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CONTENTS

PREFACE vii

ROBERT WAYNE ALLARD 3
BY MICHAEL T. CLEGG

ROBERT JOHN BRAIDWOOD 23
BY PATTY JO WATSON

HARMON CRAIG 45
BY KARL K. TUREKIAN

GEORGE KELSO DAVIS 59
BY ROBERT JOHN COUSINS

VINCENT GASTON DETHIER 77
BY ALAN GELPERIN, JOHN G. HILDEBRAND, AND THOMAS EISNER

WALTER GORDY 97
BY FRANK C. DE LUCIA AND BRENDA P. WINNEWISSER

KENNETH LOCKE HALE 115
BY MORRIS HALLE AND NORVIN RICHARDS

CHARLES F. HOCKETT 151
BY JAMES W. GAIR

HENRY M. HOENIGSWALD 181
BY GEORGE CARDONA

WILLIAM WHITE HOWELLS 207
BY JONATHAN FRIEDLAENDER, WITH DAVID PILBEAM, DANIEL HRDY, EUGENE GILES, AND ROGER GREEN

HENRY G. KUNKEL 225
BY JACOB B. NATVIG AND J. DONALD CAPRA

HALLAM LEONARD MOVIUS JR. 243
BY HARVEY M. BRICKER

WILLIAM DUWAYNE NEFF 263
BY JAY M. GOLDBERG AND NELSON Y.-S. KIANG

DONALD OSCAR PEDERSON 285
BY DAVID A. HODGES AND A. RICHARD NEWTON

JAMES MATHER SPRAGUE 307
BY ALAN C. ROSENQUIST AND S. MURRAY SHERMAN

OWSEI TEMKIN 325
BY SAMUEL H. GREENBLATT

JOHN GORDON TORREY 345
BY LEWIS FELDMAN AND ALISON BERRY

JEROME VINOGRAD 357
BY ROBERT L. SINSHEIMER

AARON CLEMENT WATERS 369
BY CLIFFORD A. HOPSON

FRED LAWRENCE WHIPPLE 393
BY GEORGE FIELD

PREFACE

On March 3, 1863, Abraham Lincoln signed the Act of Incorporation that brought the National Academy of Sciences into being. In accordance with that original charter, the Academy is a private, honorary organization of scientists, elected for outstanding contributions to knowledge, who can be called upon to advise the federal government. As an institution the Academy's goal is to work toward increasing scientific knowledge and to further the use of that knowledge for the general good.

The *Biographical Memoirs,* begun in 1877, are a series of volumes containing the life histories and selected bibliographies of deceased members of the Academy. Colleagues familiar with the discipline and the subject's work prepare the essays. These volumes, then, contain a record of the life and work of our most distinguished leaders in the sciences, as witnessed and interpreted by their colleagues and peers. They form a biographical history of science in America—an important part of our nation's contribution to the intellectual heritage of the world.

JOHN I. BRAUMAN
Home Secretary

Biographical Memoirs

VOLUME 89

ROBERT WAYNE ALLARD

September 3, 1919–March 25, 2003

BY MICHAEL T. CLEGG

ROBERT ("BOB") WAYNE ALLARD made wide-ranging contributions to both basic and applied plant genetics. He began as a plant breeder and wrote one of the most successful plant-breeding texts of his era, but his most important contributions were in evolutionary genetics. He was a founder of experimental plant population genetics and he infused the field with high standards of experimental and theoretical rigor. His investigations ranged from elegant experiments to dissect the genetic factors responsible for quantitative genetic variation, to the study of gene-environment interactions, to the analysis of selection in long-term experimental barley populations. But his most significant work was encompassed in a series of papers on the genetics of inbreeding populations, where he overturned conventional dogma by showing that inbreeding plant populations have substantial levels of genetic variation. In the course of his work on inbreeding species, he turned to the characterization of the genetics of wild and naturalized species and contributed to the origins of the field of plant ecological genetics. He was also a teacher *par excellence*, training more than 50 Ph.D. students and an even larger number of postdoctoral students over a career that spanned more than

50 years, and he led the University of California, Davis, Genetics Department to preeminence during the 1960s and 1970s.

EARLY INFLUENCES

Bob Allard was born in the San Fernando Valley of California on September 3, 1919. In the years between the two world wars the San Fernando Valley was largely pastoral and Bob's early years were spent on the family farm. Around 1930 his father relocated his farming operation to the San Joaquin Valley about 15 miles west of Modesto. Like many farmers of the era, Bob's father cooperated with University of California agricultural researchers by dedicating a portion of his land to experimental trials. A UC Berkeley plant breeder named W. W. Mackie maintained plots of lima and common beans on the Allard farm, and Bob was assigned the task of assisting Mackie with the maintenance of the experimental plots. This turned out to be the formative experience of Bob's young life, because Mackie instilled in Bob a lifelong fascination with the causes of phenotypic variation. In an oral history interview, Bob much later recalled that Mackie introduced him to the new science of Mendelian genetics during this period, thereby contributing to his later choice of scientific career.

In recounting these early experiences, Bob would passionately describe the pleasure he took in listening to Mackie and in hearing his theories about genetic variation and its practical exploitation. Bob was not a man to dwell on the past; he strongly preferred to look toward the future. His occasional recollections of Mackie were exceptional and reflected the enduring impact of this period on his later scientific development. According to Bob's much later memories, Mackie was also interested in the ecological bases of adaptation and he introduced Bob to other plant species common in their Central Valley environment, including the slender wild oat (*Avena barbata*) that would later feature importantly in some of Bob's research.

It seems likely that Mackie influenced Bob's decision to enter UC Davis as a student of agriculture in the fall of 1937. During his undergraduate years Bob worked as a student assistant for Coit Suneson of the U.S. Department of Agriculture, and this also had an enduring impact on Bob. Suneson, along with Harry Harlan and Gus Wiebe, was engaged in developing bulk populations of wheat and barley, known as composite cross populations. The theory at the time was that bulk populations would both act as a reservoir for useful genetic variation while at the same time evolving toward greater adaptation under standard agricultural conditions. Years later Bob would use these composite cross populations as a powerful resource for studies in experimental population genetics. These early experiences did much to define Bob's approaches to plant genetics and they serve to illustrate the powerful impact that scientific mentors can have on young minds.

After finishing his undergraduate training, Bob entered the graduate program at the University of Wisconsin, Madison. Certainly the biggest thing that happened to him at Madison was meeting and marrying Ann, his wife of 59 years. On the rare occasions when Bob would talk about his graduate school days, his chief recollection was being called into World War II service just prior to the scheduled date for his final dissertation defense. It seems that the university would not reschedule the defense, and Bob had to return to Madison after the war to defend his thesis. Bob's Ph.D. research was on wheat cytogenetics, and aside from publishing his dissertation work following the war, he never returned to this topic. There were strong influences at Madison, including Rubush G. Shands (his major professor), Charles E. Allen, and R. A. Brink, but I always had the feeling that Bob had a clear idea of his future research directions by the time he left Davis.

