sequence as its carboxyl terminal region. When David Chung and C. H. were searching for LTH in extracts of camel pituitaries they failed to find intact LTH, but they did find a 31-residue peptide that consisted of residues 6191 of ß-LTH. It represented the carboxyl region of ß-LTH and accounted for nearly the whole difference between ß-LTH and ?-LTH. This peptide was named ß-endorphin (ß-EP). Avram Goldstein recognized a 5-residue sequence in ß-LTH that matched Met-enkephalin and wrote to C. H. for related peptides to be assayed for morphine-like activity. Li sent him ß-EP, which C. H. knew had the five amino acids of Met-enkephalin at its amino terminus. The opioid activ ity of endorphin was thus discovered in early 1976. Once again a peptide hormone had been discovered from its chemical properties before it was identified biologically. One conclusion to be drawn from this work is that ß-LTH in addition to being a lipotropic hormone in its own right is a prohormone for not just one, but three hormones, ?-LTH, ß-MSH, and ß-EP. As C. H. typically did following the initial purification of a hormone, he and his colleagues went on to study the biological properties of ß-EP and synthetic analogs of it. In this case the potential for clinical applications to drug addiction and psychiatric problems added extra drive as Li followed up the chemical studies with many biologists and clinicians as collaborators. Undoubtedly, the follow-up work will now be continued by others.

In summary, I told a story that sketches out a lasting legacy that C. H. left to basic science. This legacy includes an understanding of the structural bases for the activities of five pituitary hormones of the anterior and intermediate pituitary. Additionally, Li's elucidation of an unusually rich example of the protein processing of a prohormone, ßlipotropin, was part of the story. Hopefully this story illustrates both Li's genius and the general nature of the stream of consciousness as the research program developed in the HRL.

Unfortunately, the story bypassed parallel research done by C. H. and the HRL on prolactin, lutropin, follitropin, growth hormone, and chorionic gonadotropins. It has not covered the extensive comparisons of amino acid sequences in most of these hormones across a very wide range of animal species, comparisons that were a rich source of data on evolution, and on structure/function relationships. Neither has it even sketched the far-ranging biological studies on all these hormones. The story was told, after all, by a protein chemist under the pressure of severe space limita tion. No doubt one of C. H.'s more biologically or medically oriented colleagues would have written with profoundly different emphases. They might have described a legacy that includes a vast store of information on metabolic and other biological effects of several of the pituitary hormones and on their evolutionary relationships. Li left a body of knowledge which will surely be a foundation that future biology will inevitably build on.

One part of Li's legacy to medical practice must not be passed over-the availability and knowledge of human growth hormone. The production of genetically engineered growth hormone and its widespread clinical use are direct extensions of Li's research program and critically dependent on it. Probably some of the deleterious effects of the overzealous use of growth hormone in the clinic might have been avoided if more physicians had paid more attention to Li's biological studies. The legacy of biological and clinical research by Li includes studies of most of the pituitary hormones, not just growth hormone. Furthermore, the legacy extends far beyond the publications that bear Li's name because he contributed substantially to clinical research by responding to requests for samples of the hormones handled in the HRL. C. H. was well known for his generosity in sharing his preparations without obligation.

Another part of Li's legacy to the clinic must be added even though it is somewhat tangential to the general line of his work. There was another of those side products that Li was alert enough to purify, characterize by amino acid sequence, and synthesize. Perhaps C. H. did it searching for active fragments of growth hormone, but in any case the side product turned out to be a growth factor active in cell cultures, and so it was named insulin-like growth factor-I. The significance of the factor for the public became clear while this memoir was being composed. The newspapers announced that insulin-like growth factor-I is effective in substantially slowing the progress of amyolateral sclerosis (Lou Gehrig's disease), and that the stock of the companies producing it by genetically engineered processes had risen abruptly. Even if the factor does not have further application to other diseases of nerve degeneration, it appears it will be a boon for many.

The legacy C. H. left to the profession of science can be outlined in terms of his service at the policy level. He was on many advisory boards, but special mention must be made of his contributions to the founding of biochemistry programs as he served on the Scientific Advisory Board of the Institute of Biological Chemistry, Academia Sinica, Taiwan, and the Academic Advisory Board of the Chinese University of Hong Kong. The editorial work C. H. did was prodigious, having included membership on numerous editorial boards, advisory boards of journals, and editorships of individual volumes. Two of his most outstanding editorial contributions were as editor of the series *Hormonal Proteins and Peptides* from its beginning in 1973 until his death in 1987 and as one of the executive editors of *Archives of Biochemistry and Biophysics* (1979-87). Perhaps Li's most lasting legacy to scientific publication is in the *International Journal of Peptide and Protein Research,* which he served as co-associate editor from 1969 to 1976 and as editor in chief from 1977 until his death. During his tenure the circulation increased and the journal established itself as a major vehicle for publication in its field.

To those of us who had the privilege of direct contact with C. H. he left a more personal legacy—the memory of his character and style. Although this could be expressed in the context of the laboratory it can also be typified by the social gatherings we used to have at the Li's in Berkeley. As a host C.H. did not dominate the group with his ego as other public stars sometimes do. Instead, he contrived to catalyze amicable interactions among all those present. He was more like a symphony conductor than a concerto performer. He had gracious and quite subtle ways of encouraging participation by those who would otherwise tend to stay on the periphery. Annie's graces complemented his so that these house parties hold a warm place in my memory. C. H. had a carefully acquired American sense of humor that added to our fun, and it was the more charming because we could sometimes detect that it did not come to him naturally. In any case, his kind concern that we all enjoy each other was manifest in the lab and outside and it remains a bond of spirit among those who worked in the Hormone Research Lab at one time or another.

An appreciation of art was conspicuous in C. H., and the Li home showed it. C. H. never missed a chance to brag about the art of his wife Annie, pointing out the art objects that she had created and reminding us that Annie had designed their house. He was pleased to tell how his daughter Eva designed the covers for two books published on anniversaries of the HRL's founding. In addition to his pride in Annie's art C. H. had a profound respect for her judgement. He commonly laid out his writings for her to read, expecting to discuss her reactions over the dinner table. Even though Annie was not trained in science C. H. valued her opinions about the significance of the reported research and particularly about how effectively he had communicated it. Li's twin interests in science and art may well be reflected in the lives of his children. The children were frequently included in lab parties and C. H. was very vocal about his pride in them. His son Dr. Wei-i Li is a surgeon in Bellevue, Washington; his artist daughter, Mrs. Eva Li Hill, lives in Toronto; and his veterinarian daughter, Dr. Ann-si Li, is in Berkeley. His family has made clear their pride in

C. H. by endowing a professorship in his name at the University of California in Berkeley. Professional colleagues have also sought to make clear the honor with which they remember C. H. by establishing memorial lectureships at the university in Berkeley and also at the Academia Sinica and National University of Taiwan.

It seems lame to close with the cliche that Choh Hao Li ought not to be forgotten and certainly won't be, but after all, that is the plain truth.

TO ANNIE LI I am deeply indebted for a wonderful visit during which she spoke from a warm and loving heart of experiences that illuminated her husband's feelings as well as his actions. I am grateful to Professor Howard Bern for his personal reflections, which he gave at the dedication of the Choh Hao Li Professorial Chair in Biochemistry and Molecular Biology at the University of California, Berkeley. Much of the story on corticotropin/lipotropin/endorphin was based on an article written by Li as chapter 10 in *Selected Topics in the History of Biochemisty,: Personal Recollections,* ed. G. Semenza. Amsterdam: Elsevier Science Publishers *(Compr. Biochem.* 35[1983]:333-52). Other information came from two books edited by Li entitled *Hormone Research Laboratory 1950-1970* and *Hormone Research Laboratory 1950-1980,* which were published as limited editions by the University of California Press to commemorate the twentieth and thirtieth anniversaries, respectively, of the founding of the laboratory. It was helpful to read the preface by Jerker Porath and the forward by the editors in *Proceedings of the International Workshop on Hormones and Proteins (1974),* eds. T. A. Bewley, L. Ma, and J. Ramachandran, published by the Chinese University of Hong Kong; and to read the dedication by J. Ramachandran and J. Meienhofer in *Archives of Biochemistry and Biophysics,* vol. 225, 1983. The last two books commemorated Professor Li's sixtieth and seventieth birthdays, respectively.

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Photo by Joel Broida

Bernd Matthias

BERND THEODOR MATTHIAS

June 8, 1918-October 27, 1980

BY T. H. GEBALLE AND J. K. HULM

FOR OVER THREE DECADES, from the time of his doctoral research until his death, Bernd Matthias was a leading discoverer of cooperative phenomena in solids. He excelled in discovering superconductivity, ferroelectricity and ferromagnetism in new materials, and left a legacy of many hundreds of new superconducting and ferroelectric compounds with a wide variety of properties. Superconductivity and ferroelectricity are now regarded as common occurrences in nature rather than as exceptional, as they were when he commenced his lifelong quest. Along the way he discovered unexpected classes of ferromagnetic compounds as well. His unique creativity was based on a remarkably deep appreciation of relationships embedded in Mendeleev's periodic table of the elements. He frequently attributed his discoveries purely to intuition. His intuition was based on his eagerness to experiment with many different materials, a phenomenal memory, a quick mind, and an uninhibited belief in the simplicity of nature. His enthusiasm for science was fueled by an unabashed joy in discovering something new, particularly when it did not depend at all on theoretical input.

Anyone who knew Bernd was brought under the spell of his powerful personality. While some were turned off by his excesses, most were turned on. He was able to communicate in many languages at a deep and personal level with almost anyone. He had scientific friends and former students scattered all over the world with whom he shared an intimate relationship and with whom he would collaborate when there was a chance to gain some new insight. Once, when returning from a trip, he told me (T.H.G.) joyfully how he replied to the taxi cab driver's query "Where to?" with "It doesn't matter, they want me everywhere."

PERSONAL HISTORY

Bernd was born in Frankfurt during the closing days of the First World War. His father, a well-to-do merchant, died when he was very young and the family, consisting of his mother and his younger sister Judith, moved a short distance away to the small town of Koenigstein/Tanunus in 1924. He attended primary school and went three or four years to the Realgymnasium. His mother created a free, intellectual, and indulgent atmosphere. Judith remembers seeing "smoke rising from the corner of his room in which he was experimenting." Little more is known about the family history, but the imprint of those early days made a lasting impression. The first thing Bernd unpacked during his frequent traveling in later life was his mother's portrait. His mother, sensing the Third Reich, sent Bernd, at the age of fourteen, to college at the Knabeninstitute auf dem Rosenberg, St. Gallen, Switzerland. That was the end of his German family life. Every Yom Kippur Bernd would renew his heritage (of his grandparents, only his paternal grandmother was not Jewish) by fasting and attending the most orthodox synagogue he could find wherever he happened to be. During the rest of the year religion itself was only a minimal part of his life, but his spiritual nature remained a dominant force.

Bernd entered the Federal Institute of Technology (ETH) in Zurich in 1936 after receiving his "Matur" at the Institute Montana, Zugerberg, Zug. He studied physics under the influence of Georg Wentzel, who became his friend, and teachers such as Karrer and Pauli. His mother's suicide in 1938 (he told me [T.H.G.] he heard the fatal shot over a long-distance phone line) left him on his own with no financial support. His future as a physicist took a fortunate turn when he became a graduate student of Paul Scherrer and commenced his lifelong study of cooperative phenomena, starting with piezo and ferroelectricity. He received his Ph.D. in 1943, and remained a research associate and close friend of Scherrer.

Bernd came to the United States in 1947 at the invitation of Arthur von Hippel and, even though he stayed in von Hippel's lab at MIT only one-year, they became good friends. William Shockley was instrumental in bringing him to Bell Labs in Murray Hill. He hired Joe Remeika, at the time an unknown and untrained technician, without the approval of the personnel department. Together they initiated work on $BaTiO_3$ before Bernd took a leave of absence to be an assistant professor (1949-51) at the University of Chicago. There he became an intimate friend of John Hulm from whom he learned techniques of experimental low temperature physics, including running the locally constructed liquefier. With encouragement from Enrico Fermi, Bernd felt that if more superconductors could be found, the patterns of occurrence might provide some essential clues, particularly since there had been little progress in developing a fundamental theory even after four decades of trying. W. H. (Willy) Zachariasen also offered encouragement plus an uncanny ability to identify new structures in complex X-ray diffraction patterns. Willy became a close, lifelong friend and sometime father figure on whom Bernd could rely for honest, often blunt and pithy responses to ideas and opinions, whether outlandish or otherwise.

Bernd returned to Bell Labs at Murray Hill in 1951 and Remeika continued their efforts on ferroelectrics while Bernd continued searching for new superconductors. He spent almost all of his time making new materials and measuring them. He devoted almost no time to the construction of apparatus or anything else that got in the way of his finding new materials. The detection system was simply a ballistic galvanometer connected to a set of balanced coils which could be cooled to liquid helium temperatures and into which six samples could be lowered by a winch arrangement constructed by Ernest Corenzwit, who later became Bernd's principal assistant and constant chess-playing opponent. There was no ambiguity of phase or signal—if, upon lowering the sample into the coil, the galvanometer needle moved to the left it meant superconductivity, to the right it meant ferromagnetism, and no response meant "nothing." Volume fractions could be crudely estimated from the small deflections and they were carefully observed as they provided important information in identifying minority phases.

Bernd started working with Geballe in 1953 studying the superconducting transition in Nb_3Sn that Bernd had just discovered and which was found to have the then highest known transition temperature. They spent many hours, typically on weekends, in the Bell library scanning newly arrived journals for recently investigated materials which showed some promise of having unusual characteristics. Typically these could be made, characterized by powder X-ray diffraction, and measured within a short time frame by a capable and involved group—Corenzwit, Vera Compton, and George Hull, and later Louis Longinotti. Members of the staff at Bell and others outside became involved when the possibility of new physics emerged. They appear as coauthors with Bernd on many of his 364 publications. It was at Bell that Bernd made most of his major discoveries, and he remained a member of the staff there for the rest of his life.

In 1961 Bernd became a professor at the newly established La Jolla University, later the San Diego campus of the University of California and, with other distinguished physicists such as George Feher, Walter Kohn, and Harry Suhl, and later John Wheatley, built the La Jolla campus into a renowned center of condensed matter physics which attracted outstanding students and visitors. He instilled in his own students, Paul Chu, Louis Creveling, John Englehardt, Zach Fisk, David Hamilton, Hunter Hill, John Huber, Tony Jensen, David Johnston, K. S. Kim, Gordon Knapp, Angus Lawson, Brian Maple, Shaun McCarthy, Brian Sales, Al Sweedler, George Webb, Dieter Wohlleben, among others his passion for discovery using the direct approach of synthesizing, observing, and making empirical generalization. They, along with postdocs, such as Chris Raub, Fred Smith, and visiting student Tord Claeson, have continued his tradition in their own individual ways. He cared little about regular hours. Typically Bernd would come into the laboratory around midnight and the students knew if they wanted to touch base with him they had better be there with work progressing.

In 1951 Bernd married Joan Trapp, the lovely and very literate daughter of the Unitarian minister in Summit. The Trapp family to a large extent replaced the one he had lost in Europe. As with the rest of Bernd's life the marriage was unconventional. There were no children. He loved parties, and his students were always welcome and treated as equals.

The Matthiases enjoyed close relationships with accomplished friends from many walks of life.

Bernd spent his summers at Los Alamos starting in the late 1950s in the seemingly incongruous position as a consultant in the theoretical group where he was encouraged to look into anything that interested him. In 1971 he was appointed (the first) Los Alamos fellow. Bernd led overlapping but distinct research programs at La Jolla, Bell, and Los Alamos throughout the 1970s. In 1980 he was planning to extend his reach even more by returning part-time to where he started—Switzerland and Germany—when he died suddenly following a massive heart attack. As science is truly international, Bernd was truly an international scientist.

LECTURING ABILITY AND IMPACT ON THE SCIENTIFIC COMMUNITY

Matthias was in great demand as a speaker to technical or general audiences. He especially cultivated an informal style, which hardly ever included a manuscript prepared in advance or a text read to the audience directly. He used slides sparingly, but almost always utilized an up-to-date version of the periodic table of superconducting elements which he greatly enlarged in his lifetime. He would usually introduce this slide with a slightly sarcastic laugh, saying that he hoped the audience was familiar with the table. The suggestion that some people might not be was often part of a general criticism of theoretical work on superconductivity which included the message that theorists hardly ever made any useful predictions of new superconductors and therefore were not of much help to experimental works in the field. This proposition was, of course, advanced merely to promote an argument which it almost always did, but without excessive ego damage amongst Bernd's large group of theoretical friends.

Bernd was elected to the National Academy of Sciences in 1965 and to the American Academy of Arts and Sciences in the same year. He received numerous other awards, including the Oliver E. Buckley Award (1970) and the International Prize for New Materials (1979) of the American Physical Society.

SCIENTIFIC ACCOMPLISHMENTS

It is convenient to discuss Bernd's superconducting, ferroelectric, and magnetic studies separately, even though they were concurrent after his first twenty or so publications on ferro and piezoelectricity in Switzerland during his thesis and postdoctoral work. He was particularly proud of the crystal bandpass filter (with Scherrer), which is probably the only device-related work he published. During that time Vul's group in Russia and von Hippel's in the United States discovered ceramic samples of $BaTiO_3$ to be ferroelectric. Soon thereafter, with H. Blattner and W. Merz, he succeeded in growing single crystals which led directly to the study of the electrical anomalies (1947) and also (we assume) led to von Hippel's invitation to come to the United States. It should be remembered that at that time ferroelectricity was a rare occurrence, and only three ferroelectric structures were known. Rochelle salt (potassium sodium tartrate discovered in 1920), KH_2PO_4 and KH_2AsO_4 (discovered in 1935), and the above-mentioned $BaTiO_3$. With his colleagues J. P. Remeika, A. N. Holden, and E. A. Wood at Bell Labs, and John Hulm at Chicago, Bernd proceeded to discover new classes of oxygen octahedral ferroelectrics. With the periodic table as a guide, he was led to the alkali metal niobates and tantalates such as $LiNbO_3$ (1951). In the midfifties the dam was broken when, with Holden, Merz, and Remeika, he found the organic salt guanidine aluminum sulfate hexahydrate, followed by simple anhydrous ammo nium sulfate (3), to be ferroelectric. The dielectric anomalies of $(NH_4)_2SO_4$ had long been known. Bernd hypothesized that the N-H-O bond itself might be a source of ferroelectricity which is what motivated him to reinvestigate anhydrous ammonium sulfate in which there is no ambiguity due to water of hydration. He found it to become spontaneously polarized parallel to the a-axis below its transition at 223 K (1956), and thus found evidence for his hypothesis, as well as giving credibility to the idea that the many dielectric anomalies reported in the literature were actually due to ferro or antiferroelectricity. By 1957, after discovering two more ferroelectrics (glycine sulfate and calcium strontium propionate with Remeika and C. E. Miller), Bernd's interests turned to superconductivity and ferromagnetism. His only further publications on ferroelectricity were a few review articles.

The initial search for superconductivity that John Hulm and Matthias engaged in at the Institute for Metals at Chicago was motivated partly by the work of Walther Meissner who discovered some superconducting interstitial compounds of transition metals with borides, carbides, and nitrides in Berlin in the early 1930s. They used the measurement of magnetic susceptibility as described above, rather than the resistance measurement employed by Meissner, which is less reliable when it comes to identifying the phase responsible for the superconductivity in multiphase samples. They confirmed much of Meissner's work, including the 10 K transition of NbC and found MoN with a 12 K transition, second only to NbN.

Shortly after returning to Bell Bernd discovered superconductivity in $CoSi_2$. The unexpected occurrence of superconductivity in a compound made by combining a ferromagnet with a semi conductor gave Bernd great pleasure which he shared with many audiences. Further work with

John Hulm (1953) showed that the $CoSi_2$ had the fluorite structure which had been regarded as one of ionic semiconductors. The earlier discovery of Meissner that superconductivity could be obtained by combining nonsuperconducting Cu with sulfur to form CuS was no longer an isolated case. $CeCo_2$, a superconductor composed of two magnetic elements discovered in England by G. F. Smith and I. R. Harris, was another of Bernd's favorites.

The discovery of superconductivity in V_3Si with the beta tungsten structure in 1953 by Hardy and Hulm opened still another family of superconductors to explore, and this one proved to be the most important one for the rest of Bernd's life. He was searching for superconductors with high transition temperatures and, again using the periodic table as a guide, was able to find the first compounds of niobium in the beta tungsten structure with tin, osmium, iridium, and platinum. Of these, Nb_3Sn was found to have the highest known transition temperature, just above 18 K (1954). It is perhaps not untypical that the initial interest in Nb_3Sn was low. In fact, it was referred to in a *Physics Today* write-up as "schmutz physiks"; yet, it became the superconductor with the most challenging normal state and superconducting state properties after the discovery by J. E. Kunzler, J. H. Wernick, E. Buehler, and F. Hsu of its high-field, high-current capabilities. It remained such for the next three decades until it was replaced in 1986 by the High Tc cuprate layered perovskites of J. G. Bednorz and K. A. M. Müller.

By the end of 1954 Bernd was ready to make one of his most important generalizations which came to be widely known as the "Matthias rules." In a paper (1955) entitled "Empirical Relation between Superconductivity and the Number of Valence Electrons per Atom" he demonstrated that a simple universal curve could be drawn for Tc as a function of the average valence electron per atom ratio in elements, alloys, and compounds with maxima at five and seven and a minimum at six. This was consistent with J. G. Daunt's observation that Tc correlated with heat capacity and magnetic susceptibility, and had the virtue of being very simple with predictive power that depended only on the periodic table. The usefulness of this Matthias rule when only a few dozen superconducting alloys and compounds were known is evident if one considers only the prediction as to what happens when Ti (valence 4) is alloyed with any or all of the transition elements to its right in the periodic table. The rule predicted that the Tc of Ti would increase upon alloying it with Nb, and that alloying with Mo would do likewise and as a function of concentration the initial rise in Tc would be twice as fast. Quantitative investigations by Hulm and Blaugher showed that the Tc maxima were at 4.7 and 6.9 average valence electrons per atom. The predictive value of the simple rule was particularly useful in the 1950s when the purity of transition metals was a major difficulty. In fact, Bernd took the apparent breakdowns in his rule as a signal that there was a materials problem, such as the presence of an unknown or unsuspected phase (1962), as was found to be the case of traces of superconducting beta uranium in alpha uranium, or traces of the superconducting compound $LaRh_5$ in pure Rh. In a comprehensive discussion "Superconductivity in the Periodic System" (1957) Bernd concluded from the available experimental data that a necessary condition for the occurrence of superconductivity is that the average number of valence electrons per atom cannot be smaller than two or greater than eight, and that between these limits the Tc is a function of volume and mass as well as the average valence electron count. The volume and mass dependencies helped to rationalize the available data but were not of much predictive help. The universality of the valence electron per atom ratio for the d-band transition metals with the two maxima can be understood as related to the filling of the d-band and, in retrospect, can be expected to hold very well for elements and alloys and compounds where there is not too much charge contrast and a rigid band filling model is applicable. Bernd recognized that such a simple model did not work for ternary compounds and never gave up trying to find a simple way of understanding them.

Rare-earth elements were obtained as highly purified metals as a result of the Manhattan Project work of Frank Spedding at the Ames Laboratory. As soon as they were released Bernd obtained them directly from Spedding and started exploring the relationship between the rare earths with their magnetic f-shell electrons and superconductivity. In a seminal paper "Spin Exchange in Superconductors" (1958) with Suhl and Corenzwit and input from Conyers Herring he studied the effect of dilute solutions of the rare-earth metals in superconducting lanthanum. The relevant interaction was found from the concentration dependence of the depression of the superconducting transition of lanthanum to be dependent on the spin rather than the total angular momentum of the f electrons. This work was followed by a series of important papers in which the relationship between superconductivity and ferromagnetism and their coexistence was investigated in Laves phases (1959) such as $Y_{(1-x)} Gd_xOs_2$.

The relationship between superconductivity and magnetism was extended to d-band metals in an important study of dilute solutions of Fe in transition metal alloys (1962). The work was carried out interactively with the theorists A. M. Clogston, P. A. Wolff, and P. W. Anderson. Anderson's theory of localized magnetic states, later was recognized in his Nobel Prize citation, and Wolff's theory of magnetic states soon followed. The discovery of superconductivity in molybdenum was an experimental follow-on (1962).

Bernd believed that the strong relationship between ferromagnetism and superconductivity in transition metal alloys and compounds might lead to a kind of superconductivity beyond the phonon-induced mechanism of the BCS theory. In contrast to the rapid decrease (some tens of degrees percent of Fe) found in some of the d-band metals reported above, there was the intriguing observation that dilute solutions of Fe and Co in titanium were ten times as effective in raising the Tc of the room temperature hexagonal phase of titanium than predicted from his empirical valence electron per atom rule; however, the anomalous behavior was given a simple metallurgical explanation after a careful investigation carried out with E. Raub's group in Germany established the presence of filaments of the bcc phase of titanium in which the Fe segregated and reached a concentration of roughly ten times its normal value. The first high-field magnet was produced with Kunzler and associates using an MoRe alloy with composition adjusted to give a high Tc, and which was ductile so that it could be easily fabricated (1961).

Bernd was also aware that the phonon mediated mechanism for superconductivity had only been established for non-transition metals with energy bands derived from s and p orbitals where magnetic effects were minimal. The superconducting isotope effect (the dependence of Tc on the square root of isotopic mass) had earlier been a key result in signaling the electron-phonon mechanism. The phonon mechanism was later directly established by superconducting tunneling spectroscopy of Rowell and coworkers, but at that time, in 1961, no successful tunneling spectroscopy had been possible with d-band metals, a fact that in itself was considered significant. (Later work showed the prob lems were materials related.) From Oak Ridge it was possible to obtain sufficient quantities of isotopes of ruthenium to make the first investigation of the isotope effect in d-band metals. Bernd considered the finding that there was no measurable dependence of the transition on mass (1961) to be evidence for a non-phonon mechanism. This was reinforced by further work with isotopes of superconducting osmium, although the latter actually showed a small effect. The inference of a new mechanism was almost immediately challenged by P. W. Anderson and P. Morel by their extension of BCS theory which included retardation effects and could account for the reduced isotope effects. The theoretical work was carried out at the same time in Anderson's office right around the corner from the lab where the experiments were done. Subsequently, when it became possible to make good tunnel junctions with transition metal, elements such as Nb spectroscopic studies found direct evidence for the phonon mechanism.

Bernd never stopped exploring new systems, looking for higher new Tc's and mechanisms. A comprehensive review published in 1963 and still valuable today discussed superconductivity in all the then known elements, alloys, and compounds (1963) and related the occurrence and nonoccurrence to crystal structure as well as to the Matthias rules. In retrospect, the compounds were restricted to binary and pseudo-binary phases and solid solutions and Bernd's generalizations really did not encompass the subtleties of ternary and more complex structures such as the layered cuprate structures in which Bednorz and Müller discovered high temperature superconductivity and likely another mechanism in 1986, although he remarked at a La Jolla seminar that "the ternary materials area is so fertile that even a blind chicken can find a grain."

Bernd noticed that when chemical substitutions resulted in an increasing Tc the increase was inevitably terminated by an instability (i.e., crossing a phase boundary to a new phase in which the superconductivity was degraded or nonexistent). This was encountered in the niobium-based betatungsten structures and eventually led to raising the superconducting transition in bulk $Nb_3(Al_{l-x}Ge_x)$ to above liquid hydrogen temperatures for the first time (1967).

Bernd and his students at La Jolla took an important step in oxide superconductivity when they discovered superconductivity in the alkali metal-tungsten bronzes (1964). This, the first ternary system he studied, already violated his rule that superconductivity was favored in cubic structures, a result he later explained by invoking a weak symmetry-breaking ferroelectric-related transition in the cubic phase (1967). Bernd concluded that "it is ironic that not ferromagnetism, but ferroelectricity instead should be the phenomenon most incompatible with superconductivity" (1967); however, he was intrigued with the enhancement of superconductivity which was found at phase transitions and lattice instabilities (1967).

Bernd's interest in intermetallic boride superconductors, which started with his first work with Hulm at Chicago, continued throughout his career with studies of superconductivity and ferromagnetism in the binary hexa- and dodecaborides and culminated in the discovery of the ternary rare-earth rhodium borides with J. M. Vandenberg at Bell. The work continued at La Jolla with M. B. Maple and members of the Matthias and Maples groups and very interesting reentrant superconductivity and magnetic ordering phenomena were found (1978). This work followed the earlier discovery of superconductivity in the molybdenum-based Chevrel phases such as $PbMo_6S_8$ which Bernd considered the first high temperature ternary superconductor (1972).

Bernd's work at Los Alamos was carried on mainly dur ing the summers with an active group of collaborators, including R. D. Fowler, A. L. Giorgi, E. G. Szklarz,J. L. Smith, Z. Fisk, G. R. Stewart, H. H. Hill, C. E. Olsen, and N. H. Krikorian, among others. The investigations, as might be expected, included 5f and other radioactive elements, alloys and compounds, and difficult-to-handle materials such as beryllium. High melting refractory compounds were found to be superconducting. Evidence for an increase in Tc (positive isotope effect) with mass was found by investigating isotopes of alpha uranium (1967).

Over the years Bernd discovered two metallic ferromagnets in which magnetism was completely unexpected because none of the constituents were magnetic metals, namely $ZrZn_2$ (1958) and Sc_3In (1961). These became important subjects for the study of weak itinerant ferromagnetism which are still of current interest. The Los Alamos work added a related compound, $TiBe_2$, which has an enhanced magnetic susceptibility and can be made ferromagnetic by substituting copper for some titanium.

Some of the extent and legacy of Bernd's contributions have hopefully been documented in this memoir. There is one more anecdote to add; it concerns the discovery of ferromagnetism in the important class of europium chalcogenides with the NaC1 structure. Bernd and J. H. Van Vleck were having lunch at Murray Hill one day when Bernd remarked that the $EuIr_2$ he had prepared was ferromagnetic, and from Zachariasen's reasoning based on lattice constant considerations, the Eu had to be trivalent. Van held emphatically that there had to be a mistake because plus three europium was not magnetic. After a spirited argument Bernd went back to the laboratory and in a very short time discovered the new compound EuO simply by reacting Eu metal with the trioxide to form divalent Eu, and in so doing opened the way to interesting phenomena in a new class of semiconducting ferromagnets.

One of the major developments of the twentieth century has been the emergence of the science of materials from the traditional disciplines of physics, chemistry, and metallurgy. Bernd Matthias will be remembered as a premier contributor.