After entering World War II service, Bob was sent to the Naval Supply School at Harvard. Later he was assigned to a research unit at Fort Detrick, Maryland, where he was engaged in work on biowarfare, a subject he never discussed, except to say that he had been in a research unit for part of the war. Still, this provided Bob's only postdoctoral training and broadened his research experience.

In 1946 Bob returned to UC Davis as assistant professor of agronomy and assistant geneticist in the Agricultural Experiment Station, and he remained at Davis affiliated with the Agronomy Department throughout his career. He was hired as a bean breeder and his particular focus was on the improvement of the lima bean. At that time Davis was a branch of the Berkeley College of Agriculture and had little autonomy. It was also a very small school with fewer than 800 students, most of whom were there for two-year terminal degrees in agriculture. Bob was an important player in a faculty generation that turned Davis from a small satellite agriculture campus into a thriving and world-renowned university.

From the beginning Bob's work blended both basic genetics and practical plant improvement. In the initial years he focused on both the identification of disease-resistance genes and applications of the backcross method of breeding for the incorporation of disease resistance into elite lines of lima beans. The search for disease resistance genes led to an extended field trip to Central and South America to collect wild relatives and primitive land race materials as genetic resources for future breeding efforts. He later published an article for the California Dry Bean Research Conference on plant exploring in Latin America. The conservation of genetic resources remained an abiding interest, one that was communicated to a number of Bob's students.

At heart Bob was a geneticist, and along with his practical work on lima bean improvement, he began to develop genetic markers in lima beans. These were largely seed coat markers based on an amazing range of seed coat color patterns. Bob and his early students patiently dissected the inheritance of these discrete color polymorphisms and then employed them as markers to study adaptive change in the lima bean. A particularly fascinating aspect of the color patterns was the interactions between different genetic factors that lead to the mosaic patterns evident on the seed coats. We learn and generalize from our empirical experiences, and these are based on the materials that we choose to study. In Bob's case the theme of gene interaction, based in part on his observations of seed coat color patterns in the lima bean, continued to dominate his thinking throughout his career.

The practical side of Bob's program prospered in these early years. He released a number of new varieties of lima beans; one variety, "Mackie," was named for his childhood mentor. He also began work on a novel plant-breeding text. The book, *Principles of Plant Breeding*, published in 1960 had an enormous impact and was ultimately translated into 17 languages. It remained the premier plant-breeding text for a generation. The book was novel because it emphasized genetic principles rather than methods and this contributed to its great success. Bob was also a very fine writer and this, too, contributed to the wide acceptance of *Principles of Plant Breeding*. He took great pains with everything he wrote, and the result was always a model of clarity and precision. Bob would not put his name on a paper until he had worked through it carefully, reanalyzed the data, and improved the exposition. He did not believe in honorary authorships and he was very economical with citations. His practice was to cite only essential supporting papers.

For many years Bob's plant-breeding colleagues urged him to write a second edition of *Principles of Plant Breeding*, and he promised to do so, but it was not until 1999, almost 15 years after Bob's retirement and just four years before his death that a second edition was published. Bob admitted that the second edition was really an entirely new book that contained little carried over from the parent book published 39 years earlier. The second book is really a plant population genetics book that synthesizes a life's study of plant evolution. It is uniquely Bob, both in the lucidity of the writing and in the presentation and articulation of his vision of evolutionary genetics.

QUANTITATIVE GENETICS

The field of quantitative genetics had a large impact on agricultural research in the 1940s and 1950s. The origins of quantitative genetics derive from R. A. Fisher's 1918 paper reconciling Mendelian genetics and Darwinian evolution by natural selection. Quantitative, or biometrical, genetics aims to partition phenotypic variation into genetic and environmental components and it provides a scientific basis for designing efficient schemes for selection. In later years Bob recalled that while a student at Wisconsin, he had been influenced by Sewall Wright, who along with R. A. Fisher was the other great architect of the field of quantitative genetics. It is clear from Bob's later recollections that he was anxious to move beyond lima bean breeding by mastering the skills of quantitative genetics. During the academic year 1954-1955, Bob found the opportunity to hone his skills in statistics and quantitative genetics by taking a year's sabbatical leave in Birmingham, England, to work with Kenneth Mather, one of the era's leaders in quantitative genetics. A few years later, in 1960, he returned to England to work at Oxford with Norman J. T. Bailey, a leading statistician in the field of mathematical genetics. These sabbaticals had an enduring impact on Bob's research directions.

In the middle 1950s Bob began to publish papers that attacked various biometrical issues of the day. One paper was devoted to maximum likelihood estimators for recombination, others focused on the analysis of various diallele crosses, and still others concerned the problem of estimating gene-environment interactions. He began publishing more frequently in broadly based genetics journals rather than in agricultural journals so that his papers would reach a broader audience of geneticists. He also continued to publish on applied topics throughout his career. One paper of this period that deserves special mention is an elegant dissection of the genetics of heading time in wheat (1965). In this paper Bob showed that a major gene controlled heading date, but he went beyond this to show how the remaining phenotypic variation in heading date could be resolved into additional genetic components, revealing the influence of multiple genetic factors of unequal effect. The paper pushed the approaches of quantitative genetics to their experimental limits. By this time Bob's research had evolved beyond the lima bean to exploit other plant species more appropriate for investigating basic questions of quantitative genetics. By the early 1960s Bob's lab was regarded as a leading lab for the study of plant quantitative genetics. Even as he achieved this goal, Bob was moving in new directions.

THE GENETICS OF INBREEDING POPULATIONS

Stimulated in part by his colleague G. Ledyard Stebbins, Bob began to investigate the genetics of inbreeding species. In his classic 1950 book, *Variation and Evolution in Plants,* Stebbins had claimed that inbreeding plant populations should be largely devoid of genetic variation. The argument put forward by Stebbins was that inbreeding leads to homozygosity and the superior inbred type should outcompete all other lines leading to a homogeneous population. Bob knew from his plant-breeding experiences that inbreeding crops, such as lima beans, had large stores of genetic variation and showed rapid genetic responses to selection. Stebbins had repeated what was the conventional dogma of the time, but this provided the stimulus for Bob to begin what became a classic series of experiments to characterize genetic variation in inbreeding plant species. Stebbins, for his part, encouraged this effort to look more deeply at the genetics of inbreeding species. The quest led Bob into an entirely new field, ecological genetics, which sought to combine population genetics with ecology, where Bob played a foundational role. It also began a series of papers on population studies in predominantly self-pollinated species that spanned a period of more than 20 years.

The studies of inbreeding populations led Bob from quantitative genetics into population genetics. Bob quickly established the leading program on plant population genetics of the 1960s, and he and his students found novel ways of approaching the fundamental questions of this field. One important innovation harked back to the composite cross populations of his early undergraduate days. At the time, population genetics was dominated by *Drosophila*, partly because the short generation time of *Drosophila* permitted experiments over many generations, thereby allowing the direct observation of evolutionary changes in gene frequencies. Bob had become the custodian of the composite cross populations, and he quickly realized that the populations he had helped synthesize in his youth would allow a multiple generation approach in longer-lived annual plant species as well. The basic reason rested on the fact that seed could be stored over a number of years, allowing an investigator to analyze gene frequencies in past generations. To see how this worked it is necessary to describe the system for propagating the composite cross populations. The practice was to advance the populations each year by growing a new generation under standard agricultural conditions at Davis, while also storing a portion of seed from each year's harvest for several years. The saved seed would then be rejuvenated by growing out a new generation every five years or so. This provided a parallel series of populations that represented early, intermediate, and late generations. By the early 1960s the oldest populations had about a 30-year history and the youngest had a history of only five or six generations. Because of this scheme, the barley and wheat composite cross populations provided a unique resource to follow changes in phenotypic traits, gene frequencies, and disease resistance loci over 30 or more generations.

Bob used every tool available to study genetic change in the composite cross populations, beginning with simple morphological polymorphisms and quantitative characters and moving on to isozymes and finally to restriction fragment length polymorphisms (RFLPs) near the end of his career. Bob was among the first to adopt the isozyme method when it appeared in the middle 1960s. Isozymes had an enormous impact, because for the first time they allowed the investigator to sample a large number of genes that coded for various enzymatic proteins. Prior to this, students of population genetics were limited to morphological variants, such as the seed coat color polymorphisms of lima bean or to quantitative traits where the underlying genes were impossible to resolve. Isozymes allowed one to sample many individual gene products and to ask questions about genome-wide levels of genetic variation. RFLPs offered the advantages of isozymes while also permitting the investigator to measure variation for portions of the genome that do not code for enzymatic proteins. Throughout his career Bob was always among the first to adopt new approaches to address scientific questions. He was undaunted by obstacles or by the investment of effort associated with acquiring new technologies.

Regardless of the experimental approach employed in studying the composite cross populations, substantial changes in trait or gene frequencies were always observed over time, and these were too large to be ascribed to genetic drift, leaving selection as the only plausible explanation. The next natural question was, could selection be quantified at individual loci? Theodosius Dobzhansky and Sewall Wright had developed approaches to the quantification of selection on inversion polymorphisms in *Drosophila pseudoobscura*, but these depended on the assumption of random mating. The basic estimation technique was to derive transition equations that predicted genotypic frequencies in one generation based on their frequencies in previous generations after accounting for the mating process. A set of weights that mapped the predicted frequencies onto the observed frequencies quantified the strength of selection.

Barley is a predominantly self-fertilizing plant, so the random mating assumption could not be employed. A quantitative theory of mating and a method to estimate the parameters of such a quantitative model was required. A quantitative theory, known as the mixed-mating model, which allowed for a mixture of self-fertilization and random outcrossing, had been published in 1951 by Fyfe and Bailey (the same Bailey that Bob had worked with on sabbatical in Oxford, England). Bob and his students employed this model to estimate the single outcrossing parameter that indexed the mixed-mating model and to derive transition equations to estimate selection in the composite cross and other populations. The technique for estimating the proportion of outcrossing relied on another important property of plants; the fact that one can easily collect numerous progeny of a single maternal plant as seed. With the use of marker genes it was possible to estimate the fraction of self-fertilization and its complement—the fraction of outcrossing—from family structured data.

Armed with a quantitative characterization of the mating process one could quantify selection at individual marker loci. But self-fertilization has another important consequence that rendered it impossible to attribute selection to the marker loci actually observed. Because self-fertilization leads to homozygosity, effective recombination is greatly reduced and any statistical associations among different loci decay slowly over time. Populations like the composite cross populations, with a relatively short history, would still retain statistical associations between loci from their initial composition. Bob and his students initiated the theoretical study of the behavior of linkage disequilibrium (the technical term for correlations between loci in allelic state) in mixed-mating systems in the middle 1960s. At a time when computer simulations were just beginning to be applied to genetic problems, they published an important simulation study describing the complex behavior of linkage disequilibrium in predominantly self-fertilizing populations. Later estimates of the magnitude of linkage disequilibrium in the composite cross and other populations showed that it was typically large. The conclusion was that chromosomal segments containing the marker loci were subject to strong selection in virtually all observed cases, but that one could not resolve selection to the level of individual loci.

Bob was not satisfied with the study of artificial populations. The question he sought to answer was the broader question concerning levels of genetic diversity in inbreeding populations of plants in nature. By the early 1960s he had launched a program to study natural populations of inbreeding plants, and this work included a broad variety of species, including *Avena* species (wild oats), other grasses native to California, such as fescue, and annual native California dicots, such as *Collinsia* species. These efforts began an intensive period of ecological genetics research that spanned nearly two decades. *Avena barbata*, the slender wild oat, was a particular target of investigation during this period. *A. barbata* is a naturalized component of the California oak savannah that was introduced into California during the Spanish Mission period from the Mediterranean basin (almost certainly from Spain). The time dimension is known, and this meant that genetic changes over a two-hundred- to three-hundred-year period could be documented.

Near the end of his life Bob recalled having been introduced to *Avena barbata* by W. W. Mackie; once again this powerful early influence determined a scientific direction, and it was a fortunate choice, because *A. barbata* showed markedly different patterns of evolution in different regions of California. As later shown by two of Bob's Spanish students (Marcelino Perez de la Vega and Pedro Garcia Garcia), these changes were not replicated in Spain, so they must have arisen since the introduction of *A. barbata* to California. Particularly dramatic were contrasting patterns of isozyme variation between the foothills of the arid Central Valley of California and the cooler and moister intermontane valleys of the costal strip. The arid regions were nearly monomorphic for a single multilocus genotype, while populations from the coastal regions exhibited substantial levels of variability. I was fortunate to play a role in these findings, and it was a wonderful way to start a research career.

INTERACTING GENETIC SYSTEMS

A pervasive theme of Bob's research and writing was the importance of interactions among genes, between genes and environments, and even among genotypes within populations. Bob believed that context was essential and that marginal effects were less important. I recall Bob attributing this belief in the importance of interactions to his early mentor W. W. Mackie. Regardless of the source, it clearly dominated Bob's thinking. This view went counter to conventional population genetics theory that is based on the notion that complex systems can be characterized by marginal gene frequency changes. It also went counter to quantitative genetics theory where additive effects were thought to account for most variation. To this day the importance of interaction remains an open question.