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Egon Orowan

EGON OROWAN

August 2, 1901-August 3, 1989

BY F. R. N. NABARRO AND A. S. ARGON

EGON OROWAN DIED in the Mount Auburn Hospital in Cambridge, Massachusetts, on 3 August 1989, a day after his 87th birthday. He is buried in the Mount Auburn Cemetery. Together with G.I. Taylor and Michael Polanyi, he was responsible for the introduction of the crystal dislocation into physics as the essential mediator of plastic deformation. Though he occasionally spoke at meetings concerned with science and technology policy, and wrote letters to the press on a number of topics, he was an essentially private person and left no biographical notes. In compiling the present Memoir, FRNN has been principally responsible for the period 1902-1951, which Orowan spent mainly in Europe, and ASA for the period 1951-1989, when Orowan was affiliated with the Massachusetts Institute of Technology.

1. ANCESTRY AND EARLY LIFE

Egon Orowan (Orován Egon in Hungarian) was born in Obuda, a part of Budapest[1] His father, Berthold

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[1] References preceded by the letter R refer to numbered papers deposited in the archives of The Royal Society.

Orowan, was a mechanical engineer (R2) and "managed some kind of factory in what is now Rumania" (R3). Berthold's parents were Jakob Orowan and Maria Neubauer, and Jakob was the son of Heinrich Orowan. The origin of the name Orowan is not clear. It sounds Slavonic to Hungarians and Hungarian to Slavs, and, according to family tradition, Heinrich was the first to use it. Egon told one of the writers that Orowan meant "a range of hills", but this meaning does not seem to be familiar to speakers of Hungarian or Czech.

Egon Orowan's mother was Josze (Josephine) Spitzer Ságvári. Her father, Mor Spitzer Ságvári, was originally named Spitzer (R3) but "became bankrupt in an agricultural crisis, went to Budapest and magyarized. Egon Orowan says his name was Mor, but Lorent [his nephew, Lorant Toth of Hungary (R4)] says it was Moris." One of Orowan's cousins, Endre Ságvári, "was an excellent Communist," and a park in Budapest was named for him. To compensate, the Orowans were also related to either Goering or Goebbels (R3). Orowan's wife, Jolan Schonfeld, was a pianist, who studied under Bela Bartok in the Budapest Academy of Music about the year 1919 or 1920. Here she met Egon Orowan, and they became friends, but were not at that time deeply attached. She stayed in Germany until about 1938, then left her work and all her possessions, and fled to her sister in Paris. After a year she found work as a domestic servant in England. She and Egon Orowan met again, and married on 20 January 1941.

According to the biographical note in his Berlin Dr. Ing. thesis (R2), Orowan studied at the Staatsobergymnasium in the IX district of Budapest, taking his Reifeprüfung in June 1920. In the academic years 1920/21 and 1921/22 he studied physics, chemistry, mathematics and astronomy in the University of Vienna. He did practical work in the winter semester of 1922, and began his studies at the Technical University of Berlin in the summer semester of 1928. After initially studying mechanical engineering, then electrical engineering, he transferred to physics under the influence of Professor R. Becker. At the end of 1928 he became Becker's assistant, and underwent his Diplom-Hauptprüfung at the end of the winter semester 1928/29. He began his doctoral research in autumn 1931, and at the time he presented his thesis (1932) he was assistant to Professors M. Volmer and W. Westphal. One of his papers is dated 8 July 1933 and addressed from Berlin-Charlottenburg; another, received 30 August 1933, describes him as "zur Zeit in Budapest". As Orowan explained in his talk at the Sorby Centennial Meeting (R4A): "For a time I could not find employment, and I lived with my mother, rethinking the results of my experiments of the last three years." (Orowan's father died in January 1933).

A letter (R5) from Professor László Bartha, Director of the Research Institute for Technical Physics of the Hungarian Academy of Sciences, says that Orowan worked with the Tungsram Research Laboratory between 1936-1939, under the supervision of Dr. Imre Br6dy. According to this letter and to (R6), Br6dy invented the krypton-filled light bulb. With the help of Mihály (Michael) Polanyi, he developed a new process for extracting krypton from air. Bartha's letter says that "Orowan was the person, who helped him to verify the large-scale separation of krypton from air by fractioned distillation of liquid air. He played an important role at the installation of a pilot plant for krypton manufacturing in a small town—Ajka—about 80 miles from Budapest. I could not find any papers or notes of him from that period."

By 1937, Orowan had moved to Birmingham (R2). The reasons for his move are not clear. According to his daughter (R3) "my understanding (which may not correspond to reality; sometimes things were hidden from me as a child and never rose to the surface) is that after a couple of years managing that tungsten process in the factory, he was offered a job in Birmingham sometime around 1937 and he went there, well before Hitler really started to misbehave." [Hitler had remilitarized the Rheinland in March 1936, occupied Austria in 1938, and Czechoslovakia in 1939].

EXPERIMENTAL WORK IN BERLIN

Orowan's doctoral thesis was not on the topic of crystal plasticity on which he started to work under Richard Becker, although his first published paper (1)[2] and his most outstanding contribution to physics (9) were on this topic. His thesis was on the cleavage of mica. His own account (R4A) is that: "The change of the subject was my fault, not Becker's. I received the problem when I was running across the main court of the Technische Hochschule one day; a fellow student ran along the other diagonal, we came within earshot near the center, and he shouted to me: 'What is the tensile strength of mica?' I shouted back 'I will tell you tomorrow.' This was the start of the doctoral thesis; I informed Becker about it when it was finished . . . in fact I could have done little if I had studied at an efficiently organized university which took care of all the students' time."

In (R4A) Orowan claims that this work "represented the first confirmation of the Griffith theory in the case of a crystalline material." The measured ("technical") tensile strength of a crystal is usually orders of magnitude less than the theoretical tensile strength. Griffith showed that this could be explained by the concentration of the applied

[2] References without the prefix R are to publications of Egon Orowan, numbered according to the bibliography at the end of this Memoir.

stress which occurs at the tip of a pre-existing crack. The question arose whether these cracks (or other centres of weakness) were accidental surface defects or were defects necessarily and systematically present in the real crystal, the so-called "Lockerstellen". The technical strength does not seem to vary greatly from one sample to another, and this fact seems to point to the existence of a systematic array of defects.

The precise lamellar cleavage of mica occurs not so much because the binding energy between sheets is small as because the sheets remain elastic even under large stresses in their plane, as is shown by their flexibility. Orowan had the simple idea of stretching a sheet of mica in its plane, using grips much narrower than the sheet, so that the edges of the sheet were free from stress and cracks in the edges would not lead to fracture. The simple idea was less simple in execution; he had to design complicated self-centering grips which ensured that both edges of the strip were simultaneously free from tensile stress. Nevertheless, the sheets were cleaved from blocks whose edges were cut very gently with a diamond saw. These sheets with unstretched edges had tensile strengths up to ten times those usually measured, showing conclusively that the usual tensile strength is controlled by defects in the edges of the sheets. Sheets with stretched edges had strengths which were of the usual order, but differed systematically between those cleaved from blocks cut with a diamond saw and those cut with shears, again demonstrating that the observed strength is determined by surface defects on the edges of the sheets. Orowan gave a detailed discussion of the fracture process in this exceedingly anisotropic material. The explanation is complicated, depending on the ability of a freshly-cleaved pair of surfaces to come together and heal perfectly. There is a footnote, which is interesting in connection with his later preoccupation with seismology and tectonics, in which he points out that the differing plastic properties of mica and of quartz play an important role in geology.

The most important conclusion is that dangerous defects are extremely rare in mica; a sheet may be reduced to half of its original thickness over a region several millimeters long by the peeling-off of imperfect layers, and yet break in another region where the stress is only half as great. This could not happen if the thinned region contained many dangerous defects.

In a paper submitted soon afterwards (5), Orowan struggled with a number of problems of brittle fracture, the effect of sample size, the effect of grain size and the Joffé-effect that a crystal of rock salt is stronger when it is being dissolved in a liquid. The principal new results are that the grain size is the effective upper limit of the size of a crack, so that, in rough agreement with experiments, the fracture stress is inversely proportional to the square root of the grain size, and that plastic flow increases the fracture stress when glide planes and fracture planes intersect, but decreases the fracture stress when these planes coincide, as for basal glide and fracture in zinc. A passing observation (6) was that a sheet of mica usually makes a sound like cardboard when it is struck; a similar sheet cut carefully with a diamond saw rings like steel. The damping in the former case arises entirely from the friction between cleaved layers at the cut edges.

ON CRYSTAL PLASTICITY

Orowan has given a full personal account (R4A) of the way in which he became involved in crystal plasticity: "My own introduction to dislocations happened on a hot Saturday afternoon in 1928. Until less than a year before that, I studied electrical engineering; I was more interested in phys ics, but my father, a mechanical engineer, knew that one could not make a living from Physics (this was before the Age of Government Contracts). So we compromised on electrical engineering which provided, at Berlin-Charlottenburg, a thrilling course of lectures on electromagnetic theory by Ernst Orlich, and also the nerve-racking tasks of computing, designing, and drawing a transformer, a motor or generator, and (this was my choice) a reversing rolling mill. Once a week, to soothe nerves and collect energy for another six days, I spent a day in the advanced laboratory course in physics offered by Ferdinand Kurlbaum whom I saw once, across the courtyard during the semesters I worked in his laboratory.

At the beginning and the end of the semester I had to acquire his signature for my roll card; this was given by the laboratory assistant who had the necessary rubber stamp. However, Kurlbaum died in 1927 and his temporary successor, the recently appointed professor of theoretical physics, Richard Becker, did not possess a signature stamp. I had to appear in his presence; he signed the card, asked why I, an electrical engineer, worked in the physical laboratory, and I explained. In the course of the following minute my life was changed by the circumstance that the professor's office was a tremendously large room (it was the room in which Gustav Hertz, Kurlbaum's eventual successor, developed the cyclic gas diffusion apparatus with which he separated the isotopes of neon and which was to play a prominent role in the manufacture of the bomb of Hiroshima). Becker was a shy and hesitating man; but by the time I approached the door of the huge room he struggled through with his decision making, called me back, and asked whether I would be interested in checking experimentally a "little theory of plasticity" he worked out three years before. Plasticity was a prosaic and even humiliating proposition in the age of De

Broglie, Heisenberg, and Schrödinger, but it was better than computing my sixtieth transformer, and I accepted with pleasure. I informed my father that I had changed back to physics; he received the news with stoic resignation. . .

. . .The assignment was to make single crystals of zinc, tin, etc., and to find out whether they had a trace of plasticity left at the temperature of liquid air: Becker's theory demanded complete brittleness at very low temperatures. Whatever Becker's theory might imply, Polanyi, Meissner and Schmid showed in 1930, before Orowan's equipment and crystals were ready, "that these metals were almost as ductile in liquid air as at room temperature." This was odd, "because the papers of Polanyi and Schmid contained the stereotyped remark that their metal crystals were drawn from the melt and then broken into pieces of suitable lengths in liquid air. When I asked Polanyi about this, he replied "Metal crystals broke in liquid air in those days: today they don't."

Though Orowan was not able to complete his experiments before the work of Meissner, Polanyi and Schmid became known, they formed the basis of his Diplomarbeit in February 1929 and of his first publication. One Saturday afternoon he had only one zinc crystal available. He dropped it on the floor, found it bent, straightened it, left it to anneal for some time, and tried a practice run. To his surprise, it extended with sharp jerks instead of flowing smoothly. From this observation, often repeated, he drew a surprising amount of information and was "led, almost unavoidably, to the concept of dislocation." It must also have led to his interest in the problem of the strain aging of steel. His paper with Becker (1) poses two questions, which are fundamental:

1. How does local gliding begin and what determines the number of glide processes which initiate every second?

2. How does the local gliding grow into an elementary act of gliding, and what determines the development (rate of gliding and extent of the individual act) of the elementary glide act?

One clear observation was that, in a stress relaxation experiment, the average size of the glide steps remained constant, while their frequency fell as the stress became less.

It is interesting to notice that this purely experimental paper on an unfashionable branch of physics was addressed from the Institute for Theoretical Physics of the Technische Hochschule, Berlin - Charlottenburg.

The real development of this work came only when Orowan returned to Budapest and stayed at home, unemployed and thinking. It led to the papers *Zur Kristallplastizität **I-V*** (7, 8, 9, 13, 14), and to several other papers (15, 16, 17, 19) in which the work is extended or applied to the observations of other workers.

Paper I begins by considering Becker's formula that the rate of deformation u of a crystal gliding under a stress s is given by

$$u = C \exp\left[-\frac{V(S-s)^2}{2GkT}\right]. \qquad (1)$$

Here C is an undetermined constant related to question (2) above, S is a stress which should be of the order of the theoretical yield stress of the crystal, and therefore perhaps 1/30 of the shear modulus G. By analyzing this formula, Orowan arrived at the important conclusion that the phenomena of crystal plasticity cannot be explained by ther mal fluctuations alone or by the presence of stress concentrations alone; both factors play an important role.

Becker had shown that the ratio $p = S/s$ could be determined by analyzing experimental results. The value of p turned out to be about 2.5. Yet it was well known that for pure single crystals the ratio of the theoretical shear strength S to the observed flow stress s was of order 10^2-10^4. The only solution seemed to be that glide was initiated in small regions where the local stress s was not the applied stress $s\sigma$, but was enhanced by a stress concentration factor q to the value $s = q\sigma$. Orowan also pointed out that, although Becker's formula (1) led to a rate of deformation which would become unobservably slow if the applied stress was held constant, in practice one adjusts the applied stress to obtain a convenient rate of flow. The consequences of this are developed further in (17). Becker's formula, with or without Orowan's stress concentration factor q, then shows that the flow stress s at temperature T is related to flow stress σ_0 at zero temperature by the simple formula

$$\sigma = \sigma_0 - B\sqrt{tT}. \qquad (2)$$

This formula fitted the observations for zinc and cadmium rather well. Later arguments have modified the formula, but the basic ideas underlying it remain valid.

Paper II, which is concerned with the theory of creep, is densely argued. It sets out to show that the "static" theory of creep, in which steady-state creep results from a balance between the rate of work hardening and the rate of recovery by softening, must be replaced by a "dynamical" theory based on modifications of Becker's formula. Orowan began by showing that the static theory leads to what has become known as the Baiey-Orowan equation. If the flow stress is σ, the elongation *x,* and time *t,* then a steady state is reached when

$$d\sigma = \frac{\partial \sigma}{\partial x}dx + \frac{\partial \sigma}{\partial t}dt = 0. \qquad (3)$$

Here $\partial\sigma/\partial x$ is the rate of work hardening, $-\partial\sigma/\partial t$ the rate of recovery. Equation (3) leads to a steady-state creep rate *u* given by

$$u = \frac{dx}{dt} = -\frac{\partial \sigma}{\partial t} / \frac{\partial \sigma}{\partial x}. \qquad (4)$$

This Bailey-Orowan equation has a remarkable history. Although Bailey developed the physical idea on which the equation is based, it seems that he did not publish the equation itself. Orowan published it, but only in order to show that it is not valid. However, he did not claim to have developed it, but attributed it to Polanyi. A further complication is that, in the form

$$\left(\frac{\partial x}{\partial t}\right)_\sigma = -\left(\frac{\partial \sigma}{\partial t}\right)_x / \left(\frac{\partial \sigma}{\partial x}\right)_t, \qquad (5)$$

it is a mathematical identity. How, then, can it not be valid? (The answer, of course, (13), is that (5) applies only when σ is a unique function of x and *t,* which is not the case in the actual experiments). Orowan's arguments are quantitative, but essentially demonstrate that at low temperatures $\partial\sigma / \partial t$ is negligibly small, $\partial\sigma/ \partial x$ is finite, and $\partial x/ \partial t$ is not necessarily small. (Surprisingly, in 1938 Orowan noted (19) that in a number of cases the activation energy for secondary creep is independent of stress, which "means that secondary creep is a flow by strain hardening recovery (thermal softening) ". It seems that the mechanisms of "secondary" or "steady-state" creep may be quite different at high and at low temperatures.)

Assuming the rate of work hardening to be constant, Orowan further modified Becker's formula to read

$$u = C\exp\left[-\frac{V(S-q\sigma+bx)^2}{2GkT}\right]. \qquad (6)$$

This formula is mathematically inconvenient, and, in applying it, Orowan replaced (6) by the function

$$\left.\begin{array}{ll} u = 0 & \text{for } 0<\sigma<c \\ u = a(\sigma-c-bx) & \text{for } \sigma>c \end{array}\right\} \qquad (7)$$

(It seems that c should be replaced by $c + bx$ in both conditions defining the ranges of σ). Using (7), Orowan was able to show that under usual testing conditions an apparent stress-strain curve of the conventional form would be obtained even in the total absence of work hardening. Consider, for example, a test with a constant rate of increase of stress. When the critical stress is first exceeded, the crystal begins to extend very slowly, while the rate of increase of stress is finite. Thus $d\sigma/dx$ is large. At high stresses the crystal flows rapidly, and $d\sigma/dx$ is small. The same argument explains the observed reductions of the flow stress when a crystal is suddenly unloaded and then reloaded at a finite rate after a brief interval. The crystal begins to flow slowly under a stress lower than that at which it was previously deforming at a finite rate, even if no recovery has occurred. This observation may be misinterpreted as a very rapid rate of recovery.

Paper III is one of the famous group of three papers, one by Orowan, one by G I Taylor and one by Polanyi, in which the idea of the dislocation as the carrier of plastic deformation was first introduced. Figures 1 (a) and (b) are taken from this paper. While the dislocations in edge orientation in Figure 1 (a) are the same as those introduced by Taylor and by Polanyi, Figure 1 (b) is unique in showing a glide zone bounded by a dislocation which takes all orientations, edge, screw and mixed. The paper is explicit in defining the dislocation as the boundary between those regions of the glide plane over which glide has or has not taken place. Orowan's description of the stresses near an edge dislocation does not seem entirely clear. The dislocation acts as a "stress concentrator" in the sense that the shear stresses it exerts on the glide plane are of opposite sign in front of it and behind it. The applied stress increases the numerical value of one of these stresses and decreases the numerical value of the other. An activation process of Becker's type will first occur on the side where the total stress is numerically greatest. Orowan clearly states that the (screw component) produces "sideways" shear stresses in planes lying perpendicular to the glide plane. The stress concentration produced by the stress concentrators in the crystal propagates in the glide plane in the form of dislocations.

Historically, it is clear that Orowan and Taylor developed the idea of dislocations as the carriers of plasticity independently. According to (R4A) "Soon after the appearance of the papers I received a letter with an enclosed galley-proof from Taylor; he wrote that he came to a similar picture, and his paper would soon appear in the *Proceedings of the*

Royal Society. In fact, he submitted his manuscript several weeks before Polanyi and I sent off ours; but the *Zeitschrift* published faster, and so our papers came out first". It is equally clear that Orowan and Polanyi were in fairly close touch. In Paper I, Orowan states clearly "The plasticity-inducing action of such 'dislocations' was recognized by Polanyi several years ago", and in (R4A) Orowan remarks that "Polanyi's term for a dislocation, for several years, had been 'vernier'; in his first publication about it he changed it to 'Versetzung', a term which I also adopted." In fact Orowan recognized (13) that the germ of the idea lay in a model invented by Prandtl before 1913 to explain the elastic aftereffect. But the development of the idea into a physical theory was due to Taylor and Orowan. Orowan recorded (R4A) that during his time in Budapest "Slowly I realized that

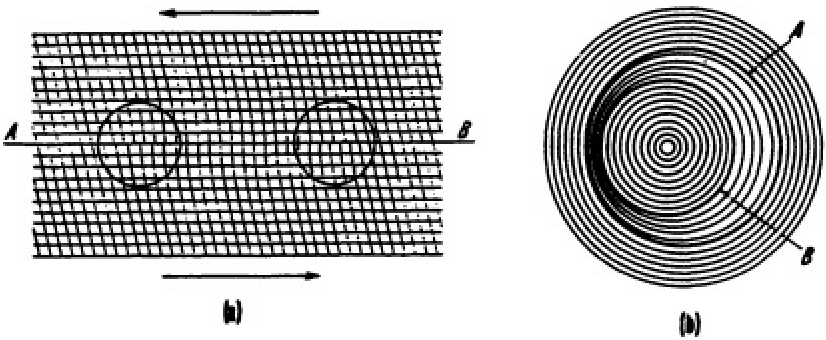

Figure 1: (a) Schematic picture of a local gliding; section in the glide direction perpendicular to the glide plane. The lattice was linear and orthogonal before loading; the dislocation zones are circled. The lattice does not allow the high shear stresses in the glide plane within the dislocation zones to be observed. (b) Schematic picture of a local gliding; view of the boundary of a glide plane. Before loading, the circles were concentric. The dislocation zone lies between the circles A and B.

dislocations were important enough to warrant a publication, and I wrote to Polanyi, with whom I discussed them several times, suggesting a joint paper. He replied that it was my bird and I should publish it; finally we agreed that we would send separate papers to Professor Scheel, editor of the *Zeitschrift für Physik*, and ask him to print them side by side. This he did".

Just 50 years after the event, Orowan wrote in a letter (R8):

> When my 1934 papers appeared, I received from Taylor a letter with galley-proofs of his papers soon to appear. I did not know who Taylor was, and his seniority, and wrote him that, unfortunately, his theory was all wrong; he replied that I was unable to follow a mathematical argument. However, soon he was convinced, and I spent a night in his house in Cambridge.
>
> The fact was that his theory was no theory at all: he assumed that the crystal had built in obstacles, and calculated how these would lead to a stress-strain curve *if* there were no pile-ups. He assumed *one* set of slip-planes, and obtained a parabolic stress-strain curve as given by cubic crystals, and the order of magnitude of hardening of cubic metals with intersecting slip.
>
> Since Taylor was an engineer, he assumed that dislocations would be produced abundantly by thermal activation (Griffith's calculation to the contrary is mentioned in your book).

A large part of the paper is devoted to a discussion of the mechanism of jerky extension. This jerky motion is most marked if the crystal has been previously bent and straightened. In translation "One can aptly equate this 'glide hindering' with the barriers to nucleation which appear in phase changes. . . . The 'hardening' which the second curve [taken some time after the first curve] shows in comparison with the first curve therefore does not consist in a real increase in the resistance to glide; it is a sort of *nucleation barrier,* which opposes the generation of the 'glide nucleus', the local gliding." The phenomena of jerky flow is evidence of the blocking of glide sources, and so indirect, but strong, evidence for the existence of glide sources. Orowan could not explain the mechanism of this blocking. It was not until 1947 that Cottrell explained the blocking as being produced by the segregation of solute atoms in the strain field of the dislocation. This mechanism is generally accepted, but it seems that Orowan was never convinced by it.

Paper IV is concerned with showing by quantitative arguments that the "dynamical" theory represents the reality of plastic deformation much better than does the "static" theory with the superposition of recovery. It begins with the analogy between the static theory and Aristotelian mechanics on the one hand and the dynamical theory and the Newtonian dynamics of conservative systems on the other hand, while work hardening in the dynamical theory is analogous to dissipation in Newtonian dynamics. Orowan then gives a formal theory of deformation in the presence of work hardening and recovery on the assumptions that:

(a) In the absence of recovery the flow stress σ is a unique function of the deformation x.
(b) Recovery returns the material to an earlier "state of damage"; further deformation after recovery therefore follows a curve displaced along the x axis from the original curve $\sigma(x)$.

A footnote says that (b) is equivalent to the statement that the totality of all possible states of damage forms a one-dimensional manifold mapped by the value of the flow stress σ, a concept that was developed extensively by E. W. Hart in 1970 and later years.

The result of the analysis is that recovery deforms the stress-strain curve by an inhomogeneous shear parallel to the x axis, so that critical stresses such as the flow stress are not altered. Moreover, the distortion of the stress-strain curve in a typical case is by a factor of 1.00002. The temperature dependence of the flow stress cannot be determined by recovery; for low-melting metals there is no recovery below about -20°C and "instantaneous" recovery about 100-150°C, whereas the temperature dependence of the flow stress is smooth from very low temperatures to the melting point. For tungsten, the discrepancy between the temperature dependence of the flow stress measured by Becker and that predicted by extrapolation of the rate of recovery at very high temperatures is "several dozen powers of ten". Similarly, recovery fails to explain the rate of the elastic aftereffect by many powers of ten.

Paper V "completes" the rate formulae (1) and (6) by allowing for jumps in the direction opposed to that favoured by the applied stress. A term with - s replaced by s is subtracted from the expressions shown.

THE CONTROVERSY WITH F. ZWICKY

On the same day (28 October 1932) that the *Zeitschrift für Physik* received Orowan's first paper (1), written jointly with Richard Becker, it received a much longer paper (2) by Orowan alone, entitled (in translation): Comment on the Works of F. Zwicky on the Structure of Real Crystals. The abstract shows a directness and self-confidence unusual in the first independent publication of a young scientist (Orowan was just 30 years old). Again in translation: "Of the two approaches which Zwicky has made to a theoretical foundation for his 'secondary structure hypothesis', one contains an error of calculation, and his assumed effect disappears when it is corrected; the second on the other hand is based on assumptions which are not fulfilled in the majority of crystals".

Zwicky had attempted to explain the structure-sensitive properties of crystals, such as mechanical and electrical breakdown, by showing that the ideal crystal structure represented only a local minimum of the energy, and that the lowest energy state contained regions with different lattice constants, which could act as nuclei of failure or of concentration of the elastic or electrical fields. His first arguments were based on the theory of ionic lattices, with allowance for polarization. They involved approximations. Orowan showed that more reasonable approximations would allow Zwicky's regions of irregularity to occur only in crystals having a dielectric polarizability much greater than any then known, and added that such polarizable ions would not form an imperfect structure of the sodium chloride type, but a perfect structure of a different type. Zwicky's second arguments concerned crystals of the sodium chloride type in which the repulsive forces between neighbouring ions decreased with their separation r more slowly than r^6. Such crystals would have a ferroelectric domain structure, and the domain boundaries would act as nuclei of mechanical or electrical breakdown. Orowan pointed out that most crystals are not ferroelectric at usual temperatures, so that Zwicky's second model does not approach the general problem of structure sensitivity.

Zwicky was not slow to publish his reply in *Helvetica Physica Acta* **6**, 210 (1932). The abstract claims that Orowan's criticism is unfounded (Haltlos), and a footnote to the abstract records that Orowan had sent Zwicky the draft of his paper, Zwicky had responded with his objections, and Orowan had published a revised version without informing Zwicky. The reply points out that neither Zwicky's nor Orowan's calculations are exact, and that (as one would say in more modern terms) a ferroelectric structure with immobile domain walls would not be easily distinguished from a perfect non-ferro electric crystal. Many other interesting issues are raised, such as the need to distinguish between an accidental mosaic structure (of higher energy than the perfect crystal) and a systematic secondary structure (of lower energy than the perfect crystal), but these have little bearing on Orowan's basic criticisms.

Orowan's reply, also in *Helvetica Physica Acta* (4), has a footnote by the editor and a footnote to the abstract. The first says that the reply is published at the express wish of Dr. Orowan, that it is followed by a response from Dr. Zwicky, that the discussion is then closed, and that the editor takes no responsibility for the content of either paper. The second, by Orowan, says that Zwicky's comments on his original draft contained no objections *(keinerlei Einwände),* but suggestions for improvements if Orowan published his work. After a general discussion of Zwicky's arguments, Orowan explains that the aim of his earlier criticism was exclusively to warn experimentalists against an undue reverence for theory by demonstrating the untenability of the "secondary structure hypothesis". In his reply, which is very moderate in tone, Zwicky leans heavily on calculations by Evjen, and then concentrates on the question of the apparent dielectric constant of a multidomain ferroelectric.

Orowan's final contribution (9) is entitled (in translation): *Comments on a Polemical Work by F. Zwicky.* The title has a footnote of 26 lines, complaining that Zwicky published his reply to Orowan in *Helvetica Physica Acta,* which was not open to Orowan as a non-Swiss. Zwicky had written to Orowan that he had chosen this medium "just in order to avoid further completely useless polemics. In this way I gave you the advantage of the much greater circulation of the *Zeitschrift für Physik".* Orowan began by claiming that *"nichts zu finden ist"* of the calculations on which Zwicky leans in the papers to which he refers. He then proceeded to a detailed refutation of the arguments in Zwicky's paper.

There is no doubt that Orowan understood the physics of the situation and Zwicky did not. It is surprising that the *Zeitschrift für Physik* allowed Orowan to express his criticisms in such a forthright style; perhaps he was the spokesman for some more senior physicists.

BIRMINGHAM 1937-1939

Orowan spent the years 1937-1939 in the physics department of the University of Birmingham; of which Oliphant was the head (R7). This was before the days in which the department of metallurgy had acquired its great reputation in the science of metals with the work of Cottrell, Raynor and others. His main contact seems to have been with Peierls, but he also thanks Moon for valuable discussion in the major paper (20) that he wrote in Birmingham.

Although (21) he constructed a "soft" tensile testing machine, his main interest was in a theory of fatigue. He began by assuming that a homogeneous sample such as a single crystal would suffer cumulative work hardening in a fatigue test, however small the applied stress range. (The remarkable ability of single crystals to "self-organize" into inhomogeneous structures was not then known.) Now suppose that there are "soft" regions (e.g. regions of stress concentration or favourably oriented grains) which deform plastically, while the applied alternating stress is too low to cause plastic deformation in the matrix. The stress amplitude in such a region falls below that in the matrix by an amount which is proportional to the plastic strain amplitude in the soft region. This plastic strain amplitude, and the amount of work hardening it produces, are in turn proportional to the excess of the effective stress amplitude in the region above the current flow stress of the "soft" region, which is continually getting harder. All of these changes decrease by a constant factor from each cycle to the next. The total change after infinitely many cycles is the finite sum of a geometrical series. If the original flow stress plus the sum of all the work-hardening increments of flow stress exceeds the fracture stress, the applied stress amplitude is above the safe limit. Developing the argument, Orowan predicted a relation between the stress amplitude A and the number of cycles to failure N very similar to that observed. The discrepancy was that the slope of the graph of log S against log N was -1, whereas the observed slope usually lies between -0.1 and -0.5. Orowan explained that at small stress amplitudes the Bauschinger effect would be important; small reverse strains occur quasi-elastically, without causing work hardening. (Oddly, Orowan did not mention the obvious interpretation of this effect in terms of dislocations.) The model correctly predicted the general observations that the safe stress range is approximately proportional to the static ultimate strength, but does not correlate with ductility, and depends little on the mean (bias) stress in an unsymmetrical stress cycle.

The other paper dating from this period (21) is largely an introduction to *Zur Kristallplastizität* for English-speakers, but it contains one elegant experimental demonstration and one partly successful theoretical prediction. The first consists of bending wires of copper and of iron around a finger. Copper, which work hardens, forms a smooth curve; iron, which shows a yield point, forms a polygon. The second considers the dissipation of energy by a moving dislocation by the analogy of a ball moving down a corrugated slope. "With the energy acquired the ball would be able to continue its movement without further help if the board were rigid . . . as the stress increases, the mechanism of the propagation of gliding will change over from the thermal activation mechanism ... to this momentum transfer mechanism". Orowan also remarks that "stress-strain curves cannot be used for processes like rolling where the deformation takes place in a less time than the first period of extension", showing that he was already preoccupied with the theory of rolling which was to occupy him for most of the war years.

A short discussion (19) is remarkable for the statement that the activation energy of secondary creep is independent of stress, and "the constancy of the activation energy means that secondary creep is a flow by strain hardening recovery and thermal softening". This approach, amplified in (27), is in apparent conflict with the arguments of *Zur Kristallplastizität.* The boundary between the two processes involved is still not clear (33).

Probably the most important consequence of Orowan's stay in Birmingham was that he introduced Peierls to the problem of the structure of a dislocation core and the stress required to move a dislocation through the lattice, a problem which Peierls solved with characteristic elegance.

CAMBRIDGE 1939-1950

Orowan's first few years in Cambridge produced several ingenious ideas. The spacing between slip bands could well be determined by the distance at which two dislocations could just pass one another under the applied stress (23). Kinking, a mechanism of deformation new in metals though long known in minerals, was described and analyzed (26). The presence of W. L. Bragg seemed to stimulate Orowan's interest in X-ray techniques. If a spot in a rotation photograph of a deformed crystal corresponding to a reflection g is unusually sharp because the curvature of the lattice planes focuses the beam on to the film, then the spot -*g* is unusually drawn out. This was illustrated by a remarkable photo graph from a cadmium crystal (24). If a fine wire grid is placed over the film and rotated through an angle proportional to the rotation of the crystal, each diffraction spot is crossed by fine parallel lines, and their inclination shows the angle of rotation of the crystal at which these planes come to satisfy the Bragg condition. These short contributions continued over many years: "static fatigue" in glass is attributed to reduction of the surface energy (28) and, with M. S. Paterson, X-ray line broadening is analyzed in metals deformed at different temperatures or with a change in temperature (38).

But Orowan's main interest at this time was devoted to the technology of munitions production. It led to a paper *The Calculation of Roll Pressure in Hot and Cold Flat Rolling* (27) which occupied 28 large pages of small print, and led to ten pages of printed discussion. It is a formidable sustained effort of applied mechanics. Orowan began by listing six physical approximations which had been introduced by previous workers. Disagreements with the only available experimental observations, those of Siebel and Lueg, could be as serious as a factor of four. "It is not clear which of the numerous simplifying assumptions and approximations is responsible.... In fact, Siebel's theory with its crude mathematical simplifications was often found to agree with roll pressure measurements much better than the theory put forward by Kármán who, with the same physical assumptions, had used far better mathematical approximations". Orowan set out "to attempt, first, a sufficiently general and accurate treatment of the problem, without respect to whether the method is simple enough for everyday use . . . from which simplified methods of calculation, valid for special cases of rolling, can be evolved". He did not seem perturbed by a discussant's report that "The rolling loads obtained . . . using a 10-inch slide rule . . . did not differ by more than 2.0 per cent from the accurate computations calculated on the Brunsviga machine". His reply to the discussion pointed out that: "Many rolling mill engineers could tell of unsuccessful experiments with rolling mills where hundreds of thousands of pounds could have been saved if a sufficiently accurate method had been available for calculating in advance whether the advantages were worth the expense".

The paper would be very difficult to read if Orowan did not lead the reader with the skill in exposition for which he became well known in his later years at MIT. He found the main errors of earlier calculations to be the assumptions that sections of the sheet normal to the rolling direction were homogeneously strained and that there was a constant coefficient of friction between the rolls and the strip. The latter assumption failed because it would lead to shear stresses in the strip several times larger than the flow stress of the material. There must be regions of the arc of contact where the tangential stress is determined by the coefficient of friction and regions where it is limited by the flow stress. Orowan replaced the former assumption by flow patters derived by Prandtl and by Nádai. Prandtl considered plastic compression between parallel plates, while Nádai considered plastic material flowing towards the apex of a wedge. Nádai's solution is appropriate for the material on the exit side of the rolls. For the entry side, Orowan made an intuitive change in the formulae; in the discussion, E. H. Lee showed the validity of this approach. As in problems of indentation, there must be a region in the strip near the line of maximum roll pressure where the stress in the strip is close to a hydrostatic pressure, and there is no plastic deformation.

The investigation involved some experimental work as well as plasticity theory. Sir Lawrence Bragg had raised the question "how a rolled bar is able to become longer even if its surface cannot slip on the rolls (i.e. in the case of complete sticking)." The solution was found by rolling laminated plasticine strips with their layers perpendicular to the roll direction. The surface layers extend suddenly as the bar enters the rolls. The deformation then propagates towards the middle of the strip as it passes through the roll.

A subsequent paper (32) by Orowan and Pascoe gives simplified formulae for roll pressure and power consumption for hot rolling where the flow stress is low (and assumed to be a linear function of the strain rate), and there is no slipping between the rolls and the sheet. Simplifying geometrical assumptions are also made, but these allow the calculation to be extended for the first time to stock of finite width.

A problem which became of great importance around 1944 was the catastrophic failure of welded "Liberty" ships by brittle fracture. The combination of physics and engineering involved was ideally suited to Orowan's gifts, and, with J. F. Nye and W. J. Cairns, he made some major contributions. Professor Nye has written the following account of the life and work of Orowan's group at that time:

> I was Orowan's first research student. Ernst Sondheimer and I had taken our Finals in December 1943 (how it came about that we were the only physics students to do so at that time is another story) and both Orowan and Randall (of magnetron fame), working in the Cavendish, needed a student to help them. Randall was the senior and had first pick and so got Sondheimer; Orowan got me. I came to the Cavendish in January 1944 to report to Sir Lawrence Bragg as head of the laboratory, and I remember asking him tentatively what kind of work it was to be, as I had been given no inkling before. This was wartime and one did not always expect to be told much. "Shatter phenomena," said Bragg firmly, and that was that. He took me down to the basement of the Cavendish where I met Orowan. On the left side of the corridor there was a rolling mill, with a conspicuous cartype gearbox with a gear lever attached to it, and there was also a testing

machine. It was on an engineering scale and rather larger than the kind of apparatus I had met as a physics student. Orowan had his desk and typewriter in a large room on the other side of the corridor, which was also occupied by Captain J. Los, a Polish exile, who was working on transient creep, and Dr. Hof from Austria, who was concerned with measurements on the rolling mill. This comprised the whole group. Soon after we were joined part-time by Warren Cairns.

There was a new contract with the Armament Research Department at Fort Halstead in Kent (under the Ministry of Supply) to study notch brittleness, and I was to help in this. The problem was to do with the fact that many of the new welded ships, used to bring supplies across the Atlantic, were cracking, some of them to such an extent that they were sinking. With the traditional method of construction, where the plates were rivetted together, any crack would run into a rivet hole or to the edge of the plate and not spread further, but, with the new method of welding the plates together, once a crack began to spread there was little to stop it. Some ships cracked completely in half. The problem was being studied from several different angles and we were to look at the fundamentals of the fracture process. Our final report (33) came out in July 1945, too late to help in the War. I remember a meeting in the Engineering Laboratory in Cambridge (26 October 1945) at which a number of the people concerned came together to report progress. G. I. Taylor described some impact experiments. Many of the participants thought they had the answer, but it was different for each one. The metallurgists were confident that the problem arose from the molybdenum in the steel; reduce the molybdenum content and there would be no cracking. The ship architects pointed out that the cracks typically started at the hatch copings, and therefore these should be made of a better grade of steel. Orowan (30) observed that, if the cleavage strength of the steel was less than about three times the yield stress, then notch brittleness was only to be expected. The mariners noted that the casualties were mostly in the North Atlantic, and therefore the ships should take care to take more southerly courses in warmer waters. The shipbuilders said the trouble was that it was American steel; British steel would not have that problem. Perhaps they were all correct.

Orowan would sit at his typewriter on one side of his desk and I would sit facing him. At first I had to follow up a number of references to examples of different kinds of fractures published in technical magazines that Orowan received, and which I was a little scornful of because they were not scien tific journals. When I asked him why he bothered with such stuff he replied, with his quaint and precise characteristic enunciation, "It is a kind of hobby". My role, apart from the experiments, was to read the many drafts he wrote and, and besides trying to be critical, to correct the English. He was fond of exploiting the eccentricities of the language in ways that would never occur to a native speaker. The draft he happened to be working on was always the "semi-final" draft; there were many, many semi-final drafts. When I went down to Fort Halstead to see Professor Mott, who was in charge of the project, Mott asked me how the work was going and I had to tell him that, alas, it was at a standstill, This clearly was not what he was expecting to hear. I explained that Orowan was fully occupied with writing the quarterly report. "He mustn't bother with that," said Mott, and I hurried back to Cambridge where Orowan sat at his typewriter to bring him the good news. After a while we moved upstairs to the first floor of the Cavendish where I still shared a room with him (the same room that Max Perutz later occupied). This did not mean that Cairns and I could always get his attention, because he had many visitors, and would always, in his politeness, deal with the latest one to join the queue, often to the dismay of the one he was talking to. As an extension of this principle, he would give the telephone absolute priority. We learned this, and when the need was urgent would take care to ring him on the internal phone. I never managed to get through a door behind him. I think he found my public school ways as odd, at first, as I found his Hungarian manner, but I found very soon that my puzzled amusement gave way to both respect and affection.

The ideas that Orowan developed while working on the notch brittleness contract were mostly included in the review article (39) on fracture that he wrote for Reports on Progress in Physics several years later. He was always slow to publish, and established something of a reputation for publishing important work in obscure places. This was partly gained because he published the work on notch brittleness in the Transactions of the Institution of Engineers and Shipbuilders in Scotland (31). In the same vein, new ideas on mechanical testing were published in a Report of the General Conference of the British Rheologists' Club (42). However, at about the time that I joined him he had just published his paper on "A simplified method...[for calculating the power needed by a rolling mill]", which had attracted much attention and had received an award from the Institute of Mechanical Engineers. His observations on kinking in cadmium, published in *Nature* (26), had been done a few years earlier. However, I do not think he published the related X-ray pictures. These he had in a drawer, and they showed how the streaks of asterism became spotty on annealing, due to polygonization. Later he set Robert Cahn to work on that problem when he arrived, and that was how polygonization came to be studied. The experimental work on transient creep in polycrystals was begun by Capt. Los; later it was continued by Eric Hall and C. L. Smith. I think logarithmic creep came from Hall's work, but I am not absolutely certain. Others who worked in the group included Robert Honeycombe (recrystallization), Norman Petch, Rodney Hill (Mathematical plasticity), F. H. Scott, Geoffrey Greenough (internal stresses from X-ray line broadening), Peter Pratt, and then there was Mr. Charter the laboratory assistant. (Mick Lomer was officially under Orowan but was "lent" to Bragg, and John Glen, who worked closely with us, was under Perutz.) We worked fairly closely with the crystallography group under Will Taylor. There were also two researchers we saw little of, who needed their own lathe because the metal they were working with was radioactive. I asked Orowan what they were working with, and I remember his diplomatic reply, "It is an element". This was before the bomb was dropped. Only later did I realize that it was uranium, and, if I had known, I would not have realized its significance.

Orowan was especially clever at bringing to bear on problems very simple ideas of stress analysis. For example, in the work on notch brittleness he appreciated the connection between the stress enhancement in a tensile specimen containing a deep notch and the problem of indentation by a circular punch; it was a matter of changing the sign of the stresses in the punch problem and so turning compression into tension. In a similar way he most elegantly explained how it is that a single crystal of aluminium, when it necks down towards the end of a tensile test, can develop a hole passing right through the centre of the neck. He would do his own meticulous mechanical drawings for the apparatus that was to be built in the workshop, and he spared time to teach me some of the tricks of mechanical drawing too. He was skillful with his hands and fingers and was keen on microscopy. We bought a polarizing microscope, which I used, under his supervision, to study the photoelastic effect of dislocations in silver chloride ('transparent metal'). It was he who had the idea for this work, made possible by the availability of rolled sheets of silver chloride from the Harshaw Chemical Company. He was generous in sharing his ideas and, as a supervisor, he taught by example. To learn from him, especially when his group was still a small one, was a delight.

On the topic of dislocations, an idea which at that time was simply a theoretical hypothesis, Orowan told me that he had played a small role in the genesis of Sir Lawrence Bragg's bubble model of a metal. Bragg had noticed, while mixing the fuel for his motor mower, that a number of equalsized bubbles were produced, which clung together on the surface to form a regular pattern like a crystal. He came in to the lab and asked his assistant Crowe to set up a small glass nozzle in a soap solution and blow air through it to reproduce the effect. Crowe was having no success; the bubbles were coming out with different sizes. At this point Orowan happened to be passing, looked to see what was happening and suggested to Crowe that he turn the nozzle so that it was pointing upwards. The tube being straight, the nozzle was naturally pointing down and the bubbles were getting in the way of each other. Crowe put a bend in the tube so that the nozzle pointed upwards and there was never any problem after that.

I mentioned that Orowan had many visitors, for he was much in demand for consultation. One such meeting was particularly fruitful, because it marked the beginning of modern glaciology. Vaughan Lewis, Lecturer in the Geography Department, was interested in cirque (or corrie) glaciers. He wanted to explain how they eroded their characteristically shaped basins; he had an idea that he called rotational slip and to work it out he needed to consider the mechanics—how the couple due to the weight of the ice mass was resisted by the friction of the bed. So, very wisely, he came to consult Orowan. I sat across the desk from them and listened with interest. The outcome was that Orowan became a main contributor to a joint meeting of the British Glaciological Society, the British Rheologists' Club, and the Institute of Metals, held at the Institute of Metals on April 29 1948. He emphasized that ice, like crystalline solids in general, is not linearly viscous but a plastic material.

He brought into glaciology for the first time the notion that creep, as studied in metals by Andrade many years before, was the basic mechanism of glacier flow. He then, characteristically, suggested the approximation of perfect plasticity with a constant yield stress and introduced three very simple models. The first was a rectangular block of ice on a slope. He showed that for a given slope there was a critical thickness for flow. The second model was a tall slender column; he showed that there was a critical height (about 20 m) beyond which the column would squeeze out at the base (in the discussion that followed W H Ward related this critical height to the depth of crevasses). The Greenland ice sheet was much thicker than

20 m (about 3000 m)and this led to the third model, which was of a wide mass of ice spreading out on a rough horizontal base; Orowan showed that from the known height and width of Greenland one could calculate a yield stress for ice, and the figure was of the correct order of magnitude. The brilliant simplicity of these models set the scene for a new era of glaciology, with the mechanics firmly based on the physical flow properties of ice as measured in the laboratory. This glaciological work was never published as a paper, but there is a full account of the meeting in the *Journal of Glaciology* (40). Having attended that meeting I was asked to write a report on it for *Nature* (J. F. Nye, the flow of glaciers, *Nature* **161**, 819 (1948)); thus began my own interest in glaciology.

Vaughan Lewis later persuaded Orowan to join him in a tour of Swiss glaciers with Professor Hollingworth, the geologist of University College, London (on a grant from the Royal Society). Orowan was already interested in rock mechanics (Nádai's book on plasticity was influential here) and this, together with the glaciology, led on later to his interest in earthquake mechanisms, flow in the Earth's mantle and his model of a convection cell in the mantle based on perfect plasticity. It was after he left Cambridge that he acquired a reputation for always doing something different from what he was employed to do (geophysics at Boeing, and economics at M.I.T.), but that was after I lost close touch with him.

I think he never felt at home in England (or perhaps anywhere). He was always the detached quizzical observer, always the foreigner. He had dining rights and later a Fellowship at Caius College, but college life did not interest him; he would have lunch in the town restaurant at the Corn Exchange (terrible sandwiches) rather than in college. He struck me as largely oblivious of his surroundings. He was fond of music. He and his wife were friends with the Diracs, and I believe with the Rideals also, but I doubt if there were any strong social ties to keep him in Cambridge. Perhaps the transition to the United States would not have seemed to him any great upheaval.

As Nye records, much of this work was first published in rather inaccessible places (29, 30, 31). It was only after a delay of several years that Orowan summarized the work in a long, but still very condensed, review paper (39), which ranges from Thomas Young's theory of the cohesive strength (published in 1805) to current unsolved problems. His criticisms of earlier work are expressed forcibly. Of the statistical theory of cracks he says "The space available for the present Report does not admit of even the briefest review of the results obtained; however, a few typical difficulties, not all of which have received due attention in the literature, must be mentioned". There follows a brilliant exposition of the observation that the brittle strength in compression is eight times that in tension, and of the interaction between tensile and hydrostatic stresses. His discussion of the "true" tensile strength ends: "A brief reflection shows that the 'true' strength, even if it existed and if it could be measured correctly, would have no practical importance for applications in engineering." On notch brittleness, to which he and his collaborators made such important contributions, he says "it is a much discussed question what types of specimen and test are best suited to give an accurate measurement of the tendency to notch brittleness.... These data can be obtained without any testing machine, by means of a vice, a hammer and a refrigerator or solid CO_2." The discussion on notch brittleness depends on a modification of calculations by Hencky and Prandtl of the indentation of an ideally plastic solid by a rigid punch. The modification shows that if the yield stress is Y, the maximum stress in a notched sample cannot exceed about 3Y. Let the brittle strength of the material be *B*. Then, in a test at room temperature,

"if $B > Y$, the material is brittle;
if $Y > B > 3Y$, the material is ductile in the tensile test but notch-brittle;
If $B > 3Y$, the material is fully ductile (not notch-brittle)"

Now take the material to low temperatures. The brittle strength *B* is hardly altered, but the flow stress *Y* is roughly trebled. A material notch-brittle at room temperature will be brittle at low temperatures. Finally, the very elegant results of (29) for a series of samples with notches of different depths are discussed briefly. It remains to be explained why there is a size effect in notch brittleness on the scale of centimetres. A square rod of side 0.5 cm with a machined notch may bend in an entirely ductile manner, while a geometrically similar sample 10 cm on a side breaks explosively in a bending test. The suggested explanation (43) is that, when a reasonable quantitative allowance is made for the work of plastic deformation on the surface of a nominally "brittle" fracture, the critical size of a Griffith crack is enhanced to about 1 mm. Such cracks will not be present in the virgin specimen, but must grow in a ductile manner during the test; there will be a size effect in notch brittleness in all specimens which are not much larger than the enhanced Griffith crack size.

One of Orowan's major contributions occurs only in the report of a discussion (36). He explained that a theory of the process of precipitation hardening was emerging, but that it could not account for the fall of flow stress on over-aging. He showed that when the particles of precipitate become large and widely separated, a dislocation will "bulge forward into the gaps between the particles... and finally detaches itself from the obstacles, leaving them encircled by small closed dislocation lines". These are now universally called Orowan loops. In a letter (R8) concerning a talk that Orowan intended to give at a meeting in London celebrating the 50th anniversary of the introduction of dislocations into the theory of crystal plasticity, Orowan wrote:

> Your suggestion that I should incorporate any addenda in "my talk" in London can hardly be carried out: according to the program I received two

> days ago I shall have only 15 or 20 minutes. My first experience in this field was that in Detroit 20 years ago, at the Sorby Symposium: I was asked to give a resumé of Taylor's contribution (he could not come), and I believed that each contribution was allotted 10 minutes. I gave 10 minutes to Taylor's paper and then started my talk, but I was interrupted after two sentences: it turned out that 10 minutes were the allotment of a speaker, not of a paper. On the 18th September in Detroit I began by mentioning that I would now continue my talk of 20 years ago; but again I misjudged the timing and was stopped after 17 or 18 minutes.—On one occasion I escaped this fate, thanks to Mott's intervention (in 1948): when I wanted to discuss briefly the condition of the extrusion of a dislocation between two obstacles, the chairman finally gave me 5 or 10 minutes for it in the discussion after Mott pressed him.

In another "discussion", this time "invited" (40), Orowan was able to explain a number of the curious features of glacier flow by considering ice to approximate to an ideally plastic solid rather than to a very viscous fluid, and thereby, as Nye writes, "set the scene for a new era of glaciology".

THE THEORY OF THE YIELD POINT

Orowan's first paper was on jerky extension, the paper in which he introduced the idea of a dislocation was largely devoted to the same effect, and he remained interested in the topic for another fifty years. In 1949 he wrote with W. Sylwestrowicz a paper (43) for The British Iron and Steel Research Association in which a single Lüders band was caused to run along an iron wire at a controlled speed. If straining was stopped and the sample was aged, a new band initiated in one of the grips ran through the old band at a higher stress, but the stress dropped to the original lower yield stress when the new band ran into the underformed part of the wire. This work was developed and later published by Sylwestrowicz and E. O. Hall. Later work at MIT led to a paper by R. A. Elliott, Egon Orowan and Teruyoshi Udoguchi (R9) and a paper (R10) by Orowan alone, writ ten largely when he was on sabbatical as a guest of the Boeing Scientific Research Laboratories. They were never published. Years later he wrote (R8):

> There is a very dark spot in my plasticity-career. In 1966-1967 I wanted to prove or disprove work of Mrs. Tipper and Polakowski, and carried out a thorough investigation with Mr. R. A. Elliott and Professor T. Udoguchi; I sent the paper, and its theoretical evaluation, in 1967 to *Phil. Mag.* The Editor sent them to Cottrell who, it seems, was sorry about them; not wanting to embark on a discussion, I postponed the reply until the affair froze. The two MSS may still be in the *Phil. Mag.*—I am enclosing the MS of the first. Since I have not followed the literature for years, I do not know whether the results have been published by somebody in the meantime. They show that, while a strain-aged steel shows the sharp yield point if it is strained in the same direction as before, it shows no trace of a yield point if the direction of straining is reversed. There are other remarkable phenomena also; you will see them in the MS [R9]. Of course, the theory of the YP requires extensive additions after these. What is needed ought to have been obvious already from my and Becker's 1932 paper and my 1934 papers, but the human brain is inefficient. I would also defend myself by the circumstance that in 1967 I switched over to my present field of work, and big-game hunting makes varmint-shooting less exciting.

The matter was clearly still on Orowan's mind in 1984. At the memorial service in the MIT Chapel on 15 September 1989 (R12), Walter Owen said:

> The last time that I saw Egon Orowan was not so long ago. With Ali Argon, we had lunch in Walker, where we met to try to help Egon with his arrangements to go to London to celebrate the events of 1934 at a meeting of the British Institute of Metals and the Royal Society, where he was to appear on a platform with Mott and Cottrell and others. It seemed to me that Egon wasn't very keen to go, but he did allow himself to be persuaded by Ali and myself in a fairly short space of time, and I had the impression that he allowed himself to be persuaded simply because he wanted to talk to both of us about the Cottrell explanation of the strain aging of steel which he considered to be wrong. Now he'd considered this to be wrong for thirty-five years at least and whenever he saw me he was reminded of thirty-five plus years ago when he for the first time thought this was wrong. So,

> periodically throughout these years he had continued this attack on Cottrell's idea of strain aging. But, during all this period of time he never published a word on any of this. In fact what there was written down in these famous pieces of paper on his desk, that have already been alluded to, were various versions of this attack that he added to from time to time. So I think he agreed to go to London because he wanted to present this attack on Cottrell's strain aging theory, and that he didn't really care too much about the British celebration of the discovery of dislocations. Unfortunately he didn't go to London because Mrs. Orowan was ill and he couldn't leave her. I think this was a pity because certainly he would have left the British scientific establishment with something to think about in no uncertain terms.

The paper R9 describes a series of experiments in which a thin-walled iron cylinder is tested in torsion. The stress system favours circumferential shear and axial shear equally; circumferential shear is induced by a circumferential groove, and then persists during the deformation. The unique feature of this design is that the direction of deformation can be reversed. If the tube is twisted plastically, and allowed to age, it will deform again in the same direction only under an increased stress, and shows a marked yield point. However, if, after aging, the tube is twisted in the opposite direction, there is a strong Bauschinger effect, plastic deformation setting in under a stress numerically less than that under which forward deformation was occurring, and there is no sign of a yield point. The second paper considers the interaction between internal stresses and the stress required to unlock a dislocation from an atmosphere, and concludes that the locking process must affect not only the mobile dislocations but also the obstacles which impede their motion. There was a long correspondence between Orowan and Cottrell on these papers; much of it is (R11) preserved in the MIT Archives. In reply to a letter of Orowan's dated January 25, 1968, Cottrell wrote "I am sorry you could not find my address. It is in fact the Cabinet Office. . . . Thank you also for the copies of your manuscripts. . . . . In fact I saw these two papers briefly in January, when W. H. Taylor invited me to referee them. I felt, however, that I was not in a good position to do so and I believe that he has now sent them to someone else." In the last letter of the series, dated April 2, 1968, Orowan wrote "pinning is quite unnecessary for preventing a general outbreak of plasticity . . . you can get no significant deformation without multiplication; the grain boundaries act, not as 'firebreaks' of deformation, but as multiplication-stoppers." He continued "Benefiting from our conversation I have re-written the Abstract (enclosed); I shall make alterations in the paper and acknowledge my debt to you, in a form that can not be interpreted as implying your agreement with the views given. "Unfortunately, he did not do this.

THE DECISION TO GO TO M.I.T.

In many ways Orowan's stay in Cambridge was a successful one. It was scientifically productive. He led a very effective group; what may have been a farewell tribute to him (R13) reads "To Dr. Orowan with best wishes from J.F. Alder, R.W. Cahn, W.J. Cairns, S.D. Charter, P.T. Davies, J.W. Glen, G.B. Greenough, E.O. Hall, R. Hill, R.W.K. Honeycombe, W.M. Lomer, D. Humphreys, J. Los, J.F. Nye, K.J. Pascoe, N.S. Paterson, N.J. Petch, V.A. Phillips, P.L. Pratt, C.L. Smith, E.M. Stokes, W. Sylwestrowicz, O.H. Wyatt, E. Yoffe." He was not without honour: Thomas Hawksley Gold Medal (1944, Fellow of Gonville and Caius College 1949 (R7), FRS 1947, University Reader 1947. Yet by 1950 it was well known that he was looking for a move. There were offers in plenty. The MIT archives contain a letter from E.P. Wigner inviting him to Princeton, one from F. Seitz inviting him to Illinois, a letter from D.B. Copland, Vice-Chancellor of the Australian National University, regretting that Orowan had not accepted their offer, and one from T.E. Allibone asking him to change his mind and join the AEI Research Laboratories in Aldermaston. But Orowan accepted the invitation of C. Richard Soderberg, Professor in Charge of the Department of Mechanical Engineering at MIT, to come for a trial period with a view to a permanent appointment. Chadwick wrote to Orowan during this trial period, outlining what had been done to stabilize Orowan's position in Cambridge, which was clearly not firmly assured. "I am not dissatisfied with what has been done. It is not all you wish for, but the rest may follow, after your return" (R14). It seems clear that Orowan's discontent was not solely with his uncertainty of tenure, but its real nature is not clear. D.B. Carpenter wrote "I do hope you will find in Massachusetts the freedom from administrative care you have been seeking", yet it seems that in some ways Orowan was seeking a more public rather than a more private position. Chadwick's letter has a paragraph "on the subject of Higher Technological Education", and the draft of a letter from Orowan to Soderberg says "the admirable Bulletin...of the MIT...is on loan to a high standing personality actively engaged in initiating reforms of the engineering education at British universities". His worries were clearly well known. Andrade wrote from the Royal Institution "It seemed to me that the course which you have taken was almost inevitable", D.A. Oliver, Director of Research, the BSA Group, wrote "I do know that certain Professorships did not seem entirely suitable and it may well be that time has run out on opportunities in the British Isles", while Mott wrote from Bristol "I . . . feel that in your case there was really no alternative". Rather more direct was Miss E. Simpson, Assistant Secretary of the Society for Visiting Scientists, who wrote "you did indicate your reasons for having to go to the United States, and I am very unhappy and distressed that this should be so. You will realize that your story is repeated often enough in other fields to make some of us very concerned, especially as we see no prospect of an amelioration", while Sir Charles Goodeve, Director of the British Iron and Steel Research Association, wrote "Cambridge has had its great days during the Rutherford period and it is suffering from a reaction." In a similar vein, Peierls wrote "(he moved to Cambridge, but never got a permanent job there, for which he blamed Bragg)" (R15), to which Mott commented (R16) "Blaming Bragg was a hobby. . .I'm afraid I did sometimes—but not after having experienced the pressures on a head of the Cav. and the choices he has to make." Walter Owen in his memorial address at MIT said "While he was at Cambridge he did some work on the rolling of steel sheet which was greeted with cries of complete incomprehension by the physicists in the Cavendish, who considered of course that engineering was not really the proper job for an English gentleman, at any rate". In fact, Bragg himself had contributed to this work, and Bragg was succeeded by Mott, who had done equally ungentlemanly work during the War years, and wrote "I was enthusiastic about Orowan's work" (R16).

CAMBRIDGE, MASSACHUSETTS 1950-1989

Orowan joined the Massachusetts Institute of Technology in the summer of 1950 as the George Westinghouse Professor in the Mechanical Engineering Department, after a three month trial period in the spring of that year with a title of Visiting Professor. Upon the resignation of Charles MacGregor, he became the head of the materials division. While the Department of Mechanical Engineering had traditionally a strong materials division, Orowan brought a fresh new mechanistic point of view to the teaching and research of mechanical behavior of materials.

During the period beginning in 1950 and extending to his formal retirement in 1968, Orowan continued to oc cupy himself with many of the same problems that he so successfully started to investigate while in England, namely: mechanisms of crystal plasticity, brittle and ductile fracture, fatigue, and the application of these to geology. After his retirement he added to these considerations other more philosophical questions of: the stability of the Western industrial economies, aging of societies, problems of higher education etc.—all of which occupied his attention up to the time of his death. The last effort which Orowan entitled "socionomy", that began in the early 60's as he started losing interest in the mechanical properties of engineering materials, and even in their applications to geology, has remained unpublished. At the time of his death this work, intended initially as a book of about 500 pages, was left behind in the form of 42 volumes of neatly arranged loose-leaf note books, covering the same subject matter in a number of different unfinished variations.

Orowan had remarkable talents as a teacher in clarifying complex concepts in mechanical behavior by simple and penetrating developments based almost exclusively on his own research. What made his lectures so memorable to both undergraduate and graduate students was his dramatizations that often began with the statement that "such and such a phenomenon was not at all well understood until some fateful moment rather recently when things suddenly became very clear". The identity of who eventually was responsible in the creation of the new clarity was evident to the experienced student. In fact, many discerning graduate students who also followed the literature separately, quickly became aware that they had been treated to a special dose of Orowania, even so, the coherent picture sketched out was greatly appreciated.