Beginning in the middle 1950s, Bob published experimental work on gene environmental interactions. In the 1960s he turned to the problem of interactions among genes at different loci. His approach of measuring linkage disequilibrium as a surrogate for gene interactions was stimulated by the theoretical calculations of R. C. Lewontin and K. Kojima giving the precise relationship between selection and recombination required for nonzero linkage disequilibria. These highly simplified models showed that only nonadditive selection over loci could retard recombination and maintain permanent linkage disequilibrium. Thus Bob focused on the estimation of linkage disequilibria in experimental plant populations as a means of detecting interactions. It later became clear that the existence of linkage disequilibrium is neither necessary nor sufficient for the existence of interlocus interactions, especially in inbreeding systems. Despite this, Bob did show that correlations among loci could be pervasive in inbreeding plant populations and that this would in turn affect their evolutionary potential.

Together with his students, Bob studied the impact of neighboring genotypes on the fitness of individual plants. This system of intergenotypic interactions creates a frequency dependent pattern of selection and widens the conditions for the maintenance of a genetic polymorphism. As with much of Bob's work, theoretical calculations were supplemented by direct measurements from experimental populations to provide a predictive framework. Bob was also a strong proponent of the idea that genetic mixtures would perform better than single pure lines in an agricultural context, although the evidence to support this view has been meager.

A LIFE'S ACCOMPLISHMENTS

As noted above, Bob worked in, and in some cases helped found, several distinct but related areas of plant genetics. He had an enduring impact on plant breeding, largely through his book but also through his early work in biometrical genetics. These contributions were later recognized through the DeKalb-Pfizer distinguished career award of the Crop Science Society and the Crop Science Award of the American Society of Agronomy. Bob was elected to the National Academy of Sciences in 1973, where he chose to affiliate with the genetics section and later with the section on population biology, evolution, and ecology after it was formed, rather than with the agricultural sections. This choice illustrates that his first love was genetics, despite a lifelong devotion to agriculture.

More than any other worker, Bob Allard is responsible for laying the rigorous experimental foundations for plant population genetics, and he played a major role in melding the union of ecology and genetics that emerged as ecological genetics. Perhaps his most enduring scientific legacy was the series on population studies in predominantly self-pollinated species. This series illustrated one of Bob's greatest strengths. He was first and foremost an empiricist who found innovative ways to test theory and to expand our empirical understanding of genetic systems. His belief in interaction ran counter to the dogma of his time and often led to intense arguments, but he never modified his views. He was passionate about his scientific views and at times the strength of his convictions seemed to overwhelm the available evidence. In retrospect, his intuition was excellent and his views have been largely vindicated.

Bob was a prolific teacher and mentor of graduate and postdoctoral students. Altogether he trained 56 Ph.D. students, and he hosted numerous visiting scientists and postdoctoral students; he also trained a host of M.S. students. He had a large number of international students, and many have become prominent figures in countries around the world. I recall students from all continents, and as a consequence he left a global intellectual legacy. He taught throughout his long tenure at UC Davis in both the Department of Agronomy and the Department of Genetics. He wrote a wonderful set of lecture notes on population genetics that were used in the introductory genetics course at Davis. During the late 1960s, I encountered his lecture notes as an undergraduate and immediately decided I wanted to study population genetics. He also taught in the introductory genetics course for many years as well as a graduate course in quantitative genetics and an undergraduate course in population genetics. He was not a classroom performer, but his lectures, like his writing, were clear and carefully organized to make a central point.

Around 1967 Bob became chair of the Department of Genetics at UC Davis, where he served with energy and skill. He played a major role in bringing Th. Dobzhansky and F. J. Ayala to Davis in the early 1970s and helped catapult the Department of Genetics to international preeminence. At its peak the department included among its faculty five members of the National Academy of Sciences. He also served on virtually every major committee of the university and for a period chaired the Davis division of the UC Academic Senate. Bob was an active member of the National Academy Sciences, where he chaired Section 27 for three years. He served on a number of National Research Council committees, including a committee that produced several volumes on managing global genetic resources. He was unstintingly generous with his time, and he served the university and the academy he loved with great devotion.

Bob retired in 1986 but remained very active. He published a remarkable number of research papers during his retirement, along with the new edition of his classic plant-breeding book. During this period, Bob and Ann Allard spent much of their time at their home at Bodega Bay on the northern California coast. He loved walking on the seaside cliffs examining plants, especially *Avena barbata,* and speculating about their unique adaptations to the California environment. He was always eager to entertain friends and colleagues; evenings with the Allards at Bodega Bay were very special events. Bob loved wine and was an accomplished student and collector of fine wines, so any dinner was resplendent with excellent wine. Bob finally had to leave his Bodega Bay home about two years before his death, owing to the onset of Alzheimer's disease and circulatory difficulties. He died on March 25, 2003, in Davis at the age of 83.

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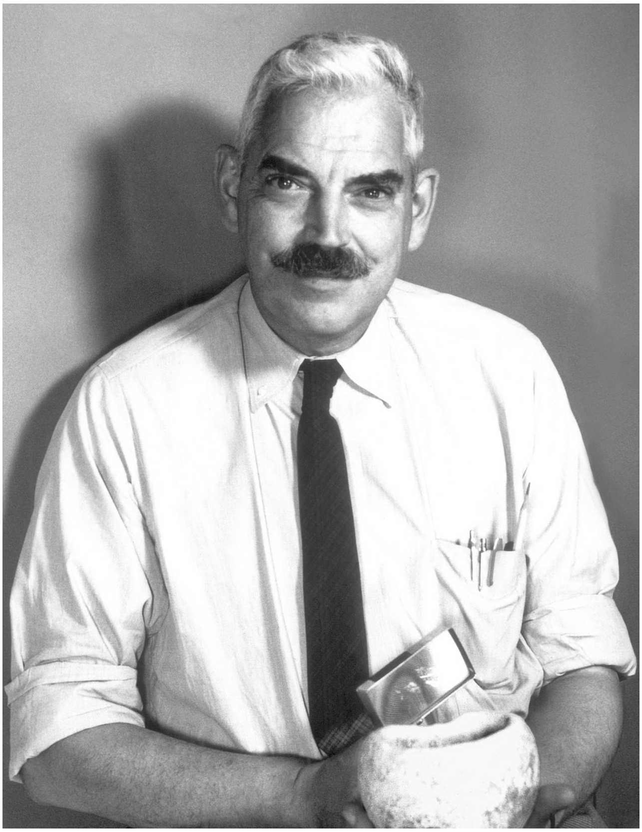

Robert J. Braidwood

ROBERT JOHN BRAIDWOOD

July 29, 1907–January 15, 2003

BY PATTY JO WATSON

ROBERT J. BRAIDWOOD, ELECTED to the National Academy of Sciences in 1964, WAS a central figure within the community of field archaeologists that elicited culture history from the earth at the middle of the twentieth century. The heroic age of world archaeology, which began prior to World War II, was dominated by legendary excavators, such as Howard Carter, Gertrude Caton-Thompson, Grahame Clark, J. Desmond Clark, Dorothy Garrod, Emil Haury, Jesse Jennings, Kathleen Kenyon, A. V. Kidder, Richard ("Scotty") MacNeish, Mortimer Wheeler, and Leonard Woolley. At the time of his death in January 2003, Bob Braidwood was the last survivor of this illustrious group.