While always cordial in his interactions with his research students or collaborators, Orowan maintained a working relationship that resembled more that between a master craftsman and his apprentices rather than between a senior researcher and his junior collaborators. His research students probably learned more from him through the demonstrations of uncompromising logic in interpreting results and planning new steps in research than through any collection of facts or formal methodology. Orowan was capable of complex mathematical analysis but rarely engaged in it in the development of theory. He preferred logical qualitative arguments and simple experiments that demonstrated the validity of one mechanism over another, reinforced, if necessary, by an order of magnitude estimate. With the exception of his tour-de-force on the sheet rolling problem discussed in some detail above, most of his analysis did not go beyond the penetrating simple statements that captured general trends as in the case of the well known Orowan stress for overcoming precipitate obstacles. His students remember his justification for this uncomplicated approach from his statement that he felt it was more important to "supply vitamins rather than calories".

FRACTURE AND THE FRACTURE INSTABILITY

An interest which Orowan brought with him from England was the brittle fracture of ship steel. With his first doctoral student David Felbeck, Orowan (48) showed that the fracture instability in steels with very little ductility could be described adequately by the well known Griffith condition in which the specific surface energy term $\propto$ of the purely brittle material had to be re-interpreted as the superficial plastic fracture work per unit area, *p*, that can be associated with the two new surfaces of fracture. Through fracture experiments on large edge-cracked plates of the tanker Panaganset that broke up in 1947 in Boston Harbor, they estimated the value of *p* to be of the order of 3.5×10^6

erg/cm-^{2}. To Orowan this was another successful demonstration of the important work of Griffith which he considered to be of epoch-making magnitude and wrote often on how much it influenced his thinking, not only on fracture but in the conceptualization of dislocations and their origin in stress concentrations. In a long private letter to Cyril S. Smith recalling his early career, he stated his views about the work of Griffith as "I need not dwell on Griffith, whose work has been among the most consequential in physics in this century; it has opened up a new chapter well-known to all except the members of the Nobel Committee" (R21). In spite of this deep admiration, Orowan was well aware of the limitation of the Griffith conditions in problems of fracture with more pervasive plasticity, and furnished clear examples of inapplicable cases in a paper on the energy criteria of fracture (49), and one on the conditions for high velocity ductile fracture (50). Apart from such observations, however, Orowan did not pursue the study of ductile fracture and its mechanisms, and had very little liking for the formal developments in fracture mechanics initiated by Irwin, that were revolutionizing the study of fracture in engineering. From the early 50's to the early 70's Orowan continued with his interest in fracture mechanisms with studies in a random collection of materials. One such study was the statistics of strength of plate glass surfaces probed by the Hertzian fracture experiment (51), stimulated by his industrial consulting arrangement with the Pittsburgh Plate Glass Company, in connection with their new float-glass process for the production of plate glass. Other studies of this period included the fracture of crazable glassy polymers with Michael Doyle and others, where propagating cracks are preceded by a narrow plastic zone in the form of a craze (52), and fracture in adhesive joints (53). In the latter Orowan considered basic requirements for achieving tough joints with brittle cements and clarified the important role of residual stresses in the adhesive layer that can retard the growth of a crack in the layer by repeatedly diverting its propagation direction away from the median plane of the joint into the adherent. In connection with this study on adhesion he revisited the well known Young equation of capillarity and provided an elegant proof of it as applied to capillary equilibrium between liquids and solids, including a precise discussion of the important differences between the notions of surface energy, surface tension, and surface stress (54).

CRYSTAL PLASTICITY

In hindsight it is now clear that at the time of his move to the U.S.A. the mechanisms of crystal plasticity no longer occupied central stage to Orowan. Nevertheless, in a number of noteworthy publications he summarized his points of view. The first of these was a comprehensive position paper associated with a main lecture on creep in metallic and non-metallic materials (55) delivered at the First U.S. National Congress of Applied Mechanics. In this he provided a far reaching discussion of the rate mechanisms of plasticity as they manifest themselves in creep behavior. The discussion included anelastic creep (in which a finite concentration of activable deformation units are mechanically polarized); Newtonian, linear viscous creep (in which thermally activable deformation units are re-created at the same rate as they are polarized); non-Newtonian creep (in which the deformation work during a thermally activated event is no longer small when compared with kT); creep by diffusional flow and by grain boundary sliding; and finally, mechanistic reasons for the non-existence of a mechanical equation of state for plastic deformation. A small section discussing the possible mechanism of plasticity in both simple atomic and chain polymeric glasses admirably anticipated much of the confusion that beset the study of these phenomena by future researchers who unfortunately were unaware of Orowan's observations.

Another remarkably comprehensive summary of Orowan's views on crystal plasticity appeared as a longish chapter on Dislocations and Mechanical Properties, in a special publication edited by Morris Cohen, resulting from a special symposium on dislocations held in 1951 during an AIME meeting (56). In this chapter, which Orowan also used in his graduate classes on Physics of Strength and Plasticity at MIT as a reference for supplementary reading, he discussed in very simple terms important characteristics of dislocations, including their topological features, stress fields, energies, how they give rise to plastic strain through their motion, and how they multiply during straining. He then discussed, again in very simple terms, by resorting only to order of magnitude estimates, important phenomena such as the lattice resistance to dislocation motion, precipitation strengthening, work hardening, the yield phenomenon in low carbon steel, and the nucleation controlled processes such as twinning, martensitic shear transformations, recovery, and re-crystallization—all with a thorough historical perspective which was his penchant. The discussions of the Taylor theory of work hardening and the Cottrell theories of the yield point in low carbon steels include perceptive and sharp criticisms but offer little in the form of quantitative alternatives. The discussion on twinning and re-crystallization make a very clear case for the need of embryos or for topological mechanisms that can build up discrete interfaces gradually rather than nucleation of fully formed saddle-point configurations.

During the early 50's Orowan, together with his co-workers Bragaw, Sylwestrowicz, and Torti conducted extensive transient creep, strain-rate change and temperature jump experiments on polycrystalline aluminium, copper, and some solid solution alloys such as alpha brass and monel metal to explore the rate mechanism in crystal plasticity. In these experiments, which were never published beyond brief reports to the funding agencies (or doctoral theses deposited with the MIT Library), the emphasis was on the time law, i.e. whether logarithmic (no recovery) or Andradian (with recovery), with little or no mechanistic interpretation—beyond what was in the mind of Orowan. It is quite likely that during the course of these studies Orowan became convinced that the old Becker-Orowan formalism of nucleation controlled plasticity could not deal with these experiments, but apparently also remained unconvinced that the new developments on forest-cutting, advanced by Cottrell and co-workers or Seeger and co-workers were appropriate. In fact Orowan returned to this conflict of nucleation vs. propagation control many times in later life by recalling that in the German school in the 30's, of which he was a part, the task was to explain why plastic flow occurred at a level so low in comparison with the ideal shear strength. In comparison, in the English school pioneered by Taylor, dislocations were assumed to be easily created and had little resistance to their motion, requiring dislocation interactions to explain a finite plastic resistance.

GEOLOGY

As remarked in the recollections of Nye, quoted above, while still in England, Orowan developed a deep interest in plasticity and fracture problems related to glaciology which he later in the U.S.A. broadened to problems of seismology, tectonics, continental drift, rifts on the ocean floor, and the like. Here he found a fertile field in which he could exercise his inventiveness and apply knowledge of mechanisms of inelastic deformation and fracture on a grand scale, in his preferred style of the semi-quantitative order of magnitude approach. The re-emergence of interest in this field seems to coincide with a three month leave of absence in 1958, spent at the California Institute of Technology upon the invitation of B. Gütenberg. In a series of well structured and unpublished tutorial notes prepared for lectures at CalTech, Orowan presents a specialized application of the physics of deformation and fracture to problems of geology.

Throughout the 60's in a series of papers (57-64) Orowan considered key mechanisms involved in continental drift, and the associated problems of convection in the mantle, formation of mid-oceanic rifts, ocean floor spreading, orogenesis, and volcanism. In these carefully reasoned papers Orowan considers the impossibility of fully developed deep convection cells in the mantle on the basis of evidence of insufficient fluidity of the deep mantle and dismisses the alternative shallow convection model limited to the asthenosphere (a term which he does not like and proposes that it be replaced with "low hardness layer") as having too high a drag. Part of the problem associated with convection becomes rectified by considering incomplete cells in which the upward motion of rising material in the mid-oceanic rises is considered to obey a plastic plug-like flow, rather than Newtonian viscous and that the loop is closed by a slight rigid body motion of part of the mantle and adjustment in the fluid core—in a pattern that he entitles "transvection". In the discussion there are many instances where more precise material behavior in the form of plasticity, Andradian power law creep, fracture, and concepts such as plasticization of rocks are introduced to replace simple elastic or Newtonian viscous behavior. In a popular article in Scientific American (64) Orowan gives an overall synthesis and speculates on an alternative to radioactivity as the main heat source to drive convection, in the form of a large (100 km diameter) asteroid impacting the earth and setting off a long-term transient convection process. In support he points out that this would be of a magnitude similar to those responsible for the formation of the large maria on the visible side of the moon. Some of the later papers of this period were finished during a stay of Orowan at the Boeing Scientific Research laboratories in Seattle in 1966-67. In another exercise on the subject of processes on the grand scale Orowan considered the origin of the surface features of the moon (65). Based on the results of the lunar expeditions of the late 60's and early 70's and on examination of high resolution photographs brought back by the lunar orbiters and the Apollo missions, Orowan considered that all lunar features are explainable by meteorite and asteroid impacts, leaving no room for volcanism.

A final, and very long paper on the "Mechanics of Continental Drift" was submitted to the Royal Society in 1978 for publication in the Proceedings. The fate of this paper and how the Royal Society dealt with some of the consequences of its rejection is discussed below.

SOCIONOMY[3]

In 1962 Orowan accepted an invitation to spend a year at the Carnegie Institute of Technology as a Visiting Institute Professor with the understanding that he could spend his time on problems of evolution of societies and economics —subjects that occupied his attention with ever increasing dedication until the end of his life. In the Fall of 1972 he accepted another appointment as Alcoa Visiting Profes

[3] A word coined by Orowan to refer to the interrelationship between sociology and economics.

sor at the University of Pittsburgh for the same purpose, where he intensified his involvement in this subject and actually gave a lecture course for which he prepared detailed notes. In an interlude of yet another one-year leave in 1965-1966 at the Boeing Scientific Research Laboratory he spent some of his time also on this subject. Between this period in the 60's and the time of his death in 1989, barring a few solitary diversions, he spent his time almost entirely on writing on this subject.

Orowan picks up his theme from the fourteenth century Tunisian Arab historian Ibn-Khaldun who studied in some detail the rise, maturation and senescence of successive North African tribes from lean dynamic beginnings to rich and decadent ends, when they are replaced by a new wave of dynamic invaders and so on. Identifying these cycles as *surges* Orowan finds many parallels to these in modern Western societies where however the role of economics becomes of central importance. Tracing the evolution of thought on the modern Western economies from the early trend setters of Adam Smith and Malthus, Orowan identifies one of the fundamental causes of the present weakness of the economic structure of advanced Western Societies to lie in the problem of overproduction resulting from ever increasing productivity which replaces the old crafts that employed many highly skilled craftsmen with automated industries requiring ever fewer people. The attendant problems of chronic unemployment then require establishment of government-charity in the form of production of armaments and establishment of government contracts for work and research not needed by society. In his writings, on this subject Orowan engages in many interesting but inessential diversions on historical facts and anecdotes for the purpose of focusing on the failings of many of the well known economists, historians and social scientists of recent times—making fascinating reading but too often diverting the attention of the reader from the main thesis. His daughter Susan Martin is considering publishing one or more special summaries of this work.

In his writings on the subjects of higher education and professional life Orowan advocated more attention on development of creativity and permitting creative people to continue in their field of expertise as individual professionals rather than directing them too early into management positions. (66).

OTHER ACTIVITIES

Throughout his professional life, and particularly during that part in the U.S.A. Orowan filed many patent applications on inventions occurring to him in the course of his research in his consulting practice and in private life. One of the few patents actually granted (U.S. Patent No. 3,100,488) (67) is for an ileostomy appliance that introduced substantial improvements over models existing on the market at that time, which often created severe problems of secondary skin infections and extreme discomfort for its wearers. This appliance that has been described in the literature (68) has made major impact on the quality of life of all the users who could be equipped with it from pilot production. There are numerous letters in the Orowan collection, in the MIT Archives, from exceedingly grateful patients, attesting to the success of the appliance. Unfortunately, an attempt to commercialize the appliance through the formation of a specialized company did not succeed.

Between the mid 50's, and until 1980 Orowan has had many consulting arrangements with industry. Two of these, with Pittsburgh Plate Glass Company and with E.I. du Pont de Nemours were of more major type and of longer duration. From the extensive notes left behind by Orowan and from statements made by scientists in these companies it is clear that he was a very effective industrial consultant. Throughout his life Orowan maintained a keen interest in all modern social and technological developments and often commented on major happenings through letters to editors. These include comments on the real causes of the disastrous Scott expedition to Antarctica; effect of possible valve malfunction due to a design inadequacy in the Three Mile Island nuclear power station accident in 1979; and a particularly detailed back and forth correspondence with the presidential commission investigating the ill-fated Challenger Shuttle disaster of 1986, on the possible "real" cause of it which Orowan thought was due to a short-transverse brittleness in the main rocket casing, bringing in eventually into the debate a sizable group of NASA scientists and Senator Patrick Moynihan, the Senate overseer of the commission.

During his years in the U.S.A. Orowan continued to receive honors and awards that were, however, more in recognition of his earlier work in Europe and less for his activities in the U.S.A. These included membership in the American Academy of Arts and Sciences (1951), the U.S. National Academy of Science (1969), corresponding membership in the Göttingen Academy of Sciences (1972), an honorary doctor of engineering degree from the Technical University of Berlin (his former alma mater) (1965), the Eugene Bingham medal of the American Society of Rheology (1959), the Gauss medal of the Braunschweiger Wissenschaftliche Gesellschaft (1968), the Paul Bergse Medal of the Danish Metallurgical Society (1973), the Acta Metallurgica gold medal (1985), and the Vincent Bendix gold medal of the American Society of Engineering Education (1971).

In his professional activities (and his intellectual hobbies) Orowan went for the unusual and hidden explana tions of things, often drawing support from forgotten historical facts, and clever anecdotal quotations or overlooked aspects of phenomena, partly for their shock value and partly to impress. His early education in engineering and his eventual practice as an applied physicist (or chemist, or metallurgist) was both a source of strength in his professional life and a cause of a split personality. He lectured to engineers on the merits of the scientist's approach and to the scientists that of the engineer. While he criticized some scientists of whom he did not approve as ". . . Oh well! he was only an engineer . . .", he took great pride in associating himself with prominent chief engineers.

FAMILY LIFE AND PERSONAL CHARACTERISTICS

His daughter Susan has written "My mother was probably his best friend, although I don't think they realized it until the last couple of years. He looked after her full-time at home from 1984 until she went to a nursing home in 1986. . . . When she died, in October 1986, I think he started to believe that he didn't want to live any more". He had many other friends. Susan remembers Peierls, Shoenberg, Dirac, Perutz, Bragg and Besicovich from England, and especially Laszlo Tisza and Leo Gross in America. Susan, after majoring in languages at Tufts, had a distinguished career in librarianship, and is presently University Librarian at Georgetown University. Her husband, Dr. David Martin, is Dean of the School of Education at Gallaudet University, D.C.

Susan's memories of Egon's private life are very clear. . . . "He liked plants, flowers, trees. He and Laci (Tisza) used to go to Jamaica Plain to the botanical gardens as one of their favourite outings". David Tabor has noted similarly (R17): "One aspect of life in the U S A that he prized above many others was the size, scale and openness of the American

National Parks". Susan continues: "Modern culture, though, and particularly 'American' things: popular music, foods, recent clothing fashions, and computers were among the things he looked down on".

Many of the comments at the MIT Memorial Service are revealing. Ali Argon learned from Orowan: "Problems had to be solved completely. . . . Your first reaction should be extreme skepticism, including of course on your own work". This skepticism could sometimes be expressed almost too forcibly. Tabor recalls: "His powerful intellect was most evident at the seminars or lectures that he attended. If a particular point caught his interest he would continuously interrupt... Experienced lecturers could take these interruptions with good humour and sometimes with enjoyment, but junior speakers would often feel frustrated. . . . Orowan, who in private life, as David Shoenberg recounts, was a warm and friendly person, appeared only to understand the intellectual aspect of his interjections, and to be quite oblivious of their effect on the lecturer and the audience". In fact in private life he could be almost excessively gentle. Peierls recalls (R15): "He had strong views about most matters and about people, which he would express clearly, but always with a veneer of politeness. We [Peierls and his wife] used to tease him about this, and once introduced him to a woman who was not only unattractive to look at, but also with a very unpleasant manner, and rather boring conversation. We wondered how he would express his comments about her. When asked, he said 'She is very cerebral (durchgeistig)'." There was another "very dark spot" in Orowan's "plasticitycareer", which throws an interesting light both on his thinking and on that of the Royal Society. He submitted a paper "Mechanics of continental drift" (and probably a companion paper) to the Royal Society. It did not meet with approval from the referees, but the Royal Society does not lightly reject the work of a senior and distinguished Fellow, and it went to further referees. Sir Charles Frank wrote (R18) "I was involved as third or fourth referee". Orowan's approach from first principles could lead him to neglect the work of others, and Frank found "extensive recitation of ideas . . . without due reference to past work . . . and a degree of ignorance of the 'plate-tectonics' revolution . . . I tried (unsuccessfully) to get him to salvage from it a shorter paper". Orowan ceased to pay his subscription to the Royal Society, which leads automatically to expulsion. Mr. Neville le Grand, who was Finance Officer of the Royal Society at the time, has explained (R19). "On the first occasion of Orowan's lapse, I wrote the usual reminder letters but we then realized the possible reason for this. So far as I can recall I spoke with [Sir] David [Martin] (and I believe Flect or Menter whoever was the Treasurer at the time) and we decided on a somewhat phony ground to make the transfer from one of the R.S.'s own funds to cover subsequent payments".

There are many stories illustrating Egon Orowan's approach to life. Sir Alan Cottrell recalls (R20) one he told of his student days in Berlin:—

> Sometime, in the 1930's, the German education authorities changed the rules for matriculation, which required all candidates thereafter to pass an examination in the physical sciences. This set a problem for a nearby convent, where the nuns made a modest income by teaching. None of them knew any science, of course. And so, one of them was sent to Orowan to 'learn physics'. This caused difficulties both for Orowan and the unfortunate one so chosen, due to the vast chasm between the religious and scientific outlooks. After one long session, Orowan finally felt that he was breaking through, on the subject of atmospheric pressure. And so he pointed to a barometer on the wall and said 'tell me, why does the mercury, in that, stay up?' She thought for a moment and said, in a perfect demonstration of the dogmatic approach, 'Oh, because it is a barometer'.

His daughter Susan has many memories (R3):—"He was a highly skilled amateur photographer. . . . He particularly liked clouds, and had a terrible fondness for obese people, which always embarrassed me". At the MIT ceremony she reminded the audience of some of Orowan's characteristic phrases: "It's very simple", and "I understood it a month ago, but now. . ." She also quoted one of his more intimate remarks: "Let me close by telling you that in my early teens I was at one stage terrified by the thought of dying. I confided this in Daddy who immediately resolved the problem, the business about the essence of the simplicity. He said, 'Do you remember what it was like before you were born?', and I said, 'No' And he said, 'Well, that's what it'll be like after you die'."

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Efraim Racker

EFRAIM RACKER

June 28, 1913-September 9, 1991

BY GOTTFRIED SCHATZ

WHEN HE ENTERED OUR Vienna laboratory on a hot summer day in 1961 I was struck by his youthful stride that belied his white hair, his foreign-looking bow tie, and a curious tension in his face. My friends told me later that I had just seen Efraim Racker, one of the foremost biochemists of our time, and that this was his first visit to Vienna since he had fled this city more than twenty-three years ago.

In 1961 Racker's work on biological ATP production had made him one of the stars of bioenergetics, the branch of biochemistry dealing with energy conversion by living cells. I had already made up my mind that I wanted to do my postdoctoral work with him and asked him the next day whether he would accept me. He drew me aside to quiz me about my work, but interrupted me after my first few sentences by asking, "How come you speak English so well?" Flattered, I explained in my German-accented English that I had spent my last year of high school as an exchange student in the United States. His immediate riposte, "How come you speak English so badly?" made us both laugh and started a lifelong bond between us that was severed only when he died thirty years later.

Racker was born on June 28, 1913, in the town of Neu-Sandez in Poland to Jewish parents who moved to Vienna before his second birthday. His family was not wealthy, and settled in the second district of Vienna called Leopoldstadt, then largely an enclave of recent immigrants from the eastern reaches of the Austro-Hungarian Empire and beyond. Most of these immigrants were poor Jews who had hoped for a better life, but then faced anti-semitism, the deprivations of the First World War, the social unrest that followed the collapse of the empire in 1918, and the rise of fascism that culminated in the Nazi takeover of Austria in 1938. During these stormy years Racker attended elementary school and high school whose formal atmosphere did not appeal to him at all. He much preferred playing soccer or chess in the Augarten, the local park. Even as an old man he loved competitive sports such as soccer, Ping-Pong or tennis and was an excellent chess player, but had only vague notions about the usual staple of classical schooling such as the names of Greek goddesses or the Habsburg family tree; however, to the end of his life he retained an intimate knowledge of the literature, the music, and the art of the Vienna he had known. Young Efraim was fascinated by the public lectures of the writer and critic Karl Kraus, knew many of the local musicians, and was profoundly influenced by the paintings of Egon Schiele. For his twelfth birthday one of his aunts gave him a painting set which soon accompanied him on his regular forays into Augarten and started his lifelong passion for painting. Encouraged by the painter and art educator Victor Löwenfeld, who had become his artistic mentor, Racker initially decided to become a painter. After finishing high school he passed the highly competitive admission examination to the Vienna Academy of Art, but was once again turned off by the rigid and formal style of training he encountered. He soon left the academy to study medicine at the University of Vienna.

In the early thirties the fame of Vienna's Medical School had largely faded, and quite a few of the professors and students belonged to right-wing student organizations called *Burschenschaften.* Many of these *Burschenschaftler* later joined the Nazi movement. I got to know many of them during my own student days in postwar Austria. They were then aging but unrepenting, and I shudder when picturing Racker among them, for it was easy to see that he was Jewish. How many hidden scars did these years leave? Perhaps they explain why Racker always had such a strong gut reaction against power, arrogance, and pompousness. This trait made him a great chairman and scientific adviser, but sometimes a difficult adversary of deans, provosts, and other members of official hierarchies.

Although disappointed by medical school, Racker was captivated by the discoveries of Sigmund Freud whose home and office in Berggasse 16 he passed on his daily walk to the university. Had Racker remained in Vienna, he would probably have become a psychiatrist. Indeed, his older brother Heinrich, who later fled to South America, was a psychoanalyst. The two brothers were very close and Racker often told me of their many late-night discussions on Freud, Adler, and their often high-strung disciples. Racker would have made an excellent psychiatrist; many of his postdoctoral fellows (including myself) had good reason to beware of his uncanny ability to read other people's minds; however, fate decided that his interest in the workings of the human mind should lead him to study brain metabolism and, later on, biological ATP production.

Racker's graduation from medical school almost coincided with Hitler's march into Austria. Racker wisely decided to leave while this was still possible and fled via Denmark to Great Britain where the biochemist J. Hirsh Quastel offered him a job at Cardiff City Mental Hospital in Wales. In this rather remote place the two of them tried to detect biochemical defects that could explain the mental abnormalities of their patients. Racker apparently decided to tackle this problem from the bottom up, for his first publication was humbly entitled "Histidine detection and estimation in urine";[1] however, there was nothing humble about the opening sentence: "During the last 10 years, many attempts have been made to detect changes in the liver metabolism of patients suffering from mental disorders." The inexperienced M.D. from Vienna clearly had set his aims high! He also studied the effect of oxygen deprivation on the metabolism of tissue slices, but quickly realized that his hopes of finding biochemical causes for mental diseases were doomed since too little was known on the metabolism of normal cells.

But once again Racker's scientific career was buffeted by the political gales that started to blow across Europe. When Great Britain entered the war Racker suddenly found himself an enemy alien whose experiments with human urine near the strategically sensitive coast posed a security risk. He lost his job at Cardiff and, together with many other refugees from Nazi Germany, was interned on the Isle of Man, where he practiced medicine for the first time in his life. Although he enjoyed being a "real doctor," he soon decided to try his luck as a researcher in the United States, not knowing that he was embarking on a twenty-five year odyssey. He started out doing a brief stint as a research associate in the Physiology Department of the University of Minnesota at Minneapolis (1941-42), but then once again worked as a physician in New York City's Harlem Hospital (1942-44). His career as a biochemist started in earnest only in 1944 with his appointment as staff member in the Microbiology Department of New York University Medical School. He often spoke with great fondness of this department and the great scientific debt he owed to several of its members, particularly to Severo Ochoa and Colin MacLeod, the department chairman. It was during the eight years in this department that he finally became a professional biochemist. Still, when he was offered the position of associate professor at Yale Medical School in 1952, he accepted and moved to New Haven. In 1954 his odyssey seemed to end when he accepted the position of chief of the Nutrition and Physiology Department at the Public Health Research Institute of the City of New York. Little did our modern Odysseus know that Manhattan would be but another way station in his wanderings and that he would reach his Ithaca only twelve years later.

It was at the Public Health Research Institute that I joined him as a postdoctoral fellow in the summer of 1964, and I will never forget my shock when I first saw this Mecca of bioenergetics. The institute was a decrepit and grimy building wedged between a run-down police garage and a coal-fired power plant whose dusty emissions darkened the window panes and settled into every crevice of the laboratory. The address "Foot of East 16th Street" should have warned me; it certainly warned cab drivers who often refused to go there. The laboratories had most of the required equipment, but many of the instruments were old and not well maintained. Cockroaches, many of ghastly dimensions, were everywhere, as floating corpses in buffer solutions, uninvited guests in lunch boxes, or electrocuted culprits in short-circuited electric equipment.

Yet I had one of the most exciting and productive years of my life in that building. It was brimming with talented, motivated people from all over the world, and "our" fourth floor appeared to have the best of them. There were Racker's trusted colleagues Maynard Pullman, Ray Wu, and Harvey Penefsky, all of them already well known in their own right.

Many of the postdoctoral fellows at that time, such as Ron Butow, Yasuo Kagawa, Howard Zalkin, and Richard McCarty later went on to distinguished careers. Two irreverent graduate students, Peter Hinkle and Gladys Monroy, loved to disagree with Racker and injected spice into our lunch discussion. Lunches were taken together in what was grandly called a lunch room and usually consisted of a homemade sandwich and, for Racker, a small can of Hawaiian fruit punch, a mysterious concoction fortunately restricted to North America. There were visiting professors such as Michael Schramm. And there was Racker himself, "Ef" to his senior colleagues. At that time he did not yet invite postdocs to address him by that nickname, but I did so anyway and he did not seem to mind. Having just turned fifty-one, Ef was a splendid leader, full of vigor, wit, enthusiasm, and self-confidence. Aided by one or two technicians, he managed to work at the bench nearly every day, yet keep all of us under close surveillance. Here in his lab he was not tense at all; he was outgoing, relaxed, and clearly aware of the fact that he was at the height of his scientific powers. And there certainly was no trace of a bow tie. In fact, the best that could be said of his dress was that it matched the building. Coming to work on Saturdays was de rigueur; offenders were received on Monday morning with a frosty "How was your weekend?" and usually sinned no more. In order to avoid rush hour traffic, work started at about 10:00 a.m. and went on until 7:00 p.m. or longer. We were a proud and happy crowd, but we also understood why Ef had his detractors. He either liked you or he did not. If he did not, his quick mind and sharp tongue could leave long-lasting wounds. We learned that doing science was not only joyful exploration, but also a game of intellectual domination and that Ef played that game well. Maynard Pullman was a welcome support when Ef's presence or impatience became too overpowering. Maynard was universally respected for his discoveries in the field of oxidative phosphorylation and well liked for his warmth and balanced judgement. He could stand up to Ef for us, and often did. Harvey Penefsky kept more to himself, but was much in demand for precise scientific information and critical discussion.

Despite his gruelling schedule Ef always seemed to have time. He never closed the door to his office; he never secluded himself in the library to read the latest journals; and he never used his hours at the institute for working on a manuscript or the book he was then writing.[1] Yet he answered every letter and rarely missed deadlines. But, as if that were not enough, he also spent much of his time at home painting, producing brilliant acrylics, which he then gave to his scientific friends and collaborators. In later years he also sold his paintings for the benefit of the Edsall Fund, which he had set up to aid needy students. Although few of his friends knew this, he was also a voracious reader who over the years amassed an extensive and varied personal library. He knew how to organize his time. He never wasted a minute. *Gemütlichkeit* was not for him.

I spent my first postdoctoral weeks in 1964 reading most of Ef's previous publications. Upon moving to Minnesota in 1941 he had continued his search for a biochemical basis of brain diseases by studying the effect of a polio virus infection on glycolysis in mouse brain. Right away, an exciting result: the virus inhibited glycolysis. After his first move to New York City in 1942 more excitement: one could also inhibit glycolysis by adding a purified preparation of another neurotropic virus directly to the brain homogenate. And then disappointment: the inhibition was caused by iron which contaminated the virus preparations. [3] At this point most others would have given up. Indeed, the discovery of this artifact called into question Ef's previous papers on this topic and might well have stopped his scientific career before it had ever taken off. But Ef's ingenuity converted this defeat into his first scientific triumph. Undeterred, he went on to show that the inhibition could be overcome by glutathione, a ubiquitous cysteine-containing tripeptide whose role in metabolism was still poorly understood; however, there was good evidence that glutathione was an essential cofactor of the enzyme glyoxylase which converts glyoxal to glycolic acid. When Ef showed that this reaction proceeded through a carboxyl-S-glutathione intermediate he had identified the first "energy-rich" thioester of biological relevance. Glyoxylase was a rather esoteric enzyme, but this could not be said of the glycolytic enzyme triose-phosphate dehydrogenase which resembled glyoxylase in its sensitivity to compounds reacting with sulfhydryl groups. Could it be that triose-phosphate dehydrogenase worked through a similar thioester intermediate? The enzyme catalyzes the energy-yielding oxidation of an aldehyde (glyceraldehyde-3-phosphate) to a carboxylic acid and couples it to the energy-requiring formation of 1,3-diphosphoglyceric acid with inorganic phosphate as phosphoryl donor. Warburg had already proposed that this reaction proceeded by direct addition of inorganic phosphate to the aldehyde group and subsequent oxidation of the adduct to 1,3-diphosphoglyceric acid. Undeterred by Warburg's authority, Ef and his technician Isidore Krimsky showed convincingly that the aldehyde group reacted first with an enzyme-bound sulfhydryl group, that the resulting thio-hemiacetal was then oxidized to an energy-rich thioester, and that this thioester was "phosphorylyzed" by inorganic phosphate to 1,3-diphosphoglyceric acid. Warburg first scoffed at what he called "Racker's Umweg" (Racker's detour), but later had to concede that nature took the Umweg rather than the direct route. Although the reactive sulfhydryl group was later shown to belong to the enzyme itself rather than to tightly bound glutathione, the elucidation of the mechanism by which a biological oxidation is coupled to ATP formation still ranks as one of the most important biochemical discoveries of all time. The simplicity of this reaction was so persuasive that for the next twenty years most biochemists were convinced that oxidative phosphorylation in mitochondria and bacteria had to obey the same principle. How wrong they were!

After moving to Yale in 1952 Ef continued his work on carbohydrate metabolism. He discovered and purified transketolase, a key enzyme of the pentose phosphate pathway. This finding, together with work by others such as Bernhard L. Horecker, eventually led to a detailed description of the entire pathway.

When Ef returned from Yale to New York City in 1954 in order to join the Public Health Research Institute, he first continued to work on the mechanism of glycolysis and the pentose phosphate pathway. By then most of the steps of glycolysis were known, but there was disagreement on how the process was regulated. Why was glycolysis of intact cells inhibited by respiration ("Pasteur effect")? Why did glycolysis inhibit respiration ("Crabtree effect")? And why did most tumor cells, unlike normal cells, convert glucose to lactate even under aerobic conditions ("aerobic glycolysis")? The answers could only come from work with reconstituted in vitro systems. Together with Ray Wu and Shimon Gatt, Ef investigated the effect of respiring mitochondria, nucleotides and specific inhibitors on glycolysis catalyzed by a cytosolic extract from Ehrlich ascites tumor cells and showed that glycolysis was dependent on the continuous regeneration of ADP and inorganic phosphate by an ATPase. The family of glycolytic enzymes thus included an ATPase, but these in vitro systems could not tell which of the many cellular ATPases was responsible for the regulation in intact cells.

This important finding did not receive the attention it deserved, probably because biochemists were then mesmerized by allostery and preferred to place the burden of glycolytic control solely on the shoulders of phosphofructokinase. Even today we do not fully understand how glycolysis is controlled in living cells and why most tumor cells exhibit aerobic glycolysis. Ef and Ray stopped working on this problem in the mid-sixties, but Ef returned to it during his final years.

I was always captivated by Ray Wu's quiet charm and professionalism. Watching him set up his complex enzyme systems with intense concentration taught me much about how to do a successful experiment. Wu rarely spoke up or contributed jokes during our lively lab discussions, but was a great help for us postdocs and a close and trusted friend to Ef.

Ef's return to New York City from Yale in 1954 had also not dimmed his interest in the pentose phosphate pathway. Together with Dan Couri he reconstituted the pathway from purified components and showed that the reconstituted system catalyzed the complete oxidation of glucose-6-phosphate. Together with June Fessenden and others Ef also continued to investigate the detailed mechanism of several enzymes of this pathway.

Soon after Ef had moved to the Public Health Research Institute Maynard Pullman joined his department. Pullman had set his sights high: he wanted to go after the Holy Grail of bioenergetics, the mechanism of ATP synthesis in mitochondria and chloroplasts. For a start, he decided to isolate the mitochondrial enzymes which coupled the oxidation of nutrients to the synthesis of ATP from ADP and inorganic phosphate. Pullman was aware that this was a formidable undertaking and must have been very pleased when the gifted Harvey Penefsky joined him as a graduate student.

Following a procedure pioneered by David Green, they obtained fresh bovine hearts from a nearby slaughterhouse, disrupted them in a mechanical blender, and isolated from the resulting homogenate several grams of mitochondrial membrane fragments which still catalyzed oxidative phosphorylation. They wanted to use these "submitochondrial particles" as their starting material for resolving, and ultimately reconstituting the individual enzymes of respirationdriven ATP synthesis. Both knew that David Green at the University of Wisconsin, Paul Boyer at the University of Minnesota, and Albert Lehninger at Johns Hopkins University were hot on their trail, and the race was on.

It was a long and frustrating race for several reasons. First, the structure of biological membranes was at that time unknown. Second, nobody knew how to assess the purity of a hydrophobic protein since SDS-polyacrylamide gels did not appear on the scene until 1967. Third, most biochemists assumed that ATP synthesis was coupled to respiration through a "high-energy" intermediate of the type that functions in glycolytic ATP production. This intermediate (fondly called x-squiggle-y) was avidly sought, but never found. By the mid-sixties the many futile attempts had led to frustration and heated controversies; however, most outsiders have later painted an exaggerated picture of the situation, perhaps because the mercurial temperament and acid humor of some of the leading mitochondriacs did not appeal to everyone. By today's standards relationships between the competing laboratories remained civilized; experimental discrepancies were usually resolved by joint experiments and I cannot recall any instance where a laboratory withheld requested reagents or information from a competitor.

We postdocs loved to gossip about the relationships between the key players. Ef seemed to be closest to Britton Chance whose brilliance, boyish temperament, and experi mental skill matched his own. He genuinely liked Paul Boyer, Henry Lardy, Albert Lehninger, Bill Slater, and Lars Ernster and respected their scientific rigor and masterful grasp of biochemistry, but in the mid-sixties none of them worked directly on the resolution of oxidative phosphorylation and scientific interactions were less frequent. Our gossip usually focused on David Green and his group. Green was imaginative, self-assured, flamboyant, and quick with tongue. He usually disagreed with Ef, easily matched him as a debater, and was Ef's perennial foil. When Peter Mitchell emerged as a major figure in the field several years later, he and Ef developed a friendly rapport, yet it seemed to me that they were never quite at ease with each other.

But I am getting ahead of my story. At first, progress in Ef's department was amazingly fast. In order to solubilize the enzymes coupling respiration to ATP synthesis, the submitochondrial particles were vigorously shaken with tiny glass beads in a shaker originally described by the Australian biochemist Peter M. Nossal. Mike Kandrach, our gifted and eccentric mechanic, soon built a monstrous U.S. version which he himself considered so dangerous that he screwed it to the floor of a separate room, operated it by remote control, and allowed nobody else to touch it. This contraption emitted a lugubrious rumble that shook the building and would have sent any contemporary Californian diving for the nearest earthquake shelter. When these tortured mitochondrial fragments were sedimented in an ultracentrifuge they still respired, but no longer synthesized ATP. Adding back the supernatant from the centrifugation sometimes restored ATP synthesis, as Pullman and Penefsky had hoped, but this effect was quite irreproducible, particularly after the first few attempted purification steps. The project seemed to be stuck; but, being the master biochemist that he was, Pullman systematically varied his experimental pro tocols until he found that the partially purified soluble fractions restored ATP synthesis nearly every time if they were stored at room temperature. In order to protect the garden of oxidative phosphorylation from unworthy intruders God had guarded the entrance by a cold-labile enzyme! But the assay was still very tedious until Pullman began to suspect that the cleavage of ATP which was catalyzed by the soluble fraction might be just another activity of the mysterious factor that restored oxidative phosphorylation. He started to monitor purification by assaying ATPase activity which was much faster and easier than assaying restoration of oxidative phosphorylation and from then on progress was fast: when he and Penefsky purified the ATPase from the supernatant it was indeed identical with the "coupling factor" of oxidative phosphorylation. The splitting of ATP in a test tube was clearly a reversal of the reaction which the enzyme catalyzed in living cells. Since the ATPase was the first defined factor that coupled respiration to ATP synthesis it was named Factor 1, or F_1. The first enzyme of oxidative phosphorylation had been identified and purified!

This fundamental discovery was published in 1960. It was greeted with universal admiration, and hopes were high that the enzymology of oxidative phosphorylation would soon be understood. In a letter he wrote me to Vienna early in 1963 Ef suggested various topics for my upcoming postdoctoral stay with him. He discouraged me from planning to work on oxidative phosphorylation with the words "Progress on this front is now quite fast and by next year our interests may have shifted to other topics." In the following years of slow progress I could always enliven my seminars by showing a slide with this sentence in Ef's own handwriting.

But for a while Ef's prophesy seemed to be correct. Together with Vida Vambutas he purified a closely similar cou pling factor from spinach chloroplasts. The purified chloroplast F_1 (termed CF_1) restored light-driven ATP synthesis to EDTA-treated chloroplast fragments, but did not cleave ATP unless it was gently treated with trypsin. This result confirmed the general expectation that photophosphorylation and oxidative phosphorylation functioned by a similar mechanism. In experiments that he first did himself and later together with Yasuo Kagawa, a gifted and hardworking postdoctoral fellow from Japan, Ef subfractionated submitochondrial particles with cholate and salt and identified a membrane factor that anchored F_1 to the membrane and rendered it cold-stable and sensitive to the toxic antibiotic oligomycin. As oligomycin was then considered the most specific inhibitor of oxidative phosphorylation its lack of effect on the ATPase activity of F_1 had been the most serious argument against a role of F_1 in oxidative phosphorylation. The identification of a factor conferring oligomycin sensitivity on soluble F_1 not only silenced this criticism, but also paved the way towards resolving the membrane-embedded components of the oxidative phosphorylation machinery. Kagawa and Ef named their insoluble F_1-binding factor F_o (in contrast to what is generally thought the subscript does not signify zero, but the letter o for oligomycin).

Kagawa presented his results to a packed audience at the 1965 Annual Federation Meeting in Atlantic City. Although he had been intensively coached by some of the American postdoctoral fellows in the lab he struggled with the sounds and the grammar of the English language. He had particular difficulty pronouncing "F_o", making it sound like the exhortation "Ef, ho!" Still, all the experts in the audience realized that they were witnessing a landmark presentation. The response in Japan must have been similarly positive because Kagawa was soon offered an attractive full professorship at Jichi University where he still works today.

Ef's work with Kagawa also showed that the characteristic knobs which lined the inner face of the mitochondrial inner membrane in electron micrographs were, in fact, F_1: the knobs disappeared when F_1 was stripped off, and reappeared when purified F_1 was added back. At that time this was perhaps the clearest evidence for the molecular asymmetry of a biological membrane.

Buoyed by these advances, Ef's oxidative phosphorylation team now tried to isolate additional protein factors by mistreating phosphorylating submitochondrial particles in various ways. The particles obtained with our Nossal-type glass bead shaker were termed N-particles. Treatment with ammonia solution yielded A-particles, intensive sonication S-particles, and treatment with phospholipids P-particles. All these particles were defective in coupling respiration to ATP synthesis and could be partly reconstituted by adding back other mitochondrial protein fractions which were termed F_2, F_3, and F_4. However, these fractions elicited only marginal effects and the effect were often irreproducible. Today we know that these fractions were impure and cross-contaminated with each other, that the factors under study had not always been completely stripped from the test particles, and that the non-linear response of the particles to added factors led many a hapless postdoc astray. Also, all efforts to reconstitute oxidative phosphorylation from completely solubilized submitochondrial particles proved unsuccessful.

By the beginning of 1966 the optimism in Ef's lab had faded somewhat and several of his postdoctoral fellows decided to chose other fields when they started their own laboratories elsewhere. As so often in a scientific career this lull coincided with a wave of official recognition. In the spring of 1966 Ef was elected into our National Academy of Sciences. A few months later Robert Holley (who was soon to receive the Nobel Prize for his work on the structure of tRNA) and Robert Morison from Cornell University in Ithaca, New York, visited Ef and tried to persuade him to help create and lead the biochemistry department of a new biology unit at that campus. The innovative concept of this plan had received a generous grant from the Ford Foundation, and Ef's own position was to be endowed by one of the prestigious Einstein professorships through which the State of New York hoped to attract outstanding scholars to its new university system.

Ef had of course received offers before, but this one had the right ring. It also came at just the right time. Although Ef had grown up in Vienna he was never a "city type" and was beginning to loathe the inconvenience of working in New York City. In order to give his family the peace it needed he lived in faraway Mount Vernon and the daily commuting by car was proving to be a burden. The challenge of creating a new research unit may have also excited him, but I suspect that his decision was really swung by his wife Franziska. He had known Franzi (Frances to most Americans) in Vienna where she received her M.D. at about the same time as he. Born to an established Viennese family who lived in the rather fashionable ninth district, she, too, had emigrated via Great Britain to the United States. She had just obtained an advanced degree in public health at Harvard Medical School when Ef and she met again in the New World. They married in 1945. Intelligent, musical, practical, and charming, she was everybody's favorite and an adopted grandmother for many of the postdocs' children. She was then, and still is, an active and successful physician. As ambitious and strong-willed as Ef himself, she was a perfect companion for him by showing him unflinching devotion while guiding him with a firm and understanding hand. She loves nature and gardening (quite unlike Ef), and was attracted by the prospect of living in the green hills of rural Ithaca and of being closer to her husband and her only child Ann, who was then sixteen years old. After only a brief deliberation Ef decided to take the plunge; in the fall of 1966 he moved his family and most of his laboratory to Ithaca.

The decision proved to be an excellent one. Cornell greeted Ef with open arms and gave him free rein in creating "his" department. His reputation helped him to recruit outstanding senior scientists such Quentin Gibson from Great Britain, Leon Heppel from NIH, André Jagendorf from Johns Hopkins University, as well as his trusted colleagues Harvey Penefsky and Ray Wu. "Young Turks" were added by the arrival of Stuart Edelstein, Peter Hinkle, Richard McCarty, and David Wilson. While all of this was happening I was back in Vienna trying to get reaccustomed to Europe; however, in 1968 I decided to cross the Atlantic once again and join Ef's new department as a faculty member. All of us were housed in Wing Hall which had been refurbished and expanded. Research funds and jobs were still plentiful and our spirits high. We owned the world.

Ef proved to be a smooth administrator who coped effortlessly with the added burden of creating and running a big department. He had a talent for picking capable aides and letting them do things their own way. His teaching obligations were light. Because of his intuitive and idiosyncratic style of thinking he preferred to teach seminar courses, and the students loved him. For the first time in his life his appointment brought him in close contact with undergraduate students and it proved to be an immediate and reciprocal love affair. These intoxicating "hippie years" were hard on parents, but they were great for outgoing and unconventional academic teachers such as Ef. Even his sartorial negligence was a distinct advantage. In spite of his many new commitments Ef continued to work at the bench and to supervise the postdoctoral fellows who now flocked to him in growing numbers. Some of the outstanding young people from this period include Günter Hauska, Richard Huganir, Baruch Kanner, Ladislav Kovác Chris Miller, Maurice Montal, Nathan Nelson, Michael Newman, Jan Rydstrøm, Dennis Stone, Bernie Trumpower, and Charles Yocum. I became particularly close with Nathan Nelson who often reminded me of Ef himself. His amazing productivity was sustained by the help from his smart wife Hannah. Both of them became lifelong friends of Ef and his family.

With such a high-caliber cast the Cornell team soon scored its first successes. These successes were triggered by the team's growing conviction that oxidative phosphorylation was not mediated by a high-energy chemical intermediate, but by a transmembrane proton gradient as Peter Mitchell had proposed a decade ago. Ef's conversion to Mitchell's ideas was triggered by his many discussions with the brilliant Peter Hinkle, who had done postdoctoral work with Mitchell in Great Britain and who now saw it his mission to save Ef's soul by converting him to chemiosmosis.

Ef loved to argue with Hinkle, but in the end his conversion probably came from two experiments in which he participated himself. Prompted by studies done by Hinkle, Ef showed that the oxidation rate of cytochrome *c* by cytochrome oxidase reconstituted into liposomes was controlled by a transmembrane proton gradient. This "respiratory control" closely mimicked that seen with respiring intact mitochondria when ATP synthesis was prevented by lack of the phosphate acceptor, ADP. Second, he and Kagawa (who had returned to Ef's lab for an extended sabbatical) finally succeeded in reconstituting ATP-^{32}Pi exchange, a partial reaction of oxidative phosphorylation, from pure F_1 and solubilized F_o. This activity was only seen when the two components were reconstituted into a sealed liposome and was lost when the liposomes were made leaky to protons.

Perhaps the most famous experiment from this period was done by Ef and Walther Stoeckenius, a German biologist working at the University of California at San Francisco. Stoeckenius had discovered bacteriorhodopsin, a purple chromoprotein from the archaebacterium *Halobacterium halobium* and, together with Dieter Oesterhelt, had found that this protein functioned as a light-driven proton pump in the bacterial plasma membrane. Why not incorporate this simple, pure proton pump into a liposome together with the F_1F_o-ATPase from bovine heart? If Mitchell was right and the two components oriented themselves properly in the liposome membrane (which was the big if), then the protons pumped out by the illuminated bacteriorhodopsin should flow back through the F_1F_o-ATPase and generate ATP from ADP and inorganic phosphate. Stoeckenius visited Ef at Cornell to do this experiment with him, even though neither of them gave it much of a chance. The gods must have been pleased to see two elderly scientists working together at the bench; the experiment worked and convinced even the most obdurate skeptics that Mitchell's hypothesis was correct. Almost two decades after elucidating a key reaction of ATP formation in glycolysis Ef had helped to nail down the mechanism of ATP formation in mitochondria, chloroplasts, and the bacterial plasma membrane.

In the years that followed, Ef and his collaborators reconstituted an astonishing array of different membrane enzymes into liposomes and established reconstitution as a powerful and generally applicable approach for unraveling the mechanism of pumps, transporters, and receptors. Numerous prestigious honors and prizes came his way, such as the Warren Triennial Prize in 1974, the National Medal of Science in 1976, and the Gairdner Award in 1980. But many biochem ists were disappointed when the 1980 Nobel Prize for Chemistry went to Mitchell alone. As usual, the Nobel committee did not divulge its reasoning, but many of the leading biochemists were surprised or even upset by the fact that Ef had not shared the prize. Only Ef himself seemed to be little concerned. He continued to work at the bench as usual, and painted more avidly than ever in his new enlarged studio which had been a birthday present from Franzi. He also enjoyed his new role as grandfather; he was never more at ease than when playing with children.

There was also renewed excitement in the laboratory, for his long and distinguished career seemed to be headed for a triumphant finale. After having been a key figure in unraveling the enzymology of glycolysis, the pentose phosphate pathway, and oxidative phosphorylation, he had returned to the problem that had still defied him: the abnormal glycolysis in tumor cells. His previous studies had convinced him that glycolysis was controlled by cellular ATPases. Could one of these ATPases be hyperactive in tumor cells and thereby cause an abnormally high glycolytic rate?

It seemed like a stroke of luck that just at this time Mark Spector joined him as a graduate student. Intelligent, hardworking, and unusually skilled at the bench, Spector embodied many of the qualities Ef admired in others. A brilliant hypothesis was born and documented by a quick succession of experiments. A tumor gene encodes a protein kinase. This kinase phosphorylates and thereby activates another kinase. This second kinase activates a third, and so on until the last member of this "kinase cascade" phosphorylates a subunit of the ATP-driven sodium-potassium pump in the plasma membrane; phosphorylation of the pump renders it less efficient, increasing its rate of ATP consumption and thereby the rate of glycolysis. But in 1981 it became clear that Spector had fabricated his data,[4] and it could not have happened at a worse time. The United States was just then going through one of its recurring crusades against human evil, and this time the evil was "scientific misconduct." Ef found himself in the center of a storm and it was then that his true stature showed. He immediately published retractions of the questionable papers, withdrew those still in print, and offered to resign from his various committees until the issue had been resolved. He set an example of courage and honesty to us and future scientists. It was no coincidence that the American Society of Biological Chemistry chose this moment to award him their prestigious Sober Memorial Lectureship. To him it must have been a dark time, but to me and many of his friends it was perhaps his finest hour.

It was in one of the difficult years that followed that he spent the only sabbatical of his life in my Basel laboratory. Once again I admired his quick mind, his enthusiasm, his experimental skill, his undimmed scientific curiosity, and his truly astounding openness to new ideas. His books on bioenergetics and the social impact of science were much appreciated in Europe and he loved to discuss them with my students and postdocs. But a long day in the lab left him exhausted, and his private talks with me often touched upon the dark sides of life. His lectures still impressed and captivated his audience even though his deteriorating hearing made it difficult for him to handle the subsequent discussions.

Upon his return to Ithaca his letters became more frequent and unusually warm and personal, but his suddenly erratic handwriting made me worry about his health. On September 6, 1991, he came home from a hard Saturday in the laboratory and was felled by a severe stroke. He never regained consciousness and died in Syracuse three days later.

What kind of man was Efraim Racker? Someone as bril liant, artistic, and intuitive as he will always defy definition. Like many great scientists Ef had several personalities and therefore elicited different responses in different people. He was one of the last great figures of biochemistry's heroic age. He embodied the artistic, even romantic approach to a field of science that has become increasingly dominated by organized collective efforts. He could be egocentric, insensitive, even overbearing; but, those who knew him well will cherish the memory of his warmth, his immense intellectual range, his lack of prejudice, and his unshakable belief in the power of human reason and inventiveness. Once you were his friend, he was always for you and forgave you everything. Few knew that he was sometimes haunted by depressions which showed in the many leafless trees of his paintings and his never-ending fascination with the secrets of the human mind.

Perhaps one would only understand the many layers of Ef's character if one could retrace his formative years in the Vienna of his youth. But the world of his youth has been brutally shattered forever, and few are still with us to tell about it. Ef rarely talked about that time and maintained only infrequent contacts with relatives outside his immediate family. Upon his emigration he had even tried, with some success, to forget the German language and tended to stammer when being addressed in that language.

But every human life is mystery and best weighed by its influence on others. Efraim Racker showed me that scientific exploration and art are but two manifestations of the same powerful spirit that makes us human, brings us joy, and gives us wings.

MANY FRIENDS AND COLLEAGUES have helped me retrace Ef's private and scientific life and improve this brief account. My special thanks go to Franziska Racker, Judy Caveney,

Peter Hinkle, Harvey Penefsky, Maynard Pullman, Ray Wu, Carolyn Suzuki, Andreas Matouschek, and Dennis Stone.

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Courtesy of Caryl Roman

Herschel L Roman

HERSCHEL L. ROMAN

September 29, 1914-July 2, 1989

BY MICHAEL S. ESPOSITO

HERSCHEL L. ROMAN, PROFESSOR emeritus and founding chairperson of the Department of Genetics at the University of Washington, made fundamental contributions to studies of the nature of the gene and chromosome behavior in maize during the early phase of his career. Later he led the emergence of *Saccharomyces cerevisiae,* budding yeast, as a premier unicellular organism for study of the basic genetics of eukaryotes. Hersch, as he preferred to be called, was a brilliant researcher, an inspired teacher, and a stalwart colleague of those who shared his love for genetic experimentation and his commitment to the welfare of genetic biology.

An innovator of pace-setting tools for genetic analysis, Hersch was the recipient of numerous distinguished national and international honors in addition to his election to the National Academy of Sciences: Guggenheim fellow, Paris; Fulbright research scholar, Paris; president, Genetics Society of America; American Academy of Arts and Sciences; Gold Medal, Christian Hansen Foundation, Copenhagen; Thomas Hunt Morgan Medal, Genetics Society of America; honorary doctorate, University of Paris; Doctor of Science, honoris causa, University of Missouri-Columbia; and presi dent of the International Congress of Yeast Genetics and Molecular Biology. In this latter role Hersch encouraged international research cooperation and collegiality among yeast geneticists for two decades.

As chair of genetics at Seattle, founding editor of the *Annual Review of Genetics,* chair of the NIH Genetics Training Committee, chair of the NIH Research Career Award Committee, member of the NSF Genetic Biology Panel, and consultant to the Biology Division of Oak Ridge National Laboratory, Hersch fostered the growth and dissemination of genetic research for over a quarter century.

Born in the village of Szumsk on the Polish-Russian border on September 29, 1914, Hersch came to the United States with his family in 1921. He spent his youth in the small towns of northern Minnesota and Wisconsin, and received his undergraduate and graduate training at the University of Missouri at Columbia, Missouri. Hersch maintained a special affection for his alma mater which had nourished his interest in science and facilitated his entry into the field of genetics.

Hersch majored in chemistry and received his undergraduate degree in 1936. In his senior year he was employed as an undergraduate assistant by Lewis J. Stadler. Stadler, an eminent maize geneticist, was professor of field crops and senior geneticist for the U.S. Department of Agriculture. Stadler required assistance in making physical measurements and calibrating the large quartz monochrometer used for mutant induction. Hersch's undergraduate training in chemistry and physics suited him to the task. This was the golden era of genetics at Missouri. Stadler and a team of premier researchers that included Daniel Mazia, Barbara McClintock, Joseph O'Mara, Ernest Sears, Luther Smith, and Fred Uber were fruitfully engaged in a variety of genetic studies. McClintock was awarded the Nobel Prize in 1983; the oth ers were of similar stature. Hersch was drawn into the excitement surrounding the group effort to determine the action spectrum of induced mutation in maize and became a graduate student under the tutelage of Stadler. His fellow graduate students included Seymour Fogel and John Laughnan.

Stadler's chief goal was to define the nature of the gene. Hersch participated in the continual refinement of the conceptual and technical tools required. In collaboration with Stadler, he documented the fact that the chromosomal rearrangements and losses of genetic material induced by X rays imposed a serious limitation on the usefulness of X rays for study of point mutations. Few X-ray induced changes could possibly be due to such gene mutations and even these few were suspect. However, ultraviolet light induced changes more closely resembled spontaneous gene mutations. This fundamental observation suggested that one might obtain information about the chemical composition of the gene by determining the action spectrum of ultraviolet light mutagenesis. Stadler's group later showed that ultraviolet light of the wavelength absorbed by nucleic acid induced gene mutations most efficiently.

For his doctoral dissertation Hersch undertook an analysis of the inheritance and segregational behavior of the supernumerary heterochromatic B-type chromosomes that occur in many strains of maize. Unlike the standard A-type complement of chromosomes that comprise the maize genome, B-type chromosomes, consistent with their total heterochromatization, are dispensable. Maize strains devoid of B-type chromosomes are indistinguishable from those containing as many as ten. Within maize strains that contain B-type chromosomes, the number of B-type chromosomes varied widely. Cytogenetic studies of the meiotic behavior of B-type chromosomes revealed that they are fragile and frequently degraded during meiosis. This did not explain the great variability in the number of B-type chromosomes from plant to plant. Moreover, there was no evidence of meiotic nondisjunction of B-type chromosomes that might explain the numerical variation in the number of B-type chromosomes per plant.

Controlled breeding experiments between strains that contain no B-type chromosomes and strains containing one or two B-type chromosomes suggested that mitotic nondisjunction of B-type chromosomes occurs at high frequency during one or both of the postmeiotic mitotic divisions that comprise pollen grain formation in maize. Hersch set out to answer three questions: (1) Does mitotic nondisjunction of B-type chromosomes occur during pollen grain formation? (2) Does nondisjunction occur during one or both mitoses? (3) What portion of a B-type chromosome is responsible for the putative mitotic nondisjunction? To answer these questions he devised a critical experiment that came to be regarded as a classic exemplar of maize cytogenetics.

In a particularly aesthetic example of the manner in which a corn kernel serves as a Petri plate for a gifted maize geneticist, Hersch identified the principal features of B-type chromosome biology. Since B-type chromosomes are devoid of marker genes, Hersch constructed eight interchanges (translocations) between A-type and B-type chromosomes that incorporated appropriate dominant genetic markers on the A-type component of the interchange. The translocations were used to determine whether a specific region of a B-type chromosome is responsible for its unusual transmission properties and to monitor the behavior of B-type chromosomes during the two postmeiotic mitotic divisions involved in pollen grain formation. A-B translocations in which an A-type chromosome contributed the cen tromere behaved like normal A-type chromosomes; however, A-B translocations in which a B-type chromosome contributed the centromere behaved like B-type chromosomes. These results indicated that the centromere region of a B-type chromosome is responsible for its peculiar behavior.

The genetic markers incorporated into the A-type components of A-B translocations that contained a B-type centromere affected the color of the kernel and adult plant as well as the texture of the kernel. The markers could therefore be used to detect postmeiotic mitotic nondisjunction and the fate of the resultant nuclei.

Hersch observed that B-type chromosomes undergo mitotic nondisjunction at very high frequency during the second division of the microspore. Consequently, the two gametes of the pollen grain contain two different chromosomal complements. For example, a pollen parent of genotype 0 B/1B, where B is an A-B interchange in which the centromeric component is of the B-type, frequently gives rise to pollen grains in which one of the gametes contains 0 B chromosomes while the other contains 2B chromosomes, due to mitotic nondisjunction during division of the generative nucleus. The 0 B gamete of such pollen grains preferentially unites with the two polar haploid nuclei (seed parent) of the developing kernel to yield the triploid endosperm of the kernel, while the 2B gamete preferentially unites with the egg nucleus of the kernel to yield the plant zygote containing two B-type chromosomes.

Mitotic nondisjunction of B-type chromosomes during pollen grain development coupled with directed fertilization of the seed parent egg nucleus by the hyperhaploid 2B gametic nucleus maintains B-type chromosomes in maize populations. B-type chromosomes, having acquired survival properties that compensate for their meiotic fragility and apparent lack of relevance to the life of their host, were harbingers of the modern concept of selfish DNA.

Life as a graduate student in Stadler's group demanded a total commitment of mental and physical energy to the maize genetics program. While Hersch and his colleagues benefited from Stadler's powers of analysis, warmth, and encouragement, they were also expected to be responsive to Stadler's affirmative and demanding leadership. Hersch was more than equal to the task. Moreover, he enjoyed a close relationship with the Stadler family and forged lifetime friendships during his Columbia days. While a graduate student, he met and married his wife Caryl Kahn. Their first daughter, Linda, was born while Hersch was a graduate student.

Hersch received his Ph.D. degree in 1942 and accepted an academic appointment in botany at the University of Washington. To his dismay, there were neither greenhouse facilities nor acreage on which to grow experimental crops. Moreover, Seattle's maritime climate prevented the proper maturation of maize ears. Hersch's professional activities, like those of so many of his contemporaries, were soon interrupted by World War II. Hersch joined the Army Air Corps and served until 1946. After his military discharge Hersch resumed his maize studies. He was awarded a Gosney Fellowship for three successive summers to participate in the robust maize genetics program headed by Edgar G. Anderson at the California Institute of Technology in Pasadena.

Thereafter, Hersch brought his crops to maturity on distant rented acreage in the hot climate of eastern Washington state. This solution proved impractical; thus, in 1950 Hersch decided to switch to an organism that could be grown in the laboratory. To take advantage of the growing power of microbial genetics he chose the budding yeast *Saccharomyces cerevisiae* because of its unicellular character, availability of both haploid and diploid strains, and ability to undergo both mitosis and meiosis in the unicellular state.