Robert John Braidwood was born on July 29, 1907, in Detroit, Michigan, a second-generation descendant of Scottish immigrants, both his mother's and his father's parents having come to the United States during the nineteenth century. As a boy, Bob Braidwood worked in his father's pharmacy after school and held occasional part-time jobs at a grocery store and a bank. During the summer before he left home to attend the University of Michigan as a freshman undergraduate, he held an apprentice card in a carpenters' union. His carpentry skills were often deployed in archaeological field camps decades later and stayed with him for the whole of his life. In the summer of 1978, by which time he was emeritus at the University of Chicago, he visited an excavation that William Marquardt and I were directing in Kentucky. He took pity on the graduate student geoarchaeologist who was struggling to encase a set of bulky sediment cores for transportation to the palynological laboratory at the University of Minnesota. Taking charge of the hammer, nails, and pieces of plywood collected for the job, he assembled the requisite number of sturdy sample boxes in a matter of minutes.

Bob Braidwood's career in anthropological archaeology began shortly after he completed a degree in architecture at the University of Michigan in 1929 and spent several months in an architectural office. The impact of the Great Depression made a future in architecture highly problematic, so he returned to Michigan to undertake coursework in two other areas that had interested him as an undergraduate: ancient history and anthropology. One of his ancient history classes was taught by Professor Leroy Waterman, a philologist and an expert on the correspondence of Neo-Assyrian rulers, who was then directing excavations at Tell Umar (ancient Selucia-on-the-Tigris), a large site south of Baghdad. At some point during that course, Waterman required each student to prepare a chronological chart showing highlights of history and cultural development for the ancient Near East. Because of the training he had received in drafting and lettering during his brief career in architecture, Braidwood—according to the story he told his students several decades later—produced such an impressive piece of graphics that Waterman invited him to join the University of Michigan's Selucia archaeological expedition as an architectural surveyor for the nine-month field season of 1930-1931. As a result of this experience Bob published a paper on Parthian jewelry, obtained the data for his master's thesis (on the economic organization of the Selucid empire), and became an archaeologist.

Bob earned his B.A. in 1932 and M.A. in 1933 at the University of Michigan before being hired by the Oriental Institute, University of Chicago, as a field assistant for the Syrian Expedition to the Amuq (the Plain of Antioch, which became part of Turkey in 1939). Bob held this position for five years (1933-1938) until work by the Syrian Expedition ceased. On his return by steamship to the United States from Syria following the 1936 Amuq season, he met someone he had first encountered during his undergraduate days at the University of Michigan: a young woman named Linda Schreiber, who was working as a buyer for a large downtown Chicago department store. As the culmination of the ensuing shipboard romance, they were married the following January. Bob and Linda Braidwood worked together at home and abroad for the next 66 years, forming a formidable team as they organized and managed archaeological field crews in Syria, Iraq, Iran, and Turkey, as well as coauthoring and coediting dozens of publications describing and discussing results of their work.

Linda joined the Amuq field staff for the last Syrian season in 1938, and then both Braidwoods enrolled in graduate coursework during the fall term at the University of Chicago. Bob was pursuing a doctoral degree under the supervision of Henri Frankfort in the Department of Oriental Languages and Literatures at the Oriental Institute, but a third of his coursework was in the Department of Anthropology across the street from the institute on the main Chicago campus. Linda earned an M.A. at the institute in 1943, but was not allowed to pursue a Ph.D. because Bob was by then a part-time faculty member of the university. Nepotism rules forbade what was viewed at the time as a conflict of interest when a professor and a graduate student were affiliated with the same university department, but Linda did hold a career-long research associate appointment at the Oriental Institute.

When Bob Braidwood was hired by the Oriental Institute in 1933, he was retained simply as a field assistant with the Syrian Expedition staff, which meant that he spent nine months (fall to spring) every year digging in Syria. He, like several others with the same kind of appointment, was on his own and was unsalaried during the three summer months when excavations were suspended. The Oriental Institute, like the University of Chicago itself, was funded primarily by John D. Rockefeller. Rockefeller had been persuaded by James Henry Breasted (the first American to obtain a Ph.D. in Near Eastern philology, in his case from the University of Berlin in Egyptian hieroglyphs and Egyptology) that a scholarly center dedicated to research on the ancient Oriental world (meaning Egypt, the Levant, and greater Mesopotamia) was an essential component for the great university Rockefeller was helping to build in south Chicago. In 1919 Rockefeller accepted Breasted's proposal, and funded construction of the Oriental Institute building during the 1920s. In his role as director of the new institute and still spending Rockefeller money, Breasted hired the best Assyriologists, Egyptologists, and other scholars of the eastern Mediterranean Bronze and Iron ages that he could find, many coming from Germany and other parts of Europe. These people were, for the most part, epigraphers and philologists who spent their research time transliterating, translating, and interpreting cuneiform or hieroglyphic writings recovered from an array of archaeological sites in western Asia and Egypt.

Breasted also established a major field program with twelve archaeological expeditions working annually in five countries: Egypt (six expeditions), Iran (one expedition), Iraq (three expeditions), Palestine (one expedition), and Syria (one, the Amuq expedition). In 1941 Bob Braidwood was hired part-time by the University of Chicago to be the Oriental Institute's sole prehistorian, a position that was made full-time in 1945, subsequently with joint appointment in the Department of Anthropology, an arrangement he held until his retirement from the University in 1978. Perhaps the best aspect of this job was the agreement that every third year Bob was to be off campus and out of residence, digging at prehistoric sites in western Asia.

ARCHAEOLOGICAL FIELDWORK IN THE NEAR EAST

Braidwood's experience with the Syrian Expedition of the Oriental Institute was similar in some ways to his nine months with the Selucia project, in that the work of both was centered upon large-scale excavation of huge mounds (*tells*, *tepes*, or *höyüks*). Such mounds mark the locales where prehistoric, protohistoric, and historic communities lived and died, later ones having been constructed atop the ruins of earlier ones. Given the aridity of western Asia, most of the architecture encountered by the archaeologists was sun-dried mud brick (adobe), a very economical and flexible technology that facilitates remodeling, or leveling and rebuilding, but is quite challenging to excavate. Fortunately for fledgling field assistants like the young Bob Braidwood at Selucia, the native dig *ustas* (master excavators) were well trained and highly skilled at disentangling mud-brick wall lines from the cultural deposits that surround them.