The choice was made with trepidation. The genetics of budding yeast was in its infancy and there was concern that it did not routinely follow normal Mendelian inheritance. This concern was unfounded. Hersch, together with his U.S. and foreign colleagues, provided unambiguous genetic data that showed budding yeast is a unicellular eukaryote whose chromosomes exhibit the expected mitotic and meiotic behavior. Hersch, Howard C. Douglas, and Donald C. Hawthorne comprised the initial yeast genetics team at Seattle.

While Hersch's principal research interests were in the areas of gene mutation, chromosome behavior and recombination, he made insightful contributions to many aspects of yeast genetics throughout his long and distinguished career. He chose research topics with extreme care and only after he was convinced that there was enough information available to design critical and informative experiments. His early studies went to the heart of the concern that budding yeast might not exhibit normal Mendelian inheritance. This concern arose from reports from the Lindegren laboratory of numerous exceptions to normal 2+:2m segregations in tetrads from yeast +/m diploids. Hersch demonstrated that the abnormal meiotic segregation patterns (4+:0m, 3+:1m, 1+:3m, and 0+:4m) reflected, at least in part, the contribution of mitotic recombination before meiosis and the failure to detect the accumulation of polyploid cells in populations of parental strains.

In the early 1950s geneticists were still struggling with the fundamental question, What is a gene? In order to answer this question it was essential to establish experimental tests that could distinguish non-complementing mutant alleles of the same gene (heteroalleles) from mutant alleles of closely linked discrete genes that confer the same phenotype, but fail to complement one another when tested in diploid cells (pseudoalleles). Investigators approached this issue by collecting and characterizing the properties of large collections of independently isolated but phenotypically similar mutants. A view of the gene as a linear array of mutational sites separable by recombination began to emerge, but remained vulnerable to the uncertainties imposed by the concern over pseudoalleles.

Hersch explored the possibility that heteroalleles might be distinguished from pseudoalleles by differences in the way in which they recombine with one another during mitosis and meiosis. In order to carry out experiments he sought a convenient method to obtain large numbers of spontaneous mutations that confer the same phenotype. The difficulty of obtaining sufficient numbers of spontaneous mutations had imposed limitations on related studies in maize. He found that haploid yeast strains that incorporate either an *ade1* or an *ade2* mutation provide a rich source of spontaneous mutations at other genetic loci that control adenine biosynthesis.

Both *ade1* and *ade2* mutants accumulate a red pigment due to the block in adenine biosynthesis and form red colonies on nutrient medium, in contrast to all other yeast strains which form white colonies. Hersch observed that spontaneous mutations of genes that control steps in adenine biosynthesis that precede those controlled by *ade1* and *ade2* block the accumulation of red pigment. When such mutations arise within a population of *ade1* or *ade2* haploids they are readily detected by their restored ability to form white colonies and persistent adenine auxotrophy. (Revertants of *ade1* or *ade2* strains also form white colonies; however, revertants are adenine prototrophs and easily identified as such.) White *adex ade1* and *adex ade2* double mutants have a selective growth advantage over *ade1* and *ade2* red strains and, with time, emerge as white papilla on the surface of red *ade1* or *ade2* haploid colonies.

Hersch was well aware of the value of a color shift system in genetic experiments from his previous studies with maize. He subsequently exploited the red-white system for studies of both gene mutation and recombination. The red-white system remains a valuable tool in modern recombinant DNA yeast research.

Using the red-white system, Hersch initially described five loci controlling adenine biosynthesis, *ADE3, ADE4, ADE5, ADE6,* and *ADE7*. The remaining *ADE* genes (*ADE8-ADE13*) were identified in later investigations. The initial working material consisted of eighty-three mutations at the *ADE3-ADE7* loci and eight mutations at the *ADE1* and *ADE2* loci. Novel *ade* mutants were assigned to the same *ADE* gene by their failure to complement one another's adenine auxotrophy and their ability to complement the adenine auxotrophy of mutants of the other *ADE* genes.

Comparisons of the frequencies with which diploids homozygous for an identical pair of alleles (homoallelic diploids, e.g., *ade3-1/ade3-1*) and diploids heterozygous for mutant alleles of independent origin (heteroallelic diploids, e.g., *ade3-1/ade3-3*) yield adenine prototrophic revertants during mitotic cell division and following meiosis provided indisputable evidence that intragenic recombination occurs principally by non-reciprocal recombination. Homoallelic diploids yielded Ade+ revertants due to gene mutation during mitosis and after meiosis at low frequencies that were essentially indistinguishable from those observed in mitotic cultures of a haploid carrying the same *ade* mutant.

Heteroallelic diploids exhibited two types of behavior. The vast majority (e.g., *ade3-1/ade3-3*) yielded Ade+ revertants following mitosis at frequencies that were at least ten times higher than the frequencies of Ade+ revertants found in control mitotic populations of the two homoallelic diploids incorporating the pertinent *ade* alleles (e.g. *ade3-1/ ade3-1* and *ade3-3/ade3-3*). Hersch correctly concluded that the enhanced yield of Ade+ revertants represented the contribution of mitotic intragenic recombination to the yield of Ade+ revertants. Since chromosome pairing and recombination at high levels occur during meiosis, Hersch also determined the meiotic yield of Ade+ prototrophs of heteroallelic and homoallelic diploids. Following meiosis, heteroallelic diploids typically yielded Ade+ prototrophs at frequencies ten to a thousand times greater than their corresponding mitotic values. In contrast, meiosis did not enhance the yield of Ade+ prototrophs from homoallelic diploids. This result was consistent with the view that intragenic recombination is the principal source of the Ade+ revertants of heteroallelic diploids.

A minority of heteroallelic diploids (e.g., *ade3-1/ade3-2*) yielded Ade+ revertants during mitosis and after meiosis at frequencies that were indistinguishable from those of the pertinent homoallelic diploids (i.e., *ade3-1/ ade3-1* and *ade3-2/ade3-2*). Such experimental data provided an operational definition of mutants occupying the same mutational site (i.e., heteroalleles inseparable by recombination). Hersch noted that the dimension of a mutational site is limited by the resolving power of intragenic recombination; thus, mutants of independent origin that are inseparable by recombination might represent identical mutations or different mutational changes of the same region of a gene.

The frequency of spontaneous gene mutation and the proportion of apparent spontaneous point mutations inseparable by recombination varied coordinately by a factor of ten among *ADE* genes. Hersch suspected that the differences reflected the physical sizes of the *ADE* genes. Eliza beth W. Jones, a fellow graduate student, and I with Hersch's guidance constructed genetic fine structure maps of *ADE3, ADE6,* and *ADE8* that supported this interpretation. Recent DNA sequencing studies of the yeast genome confirm these early findings.

Budding yeast provided an opportunity to compare the properties of mitotic and meiotic heteroallelic recombination. Hersch's initial studies showed that heteroallelic recombination in both mitosis and meiosis results principally from non-reciprocal recombination (i.e., gene conversion). Reciprocal intragenic recombination occurred rarely in both mitotic and meiotic cell populations.

During meiosis of yeast non-reciprocal recombination between heteroalleles occurs in frequent non-random association with orthodox reciprocal recombination of heterozygous markers flanking the heteroallelic locus. During mitosis non-reciprocal recombination between heteroalleles also occurs in non-random association with orthodox reciprocal recombination of heterozygous markers flanking the heteroallelic locus; however, in mitotic cells the extent of nonrandom association is typically less than in meiosis. This difference led Herschel to suspect that non-reciprocal and reciprocal recombination might be separate consequences of chromosomal pairing and genetic exchange. He saw an analogy between gene conversion in yeast and DNA transformation in bacteria (namely, localized uptake of a small segment of donor DNA by a recipient chromosome).

Hersch wished to determine whether mitotic gene conversion and intergenic recombination could be dissociated. Drawing upon his previous experience in the Stadler group, Hersch initiated a series of experiments aimed at determining whether there might be physical or chemical agents that could preferentially induce mitotic non-reciprocal recombination (gene conversion) or reciprocal intergenic re combination of heterozygous markers. Taking advantage of the red-white colony color system, he examined the inductive effects of ultraviolet light, X rays, nitrosoguanidine, and ethylmethane sulfonate upon mitotic gene conversion and reciprocal recombination. Early studies of the effects of ultraviolet light upon mitotic recombination in yeast diploids, performed in collaboration with Francois Jacob at the Institut Pasteur and Satya Kakar, showed that gene conversion and reciprocal recombination of heterozygous markers flanking the site of gene conversion are separable events. Later analyses of the recombinagenic effects of X rays, ethylmethane sulfonate, and nitrosoguanidine led to the same conclusion.

The separation of gene conversion and reciprocal mitotic recombination was not limited to mitotic recombination induced by physical or chemical agents. Hersch, William Boram, John Golin, and I later reported that certain hyper-recombination mutants enhance mitotic gene conversion but have little effect upon reciprocal recombination.

During the early stages of these studies all of the mitotic recombination data could be interpreted on the assumption that mitotic gene conversion and reciprocal recombination occur in G2 (after chromosomal replication), as they do during prophase of meiosis. There was no need to think otherwise; however, the mitotic data demonstrating the separation of gene conversion from reciprocal recombination of heterozygous markers flanking the site of gene conversion could not be easily accommodated by meiotic molecular models of gene conversion and crossing-over. These models had been designed to explain the very frequent association of meiotic gene conversion and reciprocal exchange of heterozygous markers flanking the site of gene conversion. According to the models, formation of regions of heteroduplex DNA involving a donor and a recipient pair of non-sister chromatids united by one crossing DNA strand (a Meselson-Radding structure) or a pair of crossing strands (a Holliday structure) is the chromosomal DNA substrate that yields both gene conversion (by mismatch repair of heteroduplexes) and the associated reciprocal exchange of flanking heterozygous markers (by appropriate endonucleolytic scission of the cross-strand DNA connection).

Hersch focused on providing an explanation for the contrasting properties of mitotic and meiotic recombination in yeast. In the late 1970s the puzzle began to unfold. Hersch, Francis Fabre, John Golin, Judy Wildenberg, and I obtained proof that spontaneous and induced gene conversion occurred almost exclusively during the G1 phase of the mitotic cycle. Spontaneous G2 gene conversion occurred rarely, although it could be induced in G2 cells by ultraviolet light or X-ray radiation. At this juncture the classical model of G2 mitotic gene conversion needed to be abandoned. It was possible to account for the simultaneous occurrences of G1 mitotic gene conversion and reciprocal recombination of flanking heterozygous markers by replicational resolution of Holliday structures, as occurs in some bacteriophages. An additional model could also be envisaged; one in which gene conversion that occurs in G1 induces nearby crossing-over in G2 of the same cell.

In order to evaluate these models Hersch devised diploid genotypes that would allow him to question whether recombinant mitotic colonies contain genotypes diagnostic of recombination events in G1 and G2. This series of experiments was the crowning achievement of his stellar studies of recombination in yeast. They are all the more remarkable because they were initiated as Hersch, with Caryl's devoted assistance, courageously fought to recover from the severe stroke he suffered in 1976. Hersch confirmed that about 80% of gene conversion events occur in G1, while about 20% occur in G2. Moreover, he obtained evidence that resolution of Holliday junctions occurs by endonucleolytic scission in both G1 and G2 cells. Hersch and Mary M. Ruzinski also presented evidence of G1 gene conversion followed by G2 reciprocal intergenic recombination and occasional G2 gene conversion in red-white sectored colonies. Hersch summarized these findings in his final paper published posthumously in *Genetics.*

Earlier in his career Hersch and Fred Sherman observed that diploid cells exposed to sporulation medium, in which they will eventually undergo meiosis, commit to heteroallelic recombination before they commit to completion of the meiotic nuclear divisions. Rochelle Easton Esposito, Dianne Plotkin, and I confirmed this finding and also demonstrated that such cells commit to intergenic recombination before they commit to meiosis. Hersch opined that this early commitment to recombination might reflect the capacity of diploid cells to undergo G1 recombination, although the vast majority of meiotic gene conversions and reciprocal recombination occur later during the prophase stage of meiotic G2 cells; thus, the different properties of mitotic and meiotic recombination could be explained if G1 events play a major role in mitosis and a minor role in meiosis, while G2 events play a minor role in mitosis and a major role in meiosis.

Hersch's philosophy of research was to ask good questions and to design critical experiments that provide answers showing the way to a deeper understanding. He eschewed inductive leaps in favor of solid data. His scientific legacy to the general scientific community is surely his major role in the growth and development of yeast molecular genetics, but those of us intrigued by mechanisms of recombination have received a special gift and envoi—a uni fied view of mitotic and meiotic recombination as variations upon the theme of G1 and G2.

I was privileged to perform my doctoral research under Hersch's guidance from 1962 to 1967. Hersch's standard of performance was excellence. He was an exacting mentor and used his incisive mastery of the Socratic approach to uncover the merits and defects of proposed work. He was personally supportive, but most of all he wanted us to experience the self-assurance and enjoyment that comes from knowing that one has done a well designed experiment.

The welfare of the genetics graduate students at Seattle was Herschel's prime concern; we were his academic family. On social occasions Hersch, his wife Caryl, and their daughters Linda and Ann treated us to festive dining, conversational repartee and welcome respite from the pressures of graduate student life. In 1980 Hersch stepped down as chair of the genetics department. On that occasion, former graduate students, postdoctoral fellows, professional colleagues, and friends returned to Seattle from all over the world to attend a research conference convened to celebrate Hersch's stewardship of the department and to honor his concern and affection for students. Hersch's dedication to students has been memorialized at the University of Washington, Seattle, by the Herschel and Caryl Roman Undergraduate Science Scholarship awarded to outstanding science majors in their junior year.

Hersch loved the arts, literature, and rhododendrons. I fondly recall accompanying him on walks in his garden and listening to him describe the special attributes and parentage of members of his floral collection. The memorial rhododendron garden established by the Roman family in the University of Washington Arboretum bears an apt inscription: "This display was funded by the Roman family in memory of Herschel L. Roman, founder of the University of Wash ington Genetics Department, who loved both the beauty and the genetic principles so vividly exhibited by rhododendron hybrids."

Hersch completed the manuscript of his last paper a few days before his death on July 2, 1989. He is survived by his wife Caryl, his daughters Linda Roman and Ann Roman Weiner, his brother Arnold, his sister Ruth Levin, and his grandson Aaron Weiner.

HONORS AND DISTINCTIONS

Gosney Fellow, California Institute of Technology, host: Edgar G. Anderson, 1946-48

Guggenheim Fellow, University of Paris, host: Boris Ephrussi, 1952

Fulbright Research Scholar, University of Paris, host: Boris Ephrussi, 1956

President, International Congress of Yeast Genetics and Molecular Biology, 1961-84

President, Genetics Society of America, 1968

Elected to the American Academy of Arts and Sciences, 1969

Elected to the National Academy of Sciences, 1970

Citation of Merit, University of Missouri-Columbia, 1973

Gold Medal, Emil Christian Hansen Foundation, Copenhagen, 1980

Thomas Hunt Morgan Medal, Genetics Society of America, 1985

Honorary doctorate, Université Pierre et Marie Curie, Paris, 1986

Doctor of Science, Honoris causa, University of Missouri-Columbia, 1989

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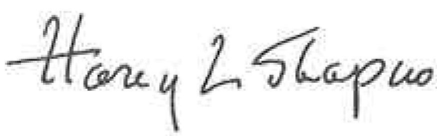

HARRY LIONEL SHAPIRO

March 19, 1902-January 7, 1990

BY FRANK SPENCER

DURING THE COURSE OF his professional career, which DJ spanned the second and third quarters of the twentieth century, Harry Lionel Shapiro made a number of significant contributions to biological anthropology, most notably his inquiries into racial mixture and the role of the environment and geography in determining racial characteristics. He also contributed to the foundations of forensic anthropology in the United States and is further distinguished by being the first in an influential series of doctorates produced under the aegis of Earnest Albert Hooton (1887-1954) at Harvard between 1925 and the early 1950s, a generation that contributed significantly to the development of academic physical anthropology in the United States. Although his professional career unfolded in a museum context (namely, the American Museum of Natural History), Shapiro was nevertheless able, through an adjunct position at Columbia University, to play a modest, yet integral role in this important development after World War II. [1]

PERSONAL HISTORY AND EDUCATION

Harry was the second of three sons born to Rose (Clemens) and Jacob Shapiro, both Polish Jews, who emigrated sepa rately to the United States in the late 1880s. Their first years in the New World were spent in New York City's Polish enclave situated on the lower east side between the Bowery and the East River. Here Rose, like many other new immigrants, eked out a living "rolling cigars" in one of the many neighborhood cigar factories. At first Jacob walked the crowded streets of lower Manhattan peddling assorted wares from a tray and later found regular employment in a shoe factory. Precisely when and under what circumstances they met is not known; however, after their marriage sometime in the mid-1890s it was decided to move to Boston, Massachusetts, where Jacob planned to open a small shoe repair shop. It was here in the Chelsea district that Harry was born and grew up. While little is known of his early childhood a yellowed photograph of a young Harry (aged four) decked out in a then fashionable "Little Lord Fauntleroy" suit suggests that his father's business had prospered; and save for the early demise of his younger brother (whose name and date of death is unknown) these early years appear to have been essentially happy ones.[2]

In contrast to his elder brother Barney (born in 1898), who could not wait to leave school and make his way in the world, Harry was inclined to more cerebral pursuits and enjoyed school. In 1916 his dedication to academic studies was rewarded when he successfully competed for a place at the prestigious Boston Latin School. At the Latin School Harry's intellectual sensibilities were refined and tuned by a rigorous classical education, which also generally prepared him for the eventual passage to Harvard three years later.

According to Harry[3] his academic plans on entering Harvard were rather vague and remained so until his sophomore year, when he apparently first encountered Hooton. At that time Hooton was a rising star in the Department of Anthropology and had already acquired a reputation for his lively lectures. Although he acknowledged that he too had been captivated by Hooton's charm and erudite wit, apparently what most impressed Harry and led him to choose anthropology as his major was the exciting prospects conjured up by Hooton "of the potential capacity of physical anthropology to make important and relevant contributions to the understanding of man and human populations."[4]

Within the broad sub-disciplinary boundaries of physical anthropology Harry was attracted to the then fledgling field of human genetics and the related study of race and racial mixture, all of which were well-aired topics in Hooton's lectures. With the probable exception of the pioneering study made by the German anthropologist Eugen Fischer (1874-1964) on the racially mixed community of Rehoboth in then German Southwest Africa[5] this field of inquiry was, as Hooton made abundantly clear, still wide open for further investigation. Apparently Hooton even suggested possible populations that would be ideal research subjects, noting in particular that "if there was anywhere in the world he would want to go to do [such] research it would be Pitcairn Island."[6]

By the end of his senior year the die was cast. On graduating magnum cum laude in 1923 Harry remained on at Harvard and spent the next year preparing for his intended field research. To this end Hooton directed him to the Bussey Institute for Applied Biology at Harvard, where his knowledge of statistics and human genetics was augmented by William Ernest Castle (1867-1954) and Edward Murray East (1879-1934). [7] Finally in 1924, supported by a graduate fellowship, Harry embarked on his study of the Pitcairn Islanders, who were the descendants of the celebrated mutiny aboard the British naval vessel *Bounty* in 1789. Originally the mutineers and their Tahitian women settled on Pitcairn, a remote and deserted tropical island in the South

Pacific; but by 1856 the island was deemed too small to support its resident population of 187 and as a consequence the Pitcairners were moved *en bloc* to Norfolk Island, a former whaling station situated temperately between Australia and New Zealand. While several families later returned to Pitcairn, most remained on Norfolk, and it was this population that Harry made the centerpiece of his doctoral research. [8] When completed, this study represented one of the first demonstrations of "hybrid vigor" in a racially mixed population (*vide infra*).

Professional opportunities in physical anthropology during the 1920s were limited, to say the least, and quite naturally as his doctoral dissertation neared completion Harry began worrying about his employment prospects. As luck would have it, early in 1925 an assistant curatorship at the American Museum of Natural History (AMNH) in New York City became available as a result of the untimely death of Louis R. Sullivan (1892-1925). Prior to his death Sullivan had been conducting pioneering anthropometric research in the Hawaiian Islands.[9] Hooton knew Sullivan and was an admirer of his work,[10] and saw Harry as a natural replacement. Clark Wissler (1870-1947), who was at that time chairman of the museum's Department of Anthropology, concurred. Thus, in 1926 on receipt of his doctorate he moved to New York City to begin what was to become a lifelong association with the AMNH.

Harry's first decade in New York was a period of intense research the products of which served to secure his scientific reputation and also a series of promotions at the museum that began with his elevation to associate curator in 1931. In 1942 he was made curator and appointed Wissler's successor as department chairman, a position he retained until his retirement twenty-eight years later.

This same period also witnessed the beginning of his re lationship with Janice Sandler (1908-62), the daughter of a prominent Manhattan lawyer. Following their marriage on June 26, 1938, Harry and Janice set up home on the upper east side. From all accounts Harry was both a devoted husband and father to their three children: Thomas (1939), Harriet (1942), and James (1946). According to James, he particularly adored Janice's "distracted mien" while cooking or drawing (she was a well-known amateur artist), and although vigorously resisting any suggestions she might make with regard to modifying something he had written, he always sought her criticism. From all accounts she regularly served as his unofficial editor until her tragic death in 1962, an event from which he never fully recovered. [11]

No private portrait of Harry is complete without some mention of the "second home" he helped build for his family in Pine Plains in upstate New York. Here Harry learned how to unwind tending his beloved garden (that included a French allée and a Jeffersonian serpentine wall). He also derived immense pleasure from chatting with passing neighbors "dressed in a broken straw hat, baggy khaki shirt and trousers, and leaning on his shovel—as his perpetual pipe haze stained the summer air." As revealed in the accompanying portrait, Harry's pipe was a trademark, which he carried everywhere. He was apparently introduced to the pleasures of pipe smoking sometime in late adolescence by his brother Barney—a lifelong habit sustained by regular shipments of his favorite brand from a Cantabrigian tobacconist.[11]

Along with these arcadian pleasures Harry also enjoyed music. With considerable perseverance he taught himself to play the cello and by the late 1950s he had become quite adept. The discovery that he and Colin M. Turnbull (1926-94), an associate curator in African ethnology in Shapiro's department from 1959 to 1969, shared a mutual interest in music led to regular informal recitals. Turnbull would play a clavichord he had built himself and Shapiro would accompany him on the cello. Following Turnball's departure from the AMNH this routine was broken until early in 1977, when it was resuscitated with the formation of a rotating musical group consisting of Leslie F. Marcus (piano), a research associate in the Department of Invertebrates; Cook Glassgold (recorder), a volunteer worker in anthropology; Clarissa Wilbur (piano), a secretary in anthropology; and Beatrice Brewster (recorder), a senior secretary in entomology. Their repertoire included pieces by Haydn, Schubert, and Beethoven.[12]

PROFESSIONAL DEVELOPMENT AND CONTRIBUTIONS

Shapiro's professional career separates into three somewhat arbitrary periods or phases of development (1926-45, 1945-70, and 1970-90), each characterized by a general shift in scholarly emphasis.

During the period 1926-45 Shapiro's primary research focus was the study of various populations in Oceania as well as selected groups along the Pacific rim. This work, however, did not begin immediately. Between 1926 and 1929 Shapiro developed several in-house research projects that included a study of a large series of crania acquired by the museum in 1924 which had been collected in the early 1900s by the German anthropologist Felix von Luschan (1854-1924) from charnel houses in the Greifenberg region of western Carinthia, Austria. While these specimens could not be arranged in chronological sequence they were known to represent samples of the Greifenberg population in the seventeenth, eighteenth, and early nineteenth centuries. Shapiro (1929,1) subjected these crania to a relatively sophisticated array of statistical analyses, the results of which constituted a substantial contribution to the knowledge of the complex racial character of central European populations. Another study was on a small series of male skeletons accidentally recovered in 1926 from a site near the Harlem River that was being excavated by the Rapid Transit System of New York City, which Shapiro (1930) subsequently discovered had been a former burial ground dating back to colonial times. Among other things this modest comparative study enabled him to confirm a basic finding of a study published in 1925 by the Smithsonian Institution anthropologist Ale Hrdlicka (1869-1943), namely, that stature had increased over time in this old American stock.[13] Like Hrdlicka, Shapiro was inclined to believe that this trend could be attributed to the complex stimulus of a favorable environment—a theme that was to reverberate throughout much of his later work.

In 1928 Shapiro received an anticipated invitation from the Bernice P. Bishop Museum in Honolulu to continue Louis Sullivan's earlier research efforts. The arrangement, which he accepted, required him to spend three extended seasons (of variable length) in the field surveying various racial groups. Beginning in April 1929 he spent nine months (April to January) in Hawaii, followed by a return visit in the summer of 1930 and again in 1931-32. On this latter occasion he spent seven months (September to March) visiting various countries on the Pacific rim, most notably China and Japan. This was followed in 1933 by a three-month sojourn (September to December) in Tahiti. This intense period of activity culminated with a six-month cruise (September 1934-February 1935) through eastern and southwestern Polynesia aboard the private yacht *Zaca* owned and skippered by Templeton Crocker (1884-1948).[14]

Flowing from all of this activity was a slew of publications dealing with a variety of subjects ranging from the physical characteristics of the Society Islanders to the ruins of Angkor.

Without question the most significant and enduring productions of this period were two books, *The Heritage of the Bounty* (1936) and *Migration and Environment* (1939). In the *Heritage* volume Shapiro revisited his doctoral dissertation from the perspective of his more recent observations and comparisons of the populations on Norfolk and Pitcairn (which he had visited during the cruise of the *Zaca*). In a nutshell, this now classic work refuted the claims of such workers as Charles B. Davenport (1866-1944) and Morris Steggerda (1900-1950), who had concluded from their study of cross-breeding that Jamaican mulattoes were biologically and intellectually inferior to their ancestral groups.[15] Finding no basis for such a pernicious thesis, Shapiro argued that the "dangers" of miscegenation were not only unfounded but that there was every reason to suppose that the production of racial mosaics had been an integral factor in the history of human civilization—an optimistic message that was subsequently underlined in a concise study published by UNESCO under the title *Race Mixture* (1954).[16] Likewise, *Migration and Environment* assaulted another "dangerous" myth of the period: the assumed stability of hereditary characteristics. Although the fallacy of this assumption had been exposed in the early 1900s by the seminal study conducted by the Columbia University anthropologist Franz Boas (1858-1942) on the descendants of immigrants born in the United States,[17] the theoretical ramifications of Boas's study had been widely resisted and criticized. This general resistance continued well into the 1920s and 1930s in spite of supporting evidence from newer studies such as the one made by the American anthropologist Leslie Spier (1893-1961), a former student of Boas.[18] Convinced of the validity of the environmental hypothesis, Shapiro orchestrated, with the assistance of two graduate students, William A. Lessa (b. 1908) in New York and Frederick S. Hulse at Harvard (1906

90), an ambitious comparative study of two oriental populations in Hawaii, the Chinese and Japanese. For reasons no longer clear Lessa's fieldwork in Hawaii and China was not utilized, whereas the anthropometric data collected by Hulse in Hawaii and Japan became the cornerstone of the Migration book.[19] Shapiro's analysis of these data provided a striking and influential example of marked differences in a range of physical characteristics which he persuasively argued were unquestionably due to complex environmental influences.

Following the disruption of World War II Shapiro did not, contrary to expectations, resume an energetic research agenda in the Pacific. Although, as indicated by subsequent field trips to the French Marquesas in 1956 and Polynesia in 1970, he continued to have a professional interest in the anthropology of the Pacific, these trips were not, so it would seem, designed to address a specific research problem and, as such, did not lead to any major publications. In fact, most if not all of the articles published in this area after 1945 fall into the category either of general articles (many of which can be found in the pages of the AMNH's popular magazine *Natural History*) or in semi-popular summations, such as his Thomas Burke Memorial Lecture on "The Peopling of the Pacific Rim" delivered in Seattle in 1964.

Along with this continuing but clearly diminished activity in the Pacific the period between 1945 and 1970 is distinguished by a mounting interest in the developing field of forensic anthropology and the practical application of physical anthropology to medico-legal issues, as well as the publication of several notable books: *Aspects of Culture* (1960), *The Jewish People: A Biological History* (1960), and an edited volume *Man, Culture, and Society* (1956). Throughout this period Shapiro held the position of adjunct professor of anthropology at Columbia University and during his tenure there (1942-1973) he was not only responsible for teaching a regular sequence of graduate courses but also sponsored several doctoral dissertations.[20]

Shapiro's involvement in the then nascent field of forensic anthropology began early in 1945. With victory in Europe at hand, he was asked by the quartermaster general in Washington, D.C., if he could assist in the grim task of identifying the remains of unknown American military personnel who had been killed in action. Complying with this request, Shapiro went to France in May 1946 and began a four-month tour of western Europe. From this survey he developed a detailed protocol for identifying the war dead based on established techniques in physical anthropology. His recommendations were immediately put into practice at the newly created Central Identification Point in Strasbourg, France, and subsequently were employed at a second identification laboratory established in Hawaii under the direction of another former student of Hooton, Charles E. Snow (1910-67).[21] Although the actual identification work in Europe was performed by European personnel under the technical direction of a French forensic specialist,[22] Shapiro was periodically consulted on problematic cases, and between the autumn of 1946 and 1948 he was a frequent visitor to the Brooklyn Naval Yard where he was called on to examine and identify skeletal material that had been shipped from Europe for repatriation and reburial. The general success of this program of recovery and identification provided an important stimulus to the subsequent development of American forensic anthropology.[23]

Following in the wake of this activity was an increasing number of requests from local and national law enforcement agencies anxious to employ his expertise in identifying skeletal remains—such as the time he was called to examine the charred bones of two young girls brutally murdered by a man in Brooklyn who had been dubbed by the tabloid press as "The Black Bishop."[24] Unfortunately, the corpus of this work was never summarized by Shapiro for publication and the same is largely true for the many paternity and adoption cases (cf. Shapiro 1963) he became involved in during the 1950s and 1960s, which profited from his knowledge and interest in problems related to racial mixture.

Another facet of Shapiro's professional activity during this middle-career period was his creation of several major exhibitions at the AMNH, in particular the Hall of the Biology of Man, which was praised in *The New York Times* as "the newest thing of value in this city" when opened to the public in 1961. He was also the principal architect of the equally successful Travelers Insurance Company exhibit, "The Triumph of Man" (a life-sized dioramic presentation of human biocultural evolution) at the 1964-65 New York World's Fair.

Coinciding with the unveiling of this latter exhibit was an invitation from the Indian Statistical Institute in Calcutta requesting his assistance in the development of a series of research studies devoted to current issues in human biology. This invitation led to two extended visits to India in 1965 and 1967, which represented his last major project prior to his retirement (cf. Shapiro 1966).

Blessed with a relatively robust constitution, Shapiro was able to continue working on projects and miscellaneous assignments until well into the latter half of the 1980s. Although several projects were never completed, such as his much anticipated biography of Hooton, this "retirement" period did witness the appearance of several publications, most notably his book *Peking Man* (1974). This work deals with the discovery of *Homo erectus* fossils in China during the late 1920s and 1930s and the mystery of their subsequent disappearance sometime early in December 1941 when

American and other westerners began a hurried retreat from China. While superficially a vehicle for an account of the fruitless search for these lost remains (dealt with essentially in the final chapters and which is not without interest) the real value of this narrative lies in the earlier chapters in which a recounting of the history of paleontology and, in particular, the events that unfolded in China between 1920 and the late 1930s is enriched with a personal knowledge of the events, as well as some occasional and tantalizing observations on individuals who figured in this history, such as Davidson Black (1884-1934) (see also Shapiro 1981), Franz Weidenreich (1873-1948), and Pierre Teilhard de Chardin (1881-1955). As this book reveals perhaps more than any other of his published works Shapiro was an inherently private and modest human being.

Towards the end of 1989 he was finally obliged to quit working. Throughout the months of November and December his health steadily deteriorated and finally on December 23 Harry L. Shapiro was admitted to Lenox Hill Hospital, where he died on Sunday, January 7, at 3.00 p.m.

POSITIONS, SERVICE, AND HONORS

Shapiro was a founding member of the American Association of Physical Anthropologists in 1930 (AAPA) and between 1935 and 1939 served a term as its secretary and subsequently as vice-president (1941-42). The AAPA and its adopted organ, the *American Journal of Physical Anthropology,* had been brought into existence by Hrdlicka *(vide ante),* who continued to watch over both of them with what the AMNH paleontologist William K. Gregory (1876-1970) called a "a fierce, almost maternal love." Like most of the AAPA's membership, Shapiro greatly admired and respected the "Old Man" for his many achievements and contributions to the discipline, he nevertheless believed, as did others from his generation, that Hrdlicka's resistance to the introduction of papers "worshipping such strange gods as statistics, genetics, and endocrinology" was unreasonable and impeding the development of the discipline. Cognizant of the mounting dissatisfaction with Hrdlicka, Shapiro early in the summer of 1941 took this delicate problem to Gregory, who was at that time president of the AAPA. He told Gregory that unless Hrdlicka desisted from conducting the society "as if it were a class of unruly schoolboys," there was a serious threat that the "younger men" might break away. Gregory was concerned and sympathetic and promised to do whatever he could to negotiate a change. [25] In the meantime, to alleviate some of the tension, Shapiro founded the journal Anthropological Briefs, which began publication in April 1942. Soon thereafter, however, Hrdlicka passed away and with it the need for Shapiro's alternative outlet.

Along with the AAPA Shapiro was also actively involved in a large number of other professional societies and national organizations. Among others, he was a fellow of the American Anthropological Association (AAA) with which he had been affiliated since his undergraduate years at Harvard. In 1948 he served a term as president of the AAA. Similarly, he had been a member of the New York-based American Ethnological Society since 1926 and served as its president from 1942-43. During the late 1940s he joined the American Eugenics Society and served as vice-president in 1953 and then president from 1955 to 1962. In this same time period he was intimately involved with the Association for American Indian Affairs, where he was a board director from 1947 to 1955. Overlapping with these responsibilities was his election to the National Academy of Sciences (NAS) in 1949. Throughout the 1950s and 1960s he was actively engaged in the work of various NAS committees, served as chairman of the anthropology section from 1953 to 1957, and was a council member from 1957 to 1960. He was also for a time associated with the National Research Council, where, among other things, he chaired the Division of Anthropology and Psychology from 1948 to 1953.

Shapiro's many honors and awards for his service and scientific contributions include the Theodore Rooselelt Distinguished Service Medal in 1964; a New York Academy of Sciences Distinguished Award for Contributions in Science in 1977; and the T. Dale Stewart Award for Distinguished Service, which was bestowed on him by the American Academy of Forensic Sciences in 1983.

ALONG WITH INFORMATION (CULLED) from archival sources[26] in the Library of the American Museum of Natural History and a recorded interview with Shapiro made by the author in December 1976 (RI:HLS/ FS-1976), this brief memoir owes much to the generosity of William W. Howells, Eugene Giles, and James E. Shapiro. Special thanks for assistance is also extended to Stanley Freed and several others at the AMNH, in particular, Ian Tattersall, Clarissa Wilbur, Leslie F. Marcus, and Richard Milner; plus Ralph L. Holloway and Joyce Monges at Columbia University.

NOTES

1. For further details see F. Spencer, "The rise of academic physical anthropology in the United States (1880-1980): a historical overview. *Am. J. Phys. Anthropol.* 56(1981):353-64.

2. Based on notes of a conversation with James E. Shapiro (June 1995) and a copy of the eulogy he delivered at his father's funeral on January 10, 1990 [E/JES].

3. From RI:HLS/FS-1976.

4. H. L. Shapiro. Earnest Albert Hooton, 1887-1954 in Memoriam cum amore. *Am. J. Phys. Anthropol.* 56(1981):432-33.

5. E. Fischer. *Die Rehobother Bastards und das Bastardierungsproblem beim Menschen.* Jena: Fischer (1913).

6. From RI:HI,S/FS-1976.

7. From RI:HLS/FS-1976. While this interview provides no insights into the comparative influence of either Castle or East on

Shapiro's thinking, there is reason to suppose that he would have found Castle's views on the human condition more attractive than those of East. For a discussion of their respective views on racial biology and the race concept per se see E. Barkan, *The Retreat of Scientific Racism,* pp. 143-148. New York: Cambridge University Press (1992).

8. H. L. Shapiro. "A study of race mixture as exemplified in the descendants of Tahitian and English mutineers of the Bounty." Ph.D. dissertation. Harvard University (1926).

9. L. R. Sullivan. Marquesan somatology with comparative notes on Samoa and Tonga. *Mem. Bernice P. Bishop Mus.* 9(2):141-249; and Observations on Hawaiian somatology. *Mem. Bernice P. Bishop Mus.* 9(4) :269-342.

10. See E. A. Hooton's obituary notice on Sullivan in *Am. Anthropol.* 27:357-58.

11. Based on E/JES.

12. See article "Chamber Music during Lunch Hour on the Fifth Floor," *Grapevine* (AMNH) 42(3).

13. A. Hrdlicka. *The Old Americans.* Baltimore: Williams & Wilkins (1925).

14. Crocker was an independently wealthy scientific dilettante and bibliophile. See his memoir, *The Cruise of the Zaca.* New York: Harper (1933).

15. C. B. Davenport and M. Steggerda. *Race Crossing in Jamaica.* Carnegie Institution Memoir No. 395. Washington, D.C.: Carnegie Institution (1929).

16. Another significant reflection of Shapiro's posture on race can be found in his involvement in the 1952 UNESCO "Statement on the Nature of Race and Race Differences by Physical Anthropologists and Geneticists." In A. Montagu, *Statement on Race,* pp. 173-82. New York: Schuman (1952).

17. F. Boas. *Changes in Bodily Form of Descendants of Immigrants.* Senate Document 208, 61st Congress, Second Session. Washington: U.S. Government Printing Office, 1911. For a shortened version see *Am. Anthropol.* 14(3):530-62.

18. L. Spier. Growth of Japanese children born in America and Japan. *Univ. Wash. Publ. Anthropol.* 3:1-30. Also it is interesting to note that C. E. Guthe (a former student of Hooton's at Harvard) noted similar changes in Boston immigrants. See his "Notes on the

cephalic index of Russian Jews in Boston." *Am. J. Phys. Anthropol.* 1(1918):213-23.

19. See F. S. Hulse. Habits, habitats, and heredity. A brief history of studies in human plasticity. *Am. J. Phys. Anthropol.* 56(1981):495501.

20. Included among the doctoral dissertations directed by Shapiro are James Taylor, "The Neanderthal tibia" (1968) and Alfonso Solimene, "An experimental investigation of the primate pelvic morphology" (1970).

21. Based on notes from a transcript of "Interview with H. L. Shapiro (AMNH) by Eugene Giles on September 12, 1989" and W. R. Wood and L. A. Stanley, Recovery and identification of World War II dead: American graves registration activities in Europe. *J. Forensic Sci.* 34(1989):1365-73.

22. C. Simonin. Identification des corps des soldats américains inconnus. *Acta Med. Leg. Soc.* 1 (1948):382-86.

23. T. D. Stewart. *Essentials of Forensic Anthropology,* pp. 11-12. Springfield, Ill.: Thomas (1979).

24. From E/JES.

25. Based on RI: HLS/FS-1976; see also letter (marked: "Confidential") from W. K. Gregory to C. B. Davenport dated July 7, 1941 (Gregory Papers, Library of the American Museum of Natural History). This letter summarizes the essential details of this episode in which Gregory notes his conversation with Shapiro ("for whom I have the highest regard") and, subsequently several others, namely, Sherwood L. Washburn, Earl Count, Morris Steggerda, and C. W. Dupertuis. The letter goes on to state, "I feel sure that on account of your long friendship and regard for Dr. Hrdlicka *[sic]* you are in a good position to plead with him to realize that, though he may disagree with Hooton or anyone else, the science itself can only grow by trial and error...."

26. At the time of preparing this short memoir Shapiro's private and professional correspondence was being catalogued and thus was not available for study.

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Courtesy of Geoffrey Grimmett

Frank L. Spitzer

FRANK LUDVIG SPITZER

July 24, 1926-February 1, 1992

BY HARRY KESTEN

FRANK SPITZER WAS A highly original probabilist and a humorous, charismatic person, who had warm relations with students and colleagues. Much of his earlier work dealt with the topics of random walk and Brownian motion, which are quite familiar to probabilists. Spitzer invented or developed quite new aspects of these, such as fluctuation theory and potential theory of random walk (more about these later); however, his most influential work is undoubtedly the creation of a good part of the theory of interacting particle systems. Through the many elegant models that Frank constructed and intriguing phenomena he demonstrated, a whole new set of questions was raised. These have attracted and stimulated a large number of young probabilists and have made interacting particle systems one of the most exciting and active subfields of probability today.

Frank Spitzer was born in Vienna, Austria, on July 24, 1926, into a Jewish family. His father was a lawyer. When Frank was about twelve years old his parents sent him to a summer camp for Jewish children in Sweden. Quite possibly the intention of this camp was to bring out Jewish children from Nazi-held or Nazi-threatened territory. Be that as it may, Frank's parents soon informed him that the situa tion was too precarious in Austria for him to return, and consequently Frank spent World War II in Sweden. He lived there in succession with two Swedish families, learned Swedish, and went through high school. He also attended Tekniska Hogskolan in Stockholm for oneyear. Somehow during the war his parents and sister made their way to the United States through the unoccupied part of France and North Africa. After the war Frank followed them to the United States, where he soon entered the army. In 1947 after his military service he entered the University of Michigan in Ann Arbor. In part because Frank managed to talk the University of Michigan into giving him college credit for several of his high school courses in Sweden, he completed his B.A. and Ph.D. in Michigan in a mere six-years (1947-53). Part of this time he was actually away from Michigan. One of his leaves was for an extended visit to Princeton, where he met the famous probabilist William Feller.

For financial support Frank drove a cab for a while in Ann Arbor. He also met and married his first wife, Jean Wallach, in Ann Arbor. Jean and Frank had two children, a daughter Karen and a son Timothy. In the mid-seventies this marriage ended in divorce, and Frank started a second marriage with Ingeborg Wald. Frank is survived by both his partners and his two children.

Two classical stochastic processes are random walk and Brownian motion. Random walk is often used as a model to describe an evolving quantity which can be observed, or which is meaningful, only for a discrete sequence of times. The value of such a process at the *k*-th observation is then typically denoted by S_k, with *k* running through the integers, or sometimes only through the positive integers. $X_{k+l} := S_{k+l} - S_k$ is then the increment of the process from the *k*-th to the (k + 1)-th observation and $S_k = S_0 + \sum_1^k X_i$. In a random walk the increments X_k are assumed to be indepen dent and identically distributed. Roughly speaking, this means that all X_k for different k have the same statistical properties, and that the value of any X_k has no influence on the values of the X_j for j ` k. A traditional example which can be modeled this way is a gambling situation in which one repeatedly plays the same game; X_k represents the gain of a given player during the k-th game. Brownian motion (also called a Wiener process because Norbert Wiener was the first to give a rigorous construction of this process) is used in some situations in which it is more appropriate to build a model with time varying continuously from $-\infty$ to ∞ or from 0 to 8, rather than having time restricted to a discrete sequence. A Brownian motion $\{B(t)\}_{t=\geq 0}$ has some fundamental similarities with random walk. For disjoint time intervals $[t_l, t_2]$ and $[s_l s_2]$, the increments $B(t_2) - B(t_l)$ and $B(s_2) - B(s_1)$ are independent, and when the intervals have the same length ($t_2 - t_l = s_2 - s_l$), then these increments even are identically distributed. In fact these increments all have a Gaussian (also called normal) distribution. In the simplest case the Brownian motion is one-dimensional, that is $B(t)$ is a real number. However, one also considers d-dimensional Brownian motion in which $B(t)$ is a d-dimensional vector. Brownian motion has many fascinating properties. For instance, its paths are continuous but nowhere differentiable; for a while in the nineteenth century mathematicians had even doubted that such functions exist.

Random walks as well as Brownian motion figured prominently in Frank's research.[1] My impression is that he usually picked his own research problems and that little of his work is due to direct guidance or influence of other probabilists; however, from some of his remarks I gather that the contacts with Feller, whom Frank met at Princeton, did have an important influence on his thesis. In the preface of his book (1964,1) Frank thanks "those of my teachers, Donald

Darling, William Feller, and Samuel Karlin, who introduced me to the theory of stochastic processes." His thesis was on two-dimensional Brownian motion {B (t)} (that means that B(t) takes values in the plane). It had been known for some time[2] that a two-dimensional Brownian motion does not visit the origin in the plane, but does come arbitrarily close to the origin. In his thesis and a 1958 paper based on it, Spitzer estimated how close a two-dimensional Brownian motion comes to the origin during a long time interval. Another peculiarity of two-dimensional Brownian motion is that it winds around the origin an arbitrarily large number of times. But it also unwinds itself and winds in the other direction infinitely often during its history. Spitzer further found the distribution of the winding number of the Brownian motion at a given time. This has led to many further investigations of the joint distributions of winding numbers with respect to more than one point. Yor[3] gives an impression of how far these investigations have gone. In (1964,2) Spitzer returned to Brownian motion and gave a limit theorem for the volume of the so-called Wiener sausage, the volume swept out by a ball whose center undergoes a Brownian motion (in dimension $d \geq 3$).

Frank's first academic position was at the California Institute of Technology as instructor from 1953 to 1955 and as assistant professor from 1955 to 1958. While there he became acquainted with Sparre Andersen's remarkable papers[4] which dealt with the maximum,

$$M_N = \max_{0 \leq k \leq N} S_k,$$

of a random walk $\{S_k\}$and the time at which this maximum is attained. Sparre Andersen showed that several relations held for these quantities independently of the distribution of the increments $\{X_i\}$. These properties derive entirely from the fact that $X_1, X_2, ..., X_n$ has the same distribution as any rearrangement $X_{\sigma(1)}, X_{\sigma(2)}, ..., X_{\sigma(n)}$ of this sequence ($_s$ here is a permutation of {1,2,...,n}). These results came as a considerable surprise to the probability community at that time, because limit relations for M_n so far had been based on specific assumptions on the distribution of the X_i. Spitzer realized what the basic combinatorial principles behind Sparre Andersen's results were and he greatly extended Sparre Andersen's papers. For instance, he showed that for any sequence $\{x_1, x_2, ..., x_n\}$, the values taken on by the maximum

$$m_n(\sigma x) = \max_{0 \le k \le n} \sum_{j=1}^{n} x_{\sigma(j)}$$

are the same as the values of

$$T(\tau x) = \sum_i \left(\sum_{j \in i-th \text{ cycle of } \tau} x_{\tau(j)} \right)^+$$

as σ and ` both run over the n! permutations of {1,2,...,n} . (Spitzer [1956] credits Bohnenblust with help on this proof.) When the x_i are replaced by independent identically distributed random variables X_i, then the T is much easier to deal with than the maxima. This led to the celebrated expression in (1956) for the generating function

$$\sum_0^\infty E \exp[-\lambda M_n] t^n,$$

where *E* denotes the mathematical expectation or average. This result is now known as the Pollaczek-Spitzer formula; Pollaczek[5] had in fact earlier derived the same formula by a much more complicated route and under more restrictive conditions. The general area of Frank's (1956) paper is now known as fluctuation theory.

From the California Institute of Technology Frank moved in 1958 to the University of Minnesota. Many of the earlier limit theorems on maxima of random walk had been developed at Cornell (by Kac, Erdös, Chung, and Sparre Andersen) and it was natural for Frank to visit Cornell at some time. He did so during the summers of 1958 and 1960. [6] This led to a move in 1961 to Cornell as a full professor, and, with the exception of a number of sabbatical and study leaves, Frank stayed there for the rest of his life. For a number of years at Cornell Frank worked on the development of potential theory for random walk. Since the famous work of Kakutani[7] and Doob[8] it had been known that there is a close connection between classical potential theory and Brownian motion. For instance, Green's function in *d* dimensions has an immediate interpretation in terms of the expected amount of time a d-dimensional Brownian motion spends in subsets of d-space. This works well when $d \geq 3$, when the Brownian motion is transient (that is, stays outside any fixed ball eventually). Also, the distribution of the position where a Brownian motion first hits a set can be used to solve Dirichlet's problem. Hunt[9] extended this relationship to situations where the Brownian motion is replaced by any transient Markov process. Spitzer then asked what the analogous results were for random walk, and more importantly, what the analogous results were for a recurrent random walk. Such a random walk spends an infinite amount of time in any ball and one cannot simply use the expected amount of time spent in a subset as an analogue for Green's function, because this quantity is usually infinite. This led Spitzer to study the so-called recurrent potential kernel. For an integer valued random walk $\{S_n\}$ this is given by

$$a(x) = \sum_{n=0}^{\infty} [P\{S_n = x\} - P\{S_n = 0\}].$$

Frank's deepest theorem of those years is probably that this sum converges for any random walk on the integers, without conditions on the distribution of the increments. He further showed that this is indeed a "good" potential kernel in the sense that one can write the solution of certain equations in terms of this kernel, and he studied the asymptotic behavior of this kernel. In turn, this allowed him to obtain limit theorems for the distribution of the position where a random walk first hits a given set. As a measure of the difficulty of these results it should be pointed out that it is still not known whether the series of a(x) always converges absolutely (Spitzer only showed conditional convergence). An excellent and readily accessible exposition of these results (and many more) can be found in Spitzer's elegant book (1964,1).

In a random walk $\{S_n\}$, S_n is sometimes interpreted as the position of a particle at time *n.* The assumption that the increments of the random walk are independent and identically distributed is reasonable when the particle moves entirely without influence of other particles. It is, however, a very simplifying assumption and is not justified in most statistical mechanics models. Even today it still is too difficult to analyze probability models which realistically deal with the interactions and collisions of molecules in a gas, say; however, Harris[10] had already considered some simplified models which incorporated collisions for particles which moved on the line. Perhaps stimulated by this, but in any case also by his desire to get away from the classical independence assumptions and to find new phenomena, Spitzer began in the late sixties to investigate a number of probabilistic models in which there are more interactions. In this vein he invented the "random walk in random environment" model. For a random walk in random environment the distribution of the increment X_{k+1} depends on the position at time k, S_k. This dependence is itself random. Formally, one first chooses a random environment which prescribes for each possible position x, what the distribution of X_{k+1} will be when $S_k = x$ (for some k). Once the environment is fixed the particle moves in this environment according to the transition rules specified by the environment. The model and its many later generalizations are of considerable interest and challenge to probabilists because of their non-Markovian nature; the full sequence of past observations gives us more information about the environment than just the last observations. In fact, by observing the successive positions of the random walk one finds out more and more about the environment even though the environment itself cannot be observed directly. During a visit to the Soviet Union in the early seventies Spitzer found that the same model had been independently invented there as a highly simplified and mathematicized model for DNA replication. This led to the joint paper (1975,4) with one author from the Soviet Union whom Frank met on his trip. Random walk in random environment is a model in which randomness is introduced in two stages, first in the choice of the environment (or equivalently, parameters for the transition mechanism), and then in the motion of the particle. Since 1975 probability models with such "two-stage" randomness have become very fashionable in probability as well as statistical physics.

More or less in the same period Spitzer also began to study models in which many (often infinitely many) particles interact locally. Nowadays we call these models "interacting particle systems." Closely related investigations were taking place in the Soviet Union by Dobrushin and his school.[11] Dobrushin's work was directly motivated by statistical physics, in particular by the Ising model for magnetism. Dobrushin was one of the people who gave a precise definition of Gibbs states (which generalize the Ising model) and who has contributed heavily to the study of their properties. It is not clear how much Frank had statistical mechanics in mind when he started looking at interacting particle systems, but it soon became an important factor. Various examples in interacting particle systems represent time evolutions which have well known statistical mechanics models for their equilibrium state. Like statistical mechanics models, several interacting particle systems exhibit a phase transition. In fact, it was precisely for such properties that Frank and Dobrushin selected some of their models for study. Because of this, interacting particle systems are responsible, in part, for the renewed interaction and cooperation between statistical physicists and probabilists taking place these days. Even though this is probably the area in which Frank had the greatest influence we have to restrict ourselves here to just two illustrations of models which he invented.[12] The first is the simple exclusion model which he introduced in (1970). Assume that there are infinitely many particles with positions in the integers. Each particle decides on its own (without influence from the other particles) when it would like to change position and where it would like to move. The interaction now comes from the single rule that at any time no two particles are allowed to occupy the same site. Thus, if a particle at position x decides at time t that it wants to move to site y, but y happens to be occupied at time t, then this move is suppressed and the particle at x stays there (until its next attempt to move somewhere). To complete the description of the model one must of course specify when and how a particle wants to jump. Often one assumes that these jumps follow a continuous time analogue of a random walk (or more generally a Markov process). In the exclusion model no particles are created or disappear. This is not the case for the second model, the so-called "nearest particle system." Again one looks at a system of particles on the integers. But now each particle that is present can disappear at a fixed death rate d and also particles can be born at an unoccupied site x at a rate which is assumed to be a function of the distances to the nearest occupied sites on the left and right of x. The first question for these models is whether there exists a decent process which corresponds to the above description; the trouble is that in theory in the exclusion model infinitely many particles may try to jump to a given site in finite time. Similarly, in the nearest particle system an infinite chain of births and deaths could conceivably occur in a finite time interval. Frank did not do much work on this existence problem by himself, but it has now been adequately solved. (See Liggett's book mentioned in note 12 for an exposition of this. This book also has many results about the nearest particle system to which Liggett himself has made major contributions.)

The next questions considered by Frank for various interacting particle systems were what the equilibrium distributions are for such processes and whether the state of the process converges to such an equilibrium distribution from suitable initial states. In many examples there is an analogue of a phase transition; for some parameter values in the transition mechanism there is a unique equilibrium state and for others there are more than one. The unraveling of these "phase diagrams" and the description of the domains of attraction of the various equilibrium distributions has been a fundamental goal of the field. For a number of examples Frank found all or some equilibrium states (1970, 1977). Such explicit results pleased him greatly. In other cases he discovered a so-called duality relation which is a basic tool for proving convergence to equilibrium. The theory has become very rich and many other questions have arisen.

An important topic nowadays is to describe how the system approaches equilibrium from suitable initial states. By taking suitable scaling limits this can often be described by partial differential equations for a local particle density (hydrodynamic limits).[13] One also tries to estimate probabilities of large deviations from equilibrium behavior and questions of metastability;[14] that is, if the system has one equilibrium state but starts out "far away" from this equilibrium state, how long can it stay far away? Another direction is to introduce particles of different types and to investigate when there are equilibrium states in which several of the types can coexist.[15] This direction has been stimulated by biological interpretations. As mentioned before, interacting particle systems are a very active, exciting area, and Frank was one of the founding fathers.

Spitzer was elected to the National Academy of Sciences in 1981 and held a Guggenheim fellowship in 1965-66. He was invited for many prestigious lectures, including a lecture at the International Congress of Mathematicians at Vancouver in 1974 and the Wald Lectures for the Institute of Mathematical Statistics in 1979. For almost twenty years he was on the editorial board of one of the major probability journals, the *Zeitschrift für Wahrscheinlichkeitstheorie und verwandte Gebiete* and its successor, *Probability Theory and Related Fields.* He also taught special courses at various summer schools.

Frank had great enthusiasm for his field. Not only did he love his own work, but, what is much rarer, he knew how to show genuine appreciation for the work of others. I often felt much encouraged by his comments, and from various messages that I received from colleagues and former students after his death, I know they felt the same. Any secretiveness about his ongoing research was totally alien to Frank. On the contrary, he usually tried to draw colleagues and students into cooperating with him. In this way I became his coauthor on a number of papers and even owe some papers by myself to Frank's questions and stimulation. He was quite generous with his time and on many occasions helped his students and teaching assistants with nonmathematical problems. Because of this and his spontaneity many people close to Frank felt great warmth towards him. Until he developed Parkinson's disease he was an inspired teacher. In fact, he even enjoyed teaching nonmathematical subjects. He taught me the rudiments of skiing and how to use the T bar and ski lift on the local ski slope. Frank knew how to help students over hurdles and once volunteered to give a pep talk to one of my Ph.D. students who seemed to have given up on his thesis work. Frank's talk had the desired effect and the student did finish his Ph.D. Of course, Frank also had considerable influence on his own Ph.D. students, and several of them have now made a career for themselves in interacting particle systems. A strong sense of elegance guided Frank in his research. For this reason he worked so hard to prove the convergence of the series for the potential kernel $a(x)$ mentioned before, without any conditions on the distribution of the increments. The great attraction of this result is that it has no extra conditions. Frank was always toying with probability models, looking for new phenomena. He did not particularly like extending the validity of some known results if this did not lead to some surprises. Once he complained about a visitor who had treated him to several blackboards full of formulae. "What did he want me to do? Eat a bunch of formulae?," Frank asked me. Probably because of these standards of his, he did not publish all that many papers (about 50), but he has helped shape probability theory as we know it today.

Frank had a great love for the outdoors, and, even though he never became a real expert, he was an avid mushroom hunter. He loved to ski. In fact, in his office he displayed with pride a list of times for a downhill run for a number of mathematicians during a "race" in which Frank had participated during his visit to the Soviet Union. He often went hiking, especially in the mountains, and cross-country skiing. He regularly went jogging almost until the end of his life, even after this became difficult because of his struggle with Parkinson's disease. In addition to Parkinson's disease Frank contracted bladder cancer. The immediate cause of his death was a urinary tract infection which seemed to have been related to the chemotherapy he was undergoing for his cancer.

I AM INDEBTED TO Jean Spitzer for a number of biographical data and to Thomas Liggett for some comments about Spitzer's work on interacting particle systems. I thank Geoffrey Grimmett for the photograph of Frank Spitzer and for several helpful suggestions for this memoir.

NOTES

1. Much of the following description of Frank's work is taken from my article in *Ann. Probab.* 21 (1993):593-607

2. P. Lévy. *Processus Stochastiques et Mouvement Brownien,* sect. 53-54. Paris: Gauthier-Villars (1948).

3. M. Yor. Etude asymptotique des nombres de tours de plusieurs mouvements browniens complexes corrélés. In *Random Walks, Brownian Motion and Interacting Particle Systems,* eds. R. Durrett and H. Kesten, pp. 441-55. Boston: Birkhäuser (1991).

4. E. S. Andersen. On the number of positive sums of random variables. *Skand. Aktuarietidskr.* 32 (1949):27-36. On the fluctuations of sums of random variables I and II. *Math. Scand.* 1(1953):263-85 and *Math. Scand.* 2:(1954):195-223. Correction in *Math. Scand.* 2(1954):193-94.

5. F. Pollaczek. Problèmes stochastiques posés par le phénomène de formation d'une queue d'attente à un guichet et par des phénomènes apparentés. *Mém. Soc. Math. France* 136, eq. 7.16.

6. I have not been able to confirm with certainty that these are the correct years.

7. S. Kakutani. Two-dimensional Brownian motion and harmonic functions. *Proc. Imp. Acad. Japan* 20(1944):706-14.

8. J. L. Doob. Semimartingales and subharmonic functions. *Trans. Amer. Math. Soc.* 77 (1954):86-121.

9. G. A. Hunt. Markoff processes and potentials I-III. *Ill. J. Math.* 1(1957):44-93 and 316-19, and 2 (1958):294-319.

.010. T. E. Harris. Diffusion with "collision" between particles. *J. Appl. Probab.* 2(1965):323-38.

11. There are too many related articles by Dobrushin to cite them all. Two examples are (1) R. L. Dobrushin. Markov processes with a large number of locally interacting components: existence of a limit process and its ergodicity. *Problems Inform. Transmission* 7(1971):14964 and (2) R. L. Dobrushin. Markov processes with many locally interacting components—the reversible case and some generalizations. *Problems Inform. Transmission* 7(1971):235-41.

12. However, the reader is referred to D. Griffeath, "Frank Spitzer's pioneering work on interacting particle systems *(Ann. Probab.* 21(1993):608-21) for a very fine survey of Frank's contributions. Excellent treatments of interacting particle systems in book form can be found in T. M. Liggett, *Interacting Particle Systems* (Berlin: Springer-Verlag, 1985) and R. Durrett, *Lecture Notes on Particle Systems and Percolation* (Pacific Grove: Wadsworth, 1988).

13. For a partial survey see A. De Masi and E. Presutti, *Mathematical Methods for Hydrodynamic Limits,* Lecture Notes in Mathematics, vol. 1501. Berlin: Springer-Verlag (1991).

14. See for instance R. H. Schonmann. Theorems and conjectures on the droplet-driven relaxation of stochastic Ising models. In *Probability and Phase Transition,* ed. G. Grimmett, pp. 265-301. Dordrecht: Kluwer Academic Publishers (1994).

15. See for instance R. Durrett. Ten lectures on particle systems. In *Lecture Notes in Mathematics,* vol. 1608, pp. 97-201. Berlin: Springer-Verlag (1995).

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Merle A. Tuve

MERLE ANTONY TUVE

June 27, 1901-May 20, 1982

BY PHILIP H. ABELSON

MERLE ANTONY TUVE WAS a leading scientist of his times. He joined with Gregory Breit in the first use of pulsed radio waves in the measurement of layers in the ionosphere. Together with Lawrence R. Hafstad and Norman P. Heydenburg he made the first and definitive measurements of the proton-proton force at nuclear distances. During World War II he led in the development of the proximity fuze that stopped the buzz bomb attack on London, played a crucial part in the Battle of the Bulge, and enabled naval ships to ward off Japanese aircraft in the western Pacific. Following World War II he served for twenty years as director of the Carnegie Institution of Washington's Department of Terrestrial Magnetism, where, in addition to supporting a multifaceted program of research, he personally made important contributions to experimental seismology, radio astronomy, and optical astronomy.