Other than day-to-day supervision and recording at various portions of the excavation, Braidwood accomplished two major pieces of work during his five years in the Amuq. He was given responsibility for deep soundings of major mound sites being dug by the expedition, the most important such vertical exposure being at Tell Judaidah. As he related the story many years later, the expedition director told him to take a crew, dig down to the bottom of the mound, and describe "the earliest stuff we've got." Braidwood did this by means of a step trench, dividing the stratified cultural remains (mostly on the basis of changing pottery types) into a long series of phases, each of which he labeled with letters of the alphabet, "A" being the earliest. His detailed account of this sequence, which he related to relevant artifacts and architecture at other Near Eastern sites, was presented in his doctoral dissertation, "Comparative Archeology of Early Syria," which was completed in 1942 and submitted in final form the following year.

Braidwood's other project was a regional survey of the entire Amuq to locate and date as many of the other sites in the area as possible. He made good use of the phase system defined in his vertical soundings to order the survey sites chronologically by means of artifacts (especially potsherds) on their surfaces. This information was published in 1937 as an Oriental Institute monograph, *Mounds in the Plain of Antioch.*

A third notable result of Bob Braidwood's years in the Amuq was the beginning of his career-long association and friendship with one of the expedition workmen, Abdullah Said Osman al-Sudani, a bright, knowledgeable young Egyptian. Bob and Linda Braidwood and Abdullah al-Sudani worked together for the next 30 years, first in Syria, then in Iraq, where Abdullah was site supervisor and all-around facilitator for the Oriental Institute's Iraq-Jarmo Project.

During the academic years from 1938 until the end of World War II, when the Braidwoods were graduate students at the University of Chicago, they participated in a seminar conducted by Bob Braidwood's dissertation professor, Henri Frankfort. The legendary Frankfort seminar, still a vivid memory that was often evoked during the 1950s-1960s by the Braidwoods and other Oriental Institute personnel who had survived it, met weekly for nine months each year: from the beginning of fall term to the end of spring term. The goal Frankfort set for the class was to begin at the beginning of the archaeological record as then known in the Near East (Middle Paleolithic to late Upper Paleolithic [Late Pleistocene] and the Natufian of the Mt. Carmel rockshelters [variously regarded as Mesolithic or early Neolithic]) and continue to 2000 B.C. This experience, together with his dissertation research on early deposits underlying the Amuq mounds, provided the data for Braidwood's production in 1945 of what he called "the gap chart," a chronological diagram he drew up as a pedagogical device that highlighted a significant lacuna (gap) spanning several thousand years between the last mobile Paleolithic hunter-gatherers camping in rockshelters, and the first appearance of the earliest agropastoral villages, such as those represented by the Amuq A phase, for example, and the site of Hassuna in northern Iraq (1946; Lloyd and Safar, 1945). Braidwood was especially intrigued by that lacuna because he had been strongly influenced, early in his student days at Michigan, by the writings of V. Gordon Childe (Childe, 1928, 1934, 1936, 1942) about the post-Pleistocene Agricultural Revolution. Childe emphasized the great significance of this achievement, whereby human groups first domesticated plants and animals to invent a new form of subsistence that laid the economic foundations for the rise of civilization. The gap Braidwood's chart delineated coincided exactly with the time when the first food-producing villages—ancestral to Amuq A phase communities and to places like Hassuna—must have appeared.

It was this gap that Bob and Linda were eager to address in their first Oriental Institute fieldwork after World War II. Although they had hoped to return to northern

Syria, they were persuaded by cuneiformist and Sumerologist Thorkild Jacobsen, who was then director of the Institute, that the political situation in Iraq was more stable and more amenable to long-term archaeological work than was that in Syria. Hence, the Braidwoods' initial foray into the research trajectory that absorbed them for the rest of their careers began in 1948 in northern Iraq rather than northern Syria. Following information given them by the Directorate of Antiquities in Baghdad, they applied for and were granted an excavation permit for a site similar to Hassuna called Matarrah, and a sondage (test dig) permit for a second site, Jarmo. Jarmo, in the Kurdish hills near the town of Chemchemal, turned out to be the more interesting of the two because it had a major preceramic component, yet certainly seemed to be a sedentary community.

Following their 1948 season at Matarrah and Jarmo, the Braidwoods—accompanied, as they had been in the previous Iraqi season, by their children, nine-year-old Gretel and six-year-old Douglas—launched the Iraq-Jarmo Project and excavated several units at Jarmo during a nine-month period in 1950-1951. Test excavations were also carried out by Braidwood's colleague, Bruce Howe, at two earlier sites: a rockshelter (Palegawra), and an open site (Karim Shahir) with a thin deposit but one that seemed to be earlier than Jarmo. Although a geologist had spent part of the 1950-1951 season working with the archaeologists in and around Jarmo, for the next (1954-1955) Jarmo season, Braidwood was determined to have a range of experts in the natural sciences on his staff. He needed botanical and zoological expertise to identify the remains of wild and domestic wheat and barley, sheep, goats, pigs, and dogs, and to provide information about prehistoric climate and environment that would complement information from Herbert Wright, the geologist. One reason Braidwood was so interested in Jarmo was its location in what he came to call "the Hilly Flanks of Breasted's 'Fertile Crescent,'" an upland region with sufficient rainfall to make irrigation unnecessary and to provide suitable habitats for wild ancestors of the first domesticated plants and animals. It seemed likely that archaeological remains of the first farmers and pastoralists would be found in areas where the ancestral species were naturally present.

With the support of a new government research-funding agency, the National Science Foundation, Bob Braidwood assembled the first interdisciplinary team to address agro-pastoral origins on the ground in the Near East. Besides Wright the geologist, this group included paleoethnobotanist Hans Helbaek, zoologist Charles Reed, and radiocarbon expert Fred Matson, as well as several archaeologists (Bob Braidwood and Linda Braidwood, Bruce Howe, and field assistants Vivian Broman and Patty Jo Andersen) and four camp managers (Mayo and Beverly Schreiber, Margaret Matson, and Rhea Wright). The 1954-1955 field season of the Iraq-Jarmo Project included a several-month period of site survey during the fall in a region northwest of Jarmo, and a three-month spring season back at Jarmo and Karim Shahir.

The Braidwoods planned further work at Jarmo and at several sites near it for the fourth Iraq-Jarmo Project season, but the nationalist revolution that took place in Iraq during the summer of 1958 resulted in significant political instability, especially in the northern Kurdish area, where Jarmo is located. As a result of this situation, the Iraq-Jarmo Project personnel moved operations across the border in 1959-1960 and became the Iranian Prehistoric Project, directed by Robert Braidwood and codirected by a prominent young Iranian archaeologist, Ezat Negahban, who had been a graduate student at the Oriental institute during the early 1950s. Beginning with a fall survey in portions of the Kermanshah valley, the team carried out excavations at a nearby rockshelter (Ghar Warwasi), and at two open sites (Tepe Asiab [somewhat similar to Karim Shahir] and Tepe Serab [yielding remains that resemble the later Jarmo materials, and possibly occupied post-Jarmo as well]) while ethnoarchaeological work went on at several contemporary villages (Watson, 1979), and a very important palynological study was begun at lakes in the Zagros Mountains. Results of the latter investigation, directed by Herbert Wright, eventually resulted in radically altering the understanding of Holocene climate and environment, not only in the Hilly Flanks but also in the entire Near East (van Zeist and Bottema, 1977, 1991; Wright, 1983, 1998).