Tuve was a dreamer and an achiever, but he was more than that. He was a man of conscience and ideals. Throughout his life he remained a scientist whose primary motivation was the search for knowledge but a person whose zeal was tempered by a regard for the aspirations of other humans.

Merle Tuve was born in Canton, South Dakota, on June

27, 1901. All four of his grandparents were born in Norway and subsequently emigrated to the United States. His father, Anthony G. Tuve, was president of Augustana College and his mother, Ida Marie Larsen Tuve, taught music there. A next-door neighbor and contemporary was Ernest Orlando Lawrence. The two boys played together and at age thirteen began to build telegraphic and later radio equipment. They were among the early radio amateurs.

After Tuve's father died in the influenza epidemic of 1918 the family moved to Minneapolis, where Merle attended the University of Minnesota, graduating in physics in 1922 and obtaining a master's degree in 1923. Following a year at Princeton, where he was an instructor, Tuve went to the Johns Hopkins University to work for his doctorate. While at Minnesota Merle developed a close friendship with Breit, a theoretical physicist who moved in 1924 to the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. After Tuve's arrival at Johns Hopkins, Breit sought his collaboration in a possible effort to study the ionosphere.

At the time, the electronics equipment available was primitive and relatively insensitive. To demonstrate the existence of the ionosphere it would be necessary to find evidence that radio signals arrived over at least two paths, a ground wave and a sky wave. To take an example: if a receiver were set up 13 miles from a radio transmitter, and if the ionosphere layer were 100 miles above the receiver, two pulses should arrive, a direct pulse and then, a millisecond later, a reflected pulse. If the height of the ionized or reflecting layer were increased or decreased, then the difference in time of arrival of the two pulses would change correspondingly. Tuve devised the necessary detecting equipment and Breit and Tuve were able to use a Naval Research Laboratory oscillator for their source of radiation. They observed delayed pulses but could not eliminate the possibility that these were reflections from the Blue Ridge Mountains. However, one evening they found that after sunset the reflecting layer moved upward from a height of about 60 miles to a height of more than 115 miles as the delayed pulses began to arrive at longer intervals. The experiment was a success. Breit persuaded Johns Hopkins to accept the work as the basis for Tuve's Ph.D. thesis, and the degree was granted in 1926. Verification of the existence of the ionosphere opened an important field of research and suggested the practicability of radar.

Throughout his life, Merle displayed excellent critical judgment in identifying the most significant challenges and opportunities of the times. In 1926 he recognized the great importance of exploration of the atomic nucleus. To implement his vision he planned to go to England to Rutherford's laboratory. However, Breit and John Fleming, then acting director of the Department of Terrestrial Magnetism, talked him into coming there. He would be given an opportunity to develop equipment for production of energetic particles.

Several years of difficult and frustrating work followed, in which Tuve achieved high voltages using Tesla coils. But the equipment was plagued with failures of glass insulators. However, Tuve learned the hard way how to distribute voltage along a column. When Van de Graaff invented his belt-charging high-voltage generator Tuve was in position to adapt it as an excellent tool for experimental nuclear physics. By February 1933 Tuve, Hafstad, and Odd Dahl were observing nuclear reactions with a 600 keV beam. Splendid voltage control and stability enabled them to discover a resonance when lithium was bombarded by protons, and gamma rays were observed. This result led to the Breit-Wigner formula. The voltage capabilities of the equipment were ex tended to 1.2 MeV in 1934 and a number of nuclear reactions were investigated.

The high mark in achievement came in 1935 with a series of experiments by Tuve, Hafstad, and Heydenburg on proton-proton interactions. It had long been known that like charges repel each other. Yet atomic nuclei existed that contained 92 protons and more. What held such nuclei together? Through precise measurements with high-energy protons from their Van de Graaff accelerator striking a hydrogen gas target, the experimenters were able to answer the question. At intermediate and long distances protons repel each other but at short distances, that is, of the order of 10^{-13} cm, an attractive force exceeds the repulsive one. Analysis of these data by Breit, Edward U. Condon, and Richard D. Present yielded a nuclear potential that was identical to that of the neutron-proton interaction which had been obtained by Goldhaber by photodisintegration of the deuteron. This discovery was immediately recognized as an historically significant milestone in nuclear physics.

In the 1930s the laboratory was one of the leading centers of nuclear physics. Prominent theoretical physicists were frequent visitors. Breit moved to New York University in 1932, but he remained a steadfast friend and consultant. A high point in scholarly exchange came in January 1939 when Niels Bohr told a conference of theoretical physicists of the discovery of uranium fission by Hahn and Meitner. Within a day the discovery was confirmed at the Department of Terrestrial Magnetism by Richard Roberts and Hafstad. Soon thereafter Roberts observed that some uranium fission events are followed by delayed emission of neutrons.

Tuve focused his efforts on nuclear physics until 1940. He supervised the design of a pressurized Van de Graaff generator, which achieved energies above 4 MeV. He also began construction of a 60-inch cyclotron designed to pro duce large quantities of radioactive isotopes for use on the east coast.

Events across the ocean impinged heavily on Tuve. One Sunday afternoon in August 1940 I was working in a laboratory at the department when he came in. He had been listening to accounts on the radio of terrible destruction caused by a massive Luftwaffe raid on England. He spoke intensely of the need for defensive measures. From his experience with radios and electronics Tuve could visualize that an electronically actuated proximity fuze that would increase the effectiveness of ground-based antiaircraft fire might be feasible, but such a device would require rugged vacuum tubes that could withstand the forces encountered when it was fired from an artillery piece. This crucial problem was tackled the next day by Roberts. He dropped lead-encased tubes from the top of a building to a steel plate on a concrete apron below, subjecting them to accelerations greater than 5,000 *g*. This crude method in turn was quickly supplanted by tests with known forces in centrifuges. Once tubes capable of withstanding 20,000 *g* were available the design and production of prototype proximity devices were soon accomplished. These were repeatedly tested by Tuve's group and ultimately by the Navy. In August 1942 the Navy gave the go-ahead for large-scale production. Tuve understood the importance of quality control and of guaranteeing against accidental misfiring that might injure naval personnel. Misfiring was guarded against by a superior design. Quality control required careful monitoring by a large staff. This in turn required a transfer of activities from the Department of Terrestrial Magnetism to larger quarters in the newly formed Applied Physics Laboratory administered by the Johns Hopkins University and directed by Tuve. This also took place in 1942.

By the end of the war 112 companies were engaged in production work on fuzes. Tuve's organization oversaw the development of vacuum tubes, batteries, and other components small enough to fit into artillery shells and rugged enough to withstand being shot from a gun and spun rapidly. There were made safe enough to be stored and handled and to have a proper shelf life under military conditions.

Tuve's presence was felt throughout the vast enterprise. He assembled the personnel and established procedures. He maintained liaison with military, industrial, and civilian research leaders. By war's end 22 million proximity fuzes had been manufactured. Many variants of the original design were devised and produced. In terms of effect on the course of World War II the proximity fuze was one of two or three of the most important new military devices.

In a book published in 1980 titled *The Deadly Fuze* Ralph B. Baldwin described his personal role in serving under Tuve at the Applied Physics Laboratory. He also provided quotations from the Navy and Army command structure praising the effectiveness of the proximity fuzes and describing their important role in combat.

Soon after their entry into World War II the Japanese converted many of the islands of the western Pacific into what they regarded as unsinkable aircraft carriers capable of servicing long-range ground-based planes, but starting in early 1943 when the U.S. Navy began using proximity fuzes the Japanese air force incurred crippling losses. For the most part the unsinkable carriers became a liability.

In 1943 British intelligence became aware of large-scale German preparations for launching a great number of V-1 buzz bombs against London. These weapons were unmanned winged aircraft carrying loads of high explosives. The British destroyed some of the launching sites, but the Germans prepared many others. Tuve was informed of the nature of the devices. He ordered production of fuzes especially de signed to destroy them. A stockpile of the fuzes was available when the Germans initiated their V-1 attacks. Ultimately the proximity fuzes had a major role in destroying V-1 bombs and in stopping attacks using them.

Until late 1944 the proximity fuze was not used in land combat. This avoided capture of duds and production of devices or countermeasures by enemies. However, field artillery shells were produced that were equipped with appropriately designed proximity fuzes. These were available at the time of the Battle of the Bulge that began in December 1944. On that occasion the Germans committed their last reserves in a desperate attempt to break the Allied lines. They were met by artillery fire that inflicted enormous losses of life and morale. These losses often occurred after dark or in the presence of fog. The effectiveness of unseen fire at all times of the day and night was confirmed by later observation and prisoner-of-war reports.

After the war Tuve received the Medal of Merit from President Truman and was named an Honorary Commander of the Order of the British Empire. He also received the John Scott Award of the City of Philadelphia. On that occasion he placed his role in context, saying ". . . the proximity fuze was not invented by any one man; it was a composite of old inventions and re-inventions both here and in Britain. It was really a development, not an invention, and many individuals contributed to it." On that same occasion Tuve revealed what must be regarded as an essential component of his success in the proximity fuze effort. He stated that the principal discovery of World War II was the efficiency of the democratic principle in dealing with people. He said:

> The democratic principle is this: Tell the worker or the people of the community what the *need* is, invite them to contribute in the best way they can, and let them help you and help each other meet that need. Any society or any group always selects men to handle certain tasks, by elections

> or by hiring them or by some other system. But notice that a boss using the democratic principle does not depend on others, he *asks* his men, his workers to *participate.* This means that they help him with the whole job, they don't just do what they are told to do. This system of asking people to help with the whole job was what I used in running the proximity fuze development. It worked so well, the whole team took hold so vigorously, that during most of the work it was a struggle to keep up with them. I often felt like a short-legged donkey trying to keep from being run down by a stampede of race horses.

It is obvious that Tuve was an excellent administrator capable of directing large enterprises. After World War II he might have chosen any one of many major managerial careers, but Tuve was a man of ideals and ideas who put research and discovery ahead of power and position. He left the Applied Physics Laboratory, where he had dominion over thousands of people, to become director of the Department of Terrestrial Magnetism, where the professional staff numbered about fifteen and where austerity was a way of life.

Vannevar Bush, president of the Carnegie Institution of Washington, had established the policy that the institution would not expand its activities in peacetime research by taking government funds. Tuve wholeheartedly agreed with this policy, but a consequence was that he deliberately foreclosed the option of spearheading activities in big science, including the development of the next generation of large accelerators for high-energy physics. Instead, he preferred to seek areas of inquiry in which tiny groups of research scientists might make significant contributions. To implement this vision it was necessary to change the thrust of the Department of Terrestrial Magnetism. Prior to 1946 the organization had for the most part conducted activities consonant with its name. Tuve changed that. He converted it into a physics department and further stated that physics is what physicists do. Thus staff members, who in the main were physicists, had a broad license to use their imaginations in defining significant areas for interdisciplinary research. This freedom led to innovative ventures by some of the staff, including those engaged in biophysics and in the radioactive dating of rocks. Members of the Biophysics Section pioneered in molecular biology and eventually produced a book, *Studies of Biosynthesis in Escherichia Coli.* This represented world-class research and had wide acceptance and use. The radioactive dating group, led by L. Thomas Aldrich, also did world-class work. They perfected radioactive clocks based on uranium-lead, rubidium-strontium, and potassium-argon decay chains. In consequence they were able to date many of the world's Precambrian rocks and tectonic events affecting them. Another example of work encouraged by Tuve was studies of the effects of thunderstorms on electric charges over the earth's surface. In 1947 and 1948 two staff members, George R. Wait and Oliver H. Gish (then close to retirement age) made 65 traverses over the center of thunderstorms at altitudes of up to 48,000 feet. They found that in some storms electric current flowed in a direction opposite that noted in fair weather. Another achievement was one by Scott Forbush, who discovered the emission of cosmic rays from the sun.

During the period 1946-66, while Tuve was director, he carried out administrative functions and responded to numerous calls for public service. However, personal involvement in research was his principal activity. His fields of investigation included experiments in seismology, radio astronomy, and the development of superior optical image tubes.

The goal of Tuve's first personal research following his return to the Department of Terrestrial Magnetism in 1946 was discovery of knowledge about the interior of the earth.

At that time geophysicists were dependent on observations of earthquakes for information about the lower crust and mantle, but earthquakes are undependable with respect both to time and place, and observations lead only to approximate descriptions of the earth's interior. In 1946 geophysicists hypothesized that the structure of the earth was somewhat analogous to that of an onion, with an outer layer of granite overlying a basaltic layer, which in turn was above other concentric structures. Tuve and associates, including Howard Tatel, ultimately showed that the earlier model was oversimplified.

To obtain detailed knowledge of the crust and mantle required a more dependable probe than earthquakes. Tuve chose to use explosions to produce vibrations in the earth, and he and his group developed new sensitive seismometers which could detect the tremors at distances of hundreds of kilometers. Up to the time of the Korean War he was able to persuade his friends in the Navy to provide explosives and detonate them for him. Later he used large explosions being conducted in quarries as a source of seismic waves. All together, hundreds of experiments were done and the data analyzed. Many of the observations were made in various regions of the United States, but a substantial effort was devoted to South America, especially to the Andes.

Part of Tuve's personal attention to seismology was diverted in 1952. At that time Ewen and Purcell at Harvard had discovered radio emission from neutral hydrogen in our galaxy. Tuve went to Cambridge and obtained from them parts of the receiver they used for their discovery. A 23-foot-diameter German radar dish, borrowed from the National Bureau of Standards, was installed at the Department of Terrestrial Magnetism. Characteristically, Tuve set about improving the essential auxiliary electronic equipment and soon had what at the time was the best of its kind in the United States. From 1953 to 1965 the Department of Terrestrial Magnetism was a leading center of radio astronomy. Ultimately others, using federal funds, were able to obtain superior equipment.

Tuve's venture into the development of image tubes was not so much a personal research effort as an exercise in guiding the production of an important tool for astronomy. Through his superb grasp of electronics he was able to visualize that an increase in the effectiveness of telescopes was attainable. Photographic plates have been rendered very sensitive, but they still convert only a fraction of the incident photons into an image. Photoelectron emitters are more sensitive, and the electrons can be accelerated and their number greatly amplified. Under Tuve's chairmanship a committee designed a tube that improved the detection of light from distant stars. The end result was that the effectiveness of dozens of the world's telescopes was improved tenfold.

One of Tuve's strengths was his ability to select and attract high-quality associates and staff members. Throughout his career most of his projects were accomplished with the cooperation of one or two close associates. Tuve served as a major source of fresh ideas, enthusiasm, and drive. Often there were more ideas than might be implemented, and the gifted associates provided discrimination and sounding boards, resulting in an enhancement of Tuve's own excellent native judgment. The careers of scientists who experienced some years of contact with Merle were fostered and many have expressed gratitude for the association.

Tuve's willingness to respond to calls for public service has already been mentioned. He participated in many such activities. He served on the first U.S. National Commission for UNESCO, on the National Research Council's Committee on Growth, and on the U.S. Committee for the Interna tional Geophysical Year. He was the first chairman of the Geophysical Research Board of the National Academy of Sciences and home secretary of the National Academy of Sciences.

In addition to the awards already mentioned Tuve received the American Geophysical Union's Bowie Medal for unselfish cooperation in research, the National Academy of Science's Barnard Medal for meritorious service to science, the 1948 Comstock Prize of the National Academy of Sciences (given every five years for the most important discovery or investigation in electricity, magnetism, or radiant energy), the Bolivian Order of the Condor de los Andes for efforts in advancing science in South America, and the Cosmos Club Award. He was also the recipient of seven honorary degrees.

Tuve found great satisfaction in a ceremony at Carleton College conducted by Lawrence Gould, who was then president of the college. On that occasion honorary degrees were conferred on Merle, on his two brothers, George Lewis Tuve and Richard Larsen Tuve, and on his sister, Rosemond Tuve. All had achieved distinction in their professions.

Merle was married in 1927 to Winifred Gray Whitman, M.D. In keeping with his regard and respect for his mother and sister and his strong feeling about equal rights for women, he insisted that she continue her professional work under her maiden name. Merle and Winifred had two children, Trygve, who died in 1972, and Lucy, who survives. Both earned Ph.D. degrees and pursued scientific careers.

A former president of the American Geophysical Union, George Woollard, characterized Merle Tuve with these words:

> Anyone who knows Merle Tuve recognizes that he is a driver, who has never spared himself; a crusader, who has espoused the cause of science to the government and the people of this country; a patriot, who never questioned the wisdom of devoting some of his most productive years to classi

> fied military research; a leader, who had much to do with the success of the International Geophysical Year as well as with the outstanding reputation enjoyed by the Department of Terrestrial Magnetism of the Carnegie Institution of Washington; a public servant, who has devoted much of his time to the service of his fellow scientists through service on various boards and committees of the National Academy of Sciences and other groups; a diplomat, who has done much to foster both understanding and working relations between American and foreign scientists; and, finally, a warm-hearted individual, who has always been willing to help others.

Tuve carried on an extensive correspondence. The Library of Congress holds his papers in more than 400 archival boxes. His bibliography includes nearly 200 items. Of these, 25 have been selected for the bibliography that follows.

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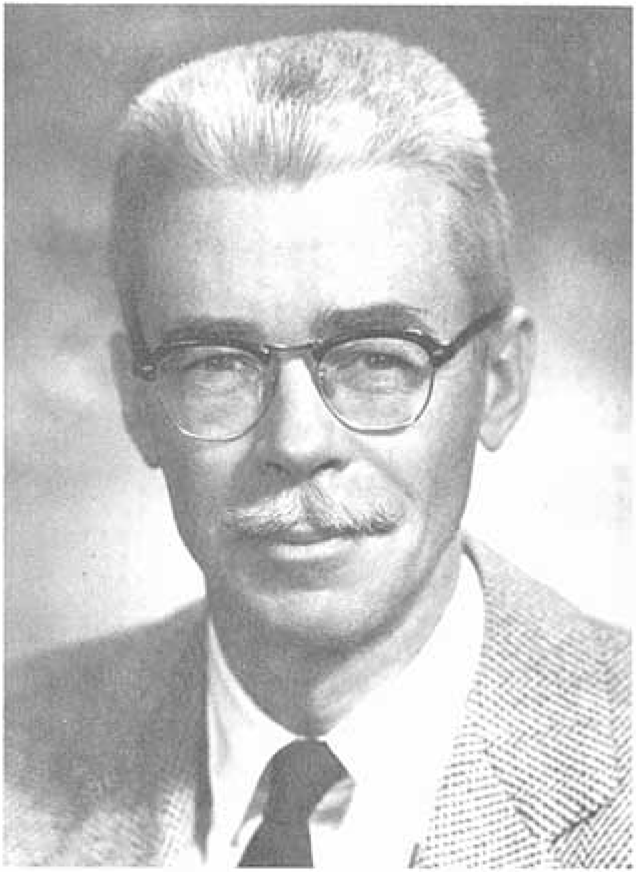

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JOHN WEST WELLS

July 15, 1907-January 12, 1994

BY WILLIAM R. BRICE

JOHN WEST WELLS DIED at his home on Brook Lane in Ithaca, New York, on January 12, 1994. As a teacher, scholar, and internationally known researcher he made an indelible mark on the world of paleontology through his own contributions and through the work of his many students. Although he spent the formative years of his teaching career at Cornell University, he served on the faculty at the University of Texas (1929-31), at the State Normal School (SUNY) at Fredonia, New York (1937-38), and at Ohio State University (1938-48). During World War II he served with the Office of Strategic Services and assisted with war damage assessment studies.

Wells was a leading authority on both modern and fossil corals, and it was through his work with these simple fossils that he provided tangible evidence of changes in the rotational period of the earth. Geophysicists had long predicted that tidal friction should cause a slowing of the earth's rotation, but it was John Wells who, using only the simplest of equipment, counted the daily growth rings on fossil corals clearly demonstrating the predicted changes in the rotational rate. This one small paper of only three pages spawned a remarkable increase in research studies dealing with the incremental skeletal growth in many groups of invertebrates.

EARLY YEARS AND EDUCATION

John Wells was born in Philadelphia on July 15, 1907, but spent most of his youth in Homer, New York, about 20 miles from Cornell University, surrounded by the classic Devonian rocks of the Finger Lakes region. After graduating from the local high school he attended the University of Pittsburgh with the intention of studying medicine, but soon switched to chemistry. As part of his course work he took a few geology courses which really captured his imagination. He especially liked the two geology professors, Ransom E. Sommers and Henry Leighton. This chance meeting of Wells with Sommers and Leighton has an interesting twist to it, for both Sommers and Leighton were graduates of Cornell University.

Eventually Wells took more courses in geology than chemistry, which was his major, and he received his bachelor of science degree in 1928. Upon graduation he obtained a position at the University of Texas, not in chemistry, but as an instructor of geology. By this time he had developed an interest in paleontology and Leighton, his mentor at the University of Pittsburgh, suggested he should study biology. As his home was only a few miles from Cornell University, it became the obvious choice and he attended two summer sessions there to increase his knowledge of biology.

Leighton also suggested that, if Wells was truly interested in paleontology, he should study with Gilbert D. Harris at Cornell. No doubt Wells had made contact with Harris during his two summers studying biology at Cornell, for he soon obtained an assistantship with Harris which covered his living expenses and $75 per term for tuition. While still teaching at the University of Texas, Wells completed his M.A. at Cornell in 1930, and the following year he left

Texas to pursue further graduate work under Harris's guidance at Cornell.

While at Cornell Wells met and married a fellow geology student, Elizabeth Baker, known to her friends as "Pie," a name which came from her childhood days in Indian Guides. John and Pie were married in 1932, and they had one daughter Ellen Baker Wells. John was predeceased by his wife of almost sixty years, who died at their summer home on Cayuga Lake in Sheldrake, New York, on July 1, 1990. Their daughter died in 1995, but he is survived by two granddaughters, Diane Elizabeth Hull and Linda Ann Wilson, and two great-grandchildren, Alan Scott Hull and Elizabeth Darlene Hull, all of San Luis Obispo, California.

After receiving his Ph.D. in 1933 Wells was awarded a National Research Council Fellowship and he spent well over a year in Europe studying at the British Museum in London, the Musée Nationale in Paris, the Humboldt Museum in Berlin, and other locations. During this time he laid a strong paleontological foundation that was to pay great dividends later with his taxonomic work on corals. The motivation behind this trip may have come from his friend and advisor at Cornell, G. D. Harris, for Harris had made a similar but less extensive study sojourn in Europe before joining the faculty at Cornell in 1894.

Upon his return from Europe, Wells managed to work with T. Wayland Vaughan in Washington, D.C., while job hunting. One result of this association was their monumental volume on the revision of Scleractinia [1] in 1943. During 1937-38 Wells taught at the State Normal School at Fredonia, now part of the state university system of New York, but soon left for a position at Ohio State University, where a former Cornell classmate, W. Storrs Cole, was on the faculty. It is most likely that his association with George White at Ohio State led Wells to develop an interest in the history of geology and he eventually produced over a dozen papers in that field. Both Wells and White were strong supporters of the History of Earth Sciences Society, which was founded in 1981-82.

In 1948 Wells followed his colleague, Storrs Cole, to Cornell where he would spend the rest of his career, serving as a professor of paleontology in the department where he had been a graduate student and occupying an office just down the hall from Cole. Wells was department chairman from 1962 until 1965, and upon his retirement in 1973 became professor emeritus.

PROFESSIONAL ACTIVITIES

From his first paper in 1930 until his last in 1988 Wells directed his research efforts toward a better understanding of scleractinian corals. Over half of his more than 175 professional papers addressed some aspect of these organisms. The first work, in which he described corals from the Glen Rose Formation, was a result of his research while at the University of Texas; but he soon expanded his work to include fossils from the Atlantic and Gulf coastal plain. Wells benefited from his relationship with G. D. Harris, who, two years before his retirement from Cornell, founded the Paleontological Research Institution (PRI) in 1932, a private research organization devoted primarily to the study of Tertiary fossils but encompassing the study of all geologic history. Several of Harris's former students were working in many parts of the world during the 1930s and they would send Harris well documented samples from their field areas, and he would have Wells do the identification and descriptions. Another benefit was derived from the fact that PRI published two paleontological journals which Harris founded, *Bulletins of American Paleontology* (1895) and *Palæntographica Americana* (1916), which provided an av enue for continuous publication of the material as it became available. Although Wells did publish many papers through PRI, he was not in any way limited to those journals; nor did he limit his research just to the corals, for he turned his boundless curiosity to other topics of paleontology such as fossil vertebrates in the Devonian rocks of Ohio and New York and, being in the heart of Devonian country, he examined the tabulate and rugose corals from those rocks. His analysis of *Heliophyllum halli*[2] remains the seminal description of individual variation in fossil invertebrates. While his work on scleractinian corals was systematic and highly developed, his work with fossils from the Devonian rocks seemed to be driven more by his interest in the particular topic or fossil than part of a planned, orderly study.

Until about 1950 Wells worked only with fossil corals from the Mesozoic and Cenozoic eras, but through his relationship with the U.S. Geological Survey, which he maintained from 1946 until well after his retirement, he and colleague Storrs Cole were selected to be part of the scientific team assigned to the atomic bomb testing sites in the Pacific: Bikini Atoll in 1947 and Arno Atoll in 1950. From this time forward he began to expand his knowledge of recent corals. Material from these and other expeditions occupied him from 1950 well into the 1980s. Few people were his equal in species-level coral taxonomy or in his encyclopedic knowledge of both fossil and recent corals. In addition to the monograph mentioned above he published in 1954 the *Recent Corals of the Marshall Islands*[3] and in 1956 with Dorothy Hill prepared most of the Coelenterata section of the *Treatise on Invertebrate Paleontology, Part F.*

From my own association with Wells at Cornell, first as a graduate student in the department and then as a member of the summer teaching faculty, I remember countless boxes of coral specimens arriving on a continual basis and all addressed to Dr. Wells. In 1971 alone he identified well over one thousand specimens from the Red Sea sent to him by colleagues at Tel Aviv University after other people failed to identify them.

Even though the bulk of his work was on corals mainly from Mesozoic to Recent it was his work with some Devonian rugose corals of New York that really caught public attention. In 1954 Wells was a Fulbright Lecturer at Queensland University in Brisbane, Australia, which provided him with an opportunity both to study the corals of the Great Barrier Reef and make an extensive collection of corals for the U.S. National Museum in Washington, D.C. Around the Cornell department the story was told that it was while studying these living corals and their diurnal habits (active in the daytime and more dormant at night) Wells started thinking about the fossil corals with regard to this activity. Using Devonian rugose corals collected near Cornell and using very unsophisticated equipment, Wells began to count fine ridges about 50 microns wide between the larger ridges which were interpreted as annual layers on these samples. He believed these fine ridges represented daily growth lines deposited during the daytime activity of the animal; thus, a count of these would indicate the number of days the organism was active and, most importantly, this count would represent the number of days in the year at the time the coral was alive. His ridge counts centered around 400; thus, he postulated that the earth rotated 400 times per year during the Devonian, compared to about 365 today. Later studies showed the number of days during the Pennsylvanian to be somewhere in between. So he had provided independent evidence to support the geophysical calculations and speculation about the gradual slowing of the earth's rotational period. He first delivered these results as part of his presidential address to the Paleontological Soci ety in November 1962; shortly thereafter his address was published in *Nature* as *Coral Growth and Geochronometry*[4]. This discovery came at a time when "big science" was the order of the day and huge sums of money were being spent on equipment and research. J. B. S. Haldane, the British scholar, noted this fact in a *New York Times* article and pointed out that great science can still be done with nothing more complex than a hand lens and careful observation.

It was this work by Wells and the literal explosion of similar studies on the daily, monthly, and annual growth records preserved in fossils that allowed for new calculations on the orbital pattern of the earth; this, in turn, has caused a re-interpretation of the cyclical deposition of sedimentary rocks and climate variation.

THE FINAL YEARS

When the Department of Geological Sciences at Cornell moved from the College of Arts and Sciences to the College of Engineering in 1981, Wells, after being in and around McGraw Hall since his student days in the early 1930s, moved with it. In 1973 after twenty-five years at Cornell he retired from active teaching and became professor emeritus, but he certainly did not retire from active research. He published seven papers and abstracts in the year of his retirement and followed these with continued research and publication. His 1983 revision of the Galápagos Scleractinia[5] provided quite a comprehensive account of this fauna.

Although Wells was a private person he was generous and loyal to his friends. With a house, "Luck Stone Lodge", on Cayuga Lake at Sheldrake, New York, Wells's small home near Cornell was always vacant during the summer. It was not uncommon for him to loan his Ithaca place to a newly appointed professor of prior acquaintance who was waiting for his own dwelling to be readied. That invitation would be supported by a bottle of champagne left in the refrigerator as a welcome. Wells was a noted historian of the Cayuga Lake region and I remember on field trips how he would give commentary not only on the fossils and rocks we were studying, but also on the changes in architectural styles of the houses over time and from which quarry the building stones in the various houses had come. His small publication *The Cayuga Lake Bridge*[6] went through three editions.

He earned and received many awards and accolades over his life time (e.g., a fellow in the Geological Society of America, president of the Paleontological Research Institution [1961-63], president of the Paleontology Society [196162], member of the National Academy of Sciences [1968], Paleontology Society Medal [1974], and the James Hall Medal of the New York Geological Survey [1987]).

The fame changed him very little; he remained just John Wells. One day not long after his election to the National Academy of Sciences he was sorting his mail during the slow elevator ascent to the fourth floor of McGraw Hall. Just as he reached the floor a student heard him mutter to himself after opening an impressive looking envelope, "Oh, dinner at the White House. The wife will like that," and went through the open door into his laboratory, leaving a somewhat speechless student in his wake.

Perhaps the James Hall Medal citation says it best:

John W. Wells

for distinguished contributions to paleontology and stratigraphy

Professor, Historian, Mentor

NOTES

1. Revision of the suborders, families, and genera of the Scleractinia. Geological Society of America Special Paper, no. 44.

2. Individual variation in the rugose coral species *Heliophyllum halli E. & H. Palaeontogr. Am.* 2 (6):1-22, 1937.

3. U.S. Geologic Survey Professional Paper, 260-I, p. 385-486.

4. *Nature* 197(4871):948-50.

5. Annotated list of the scleractinian corals of the Galápagos. In *Corals and Coral Reefs in the Galápagos Islands,* eds. P. W. Glynn and G. M. Wellington, pp. 212-96. Berkeley: University of California Press.

6. Ithaca, New York: DeWitt Historical Society, 1958, 1961, 1966.

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