Another significant result of the Iranian Prehistoric Project was the work carried out elsewhere in Iran during several subsequent seasons by two young staff members of the 1959-1960 Oriental Institute expedition, Frank Hole and Kent Flannery, who initiated their own research in the Deh Luran valley of southwestern Iran in 1961 (Hole et al., 1969).

Following the Iranian season, Braidwood's group moved once more as the result of a very appealing arrangement negotiated by an energetic and persuasive colleague: Professor Halet Çambel, head of the Prehistory Department at Istanbul University. The Joint Prehistoric Project, Istanbul-Chicago, codirected by Bob Braidwood and Halet Çambel, was established in 1963 and began fieldwork during 1963-1964 in southeastern Turkey. After an initial regional survey, the project's focus was largely on the remarkable village site of Çayönü, several hundred years older than Jarmo but much fancier architecturally and artifactually (1980, 1982). During the 1968 and 1970 field seasons, however, intensive surface survey and subsequent test excavations were carried out at a nearby, somewhat younger site: Girikihaciyan (Redman and Watson, 1970; Watson and LeBlanc, 1990).

Beginning in 1978, Wulf Schirmer led a team from West Germany's Institut für Baugeschichte, Karlsruhe University, in recording and interpreting the complexities of buildings and building sequences at Çayönü. Research on plant remains was carried out by Jack Harlan, Robert Stewart, and Willem van Zeist; Charles Reed, John McArdle, Richard Meadow, and Barbara Lawrence applied their zoological expertise to the animal bones; Gary Wright and Richard Watson surveyed several obsidian sources thought to have been quarried prehistorically (obsidian [volcanic glass] was highly valued and widely traded throughout western Asia and the Aegean); and Robert Megard analyzed microfauna from lakes and ponds for detailed paleoclimatic evidence. As had been the case in both Iraq and Iran, participation by these collaborative scientists was funded primarily by the National Science Foundation.

Çambel and the Braidwoods continued research at Çayönü until 1989, when Halet retired, and the Joint Prehistoric Project directorship was turned over to a former student of hers, Professor Mehmet Özdoğan of Istanbul University.

HIS LEGACY

Bob and Linda Braidwood died of pneumonia within hours of each other on January 15, 2003. In April of that same year, Halet Çambel, Mehmet Özdoğan, and another friend and colleague of the Braidwoods from Istanbul University, Güven Arsebük, attended the memorial service at the University of Chicago. All three scholars referred to the Braidwoods' dedication to their work and to the unusual strength and productivity of the collaborative Turkish-American project they helped initiate. Together with the large international group of students and young colleagues who have participated in Oriental Institute Prehistoric Project expeditions (over the years staff members have come from

England, France, Germany, Greece, Iran, Italy, Korea, the Netherlands, and Turkey as well as the United States), the Joint Turkish Prehistoric Project is one of the Braidwoods' major legacies to their discipline.

There are several other significant and lasting achievements to the credit of Robert J. Braidwood. The chronological sequence he delineated when a young staff member of the Oriental Institute Syrian Expedition, for example, has stood the test of time admirably, and is still centrally referred to in discussions of prehistory, protohistory, and early Bronze Age archaeology in the Levant. His survey of the Amuq plain and the regional perspective from which the survey derived were highly innovative at a time when virtually the entire field of Near Eastern archaeology centered upon site-oriented research: major excavations by hundreds of loosely supervised workers at very large mounds, preferably sites that were mentioned in the Bible or in cuneiform, hieroglyphic, or other archives of the Bronze and Iron ages.

Braidwood's primary contribution, however, is widely recognized to be his initiation of systematic, international, interdisciplinary field research on agropastoral origins. In 1995 the Society for American Archaeology (a large, powerful professional organization, which he respected but never joined because all his own work had been in the Old World) honored Robert J. Braidwood with its Fryxell Medal for distinguished interdisciplinary research in archaeology. That award and the symposium in his honor that accompanied it highlighted Braidwood's most significant and best known contribution to world scholarship: the theoretical and methodological approach he devised and that he and Linda applied to their work at Jarmo. The Iraq-Jarmo Project was the first systematic, empirical attempt to recover solid evidence—animal bones, plant remains, geological observations enabling paleoenvironmental reconstructions—about the indigenous origins of a prehistoric agropastoral economy. The basic research design Braidwood created in the early 1950s has long since become standard operating procedure, and the agropastoral origins problem he defined in the late 1940s has been taken up by interdisciplinary teams around the globe to track food-producing revolutions very different from the one he and Linda pursued for nearly half a century. The expertise in floral, faunal, and geological remains that Bob had to beg, borrow, or bootleg throughout most of his career is now lodged in a series of formal subdisciplines (archaeobotany, geoarchaeology, and zooarchaeology) for which his field projects in Iraq, Iran, and Turkey provided powerful stimuli.

Bob and Linda Braidwood were an archaeological team without parallel in their dedication to answering the questions that caught Bob's imagination in his student days at Michigan, when he first read the speculative writings of V. Gordon Childe, and then again in 1945 when his gap chart so clearly displayed a total lack of archaeological data about the critical portion of the post-Pleistocene record. Childe argued persuasively that the Agricultural Revolution in western Asia was enormously significant in that it provided the foundations for urban civilization in ancient Mesopotamia, but he paid scant attention to the questions that fascinated the Braidwoods throughout their careers: where, when, how, and why did that revolution take place? Their work in Iraq, Iran, and Turkey did not provide final answers to those queries. Their research, however, and that of dozens of other archaeologists all over the world—inspired by their results and by the enthusiasm and joy they and their collaborators poured into the search for answers—has enormously advanced archaeological theory, method, and substantive knowledge about a major transition in the human past.

GRETEL BRAIDWOOD, RAY TINDEL, AND WILLIAM SUMNER provided some of the information included in this memoir. See also "Archaeological Retrospect 2" (1981), my biographical memoir for Robert John Braidwood in the *Proceedings of the American Philosophical Society,* vol. 149, no. 2, as well as obituaries in the *American Anthropologist,* vol. 106, no. 3 (by P. J. Watson); the *American Journal of Archaeology*, vol. 107, no. 4 (by Andrew Moore); the *Journal of Anthropological Research*, vol. 59, no. 2 (by P. J. Watson); the *New York Times* for January 17, 2003 (by Stuart Lavietes); and *Neo-Lithics*, Jan. 2003 (by Geoffrey Clark). The Braidwood memorial issues of two Turkish journals are very informative: *Arkeoloji ve Sanat* 113 (2003) and *Tüba-Ar* (Turkish Academy of Sciences Journal of Archaeology) 7 (2004). Additional sources are *The Encyclopedia of Archaeology: The Great Archaeologists* (edited by Tim Murray), vol. 2, pp. 495-505, 1999; Linda Braidwood's *Digging Beyond the Tigris* (1953), Robert Braidwood's *Archeologists and What They Do* (1960), and L. and R. Braidwoods' essay, "A Highly Successful Collegiality" in *Light on Top of the Black Hill: Studies Presented to Halet Çambel* (eds. G. Arsebük, M. Mellink, and W. Schirmer, pp. 189-194, 1998).

CHRONOLOGY AND PROFESSIONAL RECORD

1907	Born July 29 in Detroit, Michigan
1926-1929	Attended University of Michigan College of Architecture
1929	Junior member of an architectural firm in Detroit
1930	January: returned to University of Michigan for coursework in ancient history and anthropology
1930-1931	Surveyor and artist with the University of Michigan Selucia-on-the-Tigris expedition
1932	B.A. in anthropology and ancient history, University of Michigan
1933	M.A. in anthropology and ancient history, University of Michigan

1933	Summer: topographic surveyor for the Department of Anthropology's excavations in Fulton County, Illinois
1933-1938	Field assistant on the staff of the Oriental Institute's Syrian Expedition, University of Chicago
1934	Summer: attended University of Berlin
1937	January: married Linda Schreiber
1938-1942	Doctoral studies at the Oriental Institute and Department of Anthropology, University of Chicago
1941	Summer: Field supervisor, Field Museum of Natural History excavations at the SU site near Reserve, New Mexico
1942	Ph.D. in Oriental Languages and Literatures (with considerable coursework in Anthropology), Oriental Institute, University of Chicago
1945	Permanent faculty position at the University of Chicago (Oriental Institute)
1948, 1950s	Director of the Iraq-Jarmo Project fieldwork in northern Iraq
1959-1960	Director of the Iranian Prehistoric Project fieldwork in northwestern Iran
1960s-1980s	Codirector, then advisor and consultant to the Turkish Prehistoric Project in southeastern Turkey
1978	Formal retirement from the Oriental Institute and Department of Anthropology, University of Chicago

MEMBERSHIPS

American Anthropological Association
Archaeological Institute of America
International Union of Pre- and Protohistoric Sciences

Foreign correspondent or honorary fellow of the following:
Académie des Inscriptions et Belles Lettres, Institut de France
Deutsches Archaeologisches Institut
Osterreicheschie Akademie der Wissenschaften
Instituto Italiana di Preistoria e Protostoria
Jysk Arkaeologisk Selskab
Kungl. Vetenskaps-och Vitterhets-Sammhallet i Goteborg
Society of Antiquaries of London

ELECTIVE OFFICES HELD IN PROFESSIONAL ORGANIZATIONS

1961-1964 Executive Board, American Anthropological Association

1975-1980 Committee on Membership IV, American Philosophical Society

Permanent Council, International Union of Pre- and Protohistoric Sciences

HONORARY DOCTORATES

1971 Indiana University, Sc.D.

1975 University of Paris I (Sorbonne), Docteur

1984 University of Rome, D. Lit.

OTHER AWARDS AND HONORS

1963 Elected to American Philosophical Society

1964 Elected to National Academy of Sciences

1966 Elected to American Academy of Arts and Sciences

1971 American Anthropological Association Distinguished Lecturer

1971 Archaeological Institute of America Gold Medal for Distinguished Archaeological Achievement

1982 Festschrift, *The Hilly Flanks and Beyond: Essays on the Prehistory of Southwestern Asia*, presented to Robert J. Braidwood, November 15, 1982. Edited by T. Cuyler Young Jr., Phillip E. L. Smith, and Peder Mortensen, and published in 1983 as *Studies in Ancient Oriental Civilization* No. 36. Chicago: The Oriental Institute of the University of Chicago.

1995 Society for American Archaeology Fryxell Medal for Distinguished Contributions to Archaeology Through Interdisciplinary Research

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1950

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1952

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1953

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1954

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1963

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HARMON CRAIG

March 15, 1926–March 14, 2003

BY KARL K. TUREKIAN

HARMON BUSHNELL CRAIG (he never used his middle name) was born in the borough of Manhattan in New York City on March 15th, 1926. He died on March 14th, 2003, a day short of his seventy-seventh birthday. Craig was the product of two major forces in his life. His father, John Craig Jr., was from a family long in the theater as actors, directors, and producers. Indeed, John Craig's major activities, after his heroic involvement in World War I, were in running theaters in the northeastern United States. Young Harmon was surrounded by a theatrical crowd during his early childhood. His mother came from a long line of activist Quakers, who, starting before the Civil War, established schools for freed slaves. This activity moved the family from its initial homestead in Virginia westward, finally to Kansas. The influence of his mother's ethos permeated young Harmon as his mother fed his inquisitive mind with books on a wide range of subjects, especially those heroic and exploratory in nature. It was the blending of the thespian and the Quaker ethos that shaped young Harmon in his early years and set the behavior pattern of his later life.

Harmon's youthful love of adventure, adventurers, and science blossomed into a career in the earth sciences when he discovered fossils in a rock on a family outing. He went off to the University of Chicago as a freshman with a clear idea of pursuing studies in geology. World War II interrupted his education. He went off to an officer's training program in the navy and eventually joined the fleet in Norfolk, Virginia. He returned to the University of Chicago after demobilization, and his future scientific life was shaped there.

After World War II the faculty of the University of Chicago, weary of their part in the development of the atomic bomb, turned to research in areas of the most esoteric sorts. With mass spectrometers in place, Harold C. Urey and his students, postdocs, and research collaborators delved into the arcane worlds of determining the warmth of an ocean 100 million years ago, determining the ages of rocks and the Solar System, and exploring the chemistry of the Universe. It was in this hotbed of national-defense-irrelevant research that Harmon Craig found himself. An undergraduate geology major at the University of Chicago, he was propelled into this world of geochemistry and cosmochemistry without waiting to get his undergraduate degree.

The measurement of ancient sea temperature depended on analyzing carbon dioxide released from calcium carbonate fossils and measuring the relative masses of carbon dioxide composed of ^{18}O and ^{16}O. The constancy of the carbon isotope loading on the carbon dioxide was tacitly assumed. Craig, for his thesis, measured the natural variability of $^{13}C/^{12}C$ to establish the baseline for all future studies involving the carbon system.

The independent discovery of natural radioactive ^{14}C by W. F. Libby at the University of Chicago immediately led to the application of ^{14}C to dating in archaeology and Pleistocene geology. The stable carbon isotope study from Craig's thesis allowed for corrections due to mass fractionation and permitted the proper determination of radiocarbon ages (later to be corrected to calendar years by accommodating the variations in the initial $^{14}C/^{12}C$).

Craig's thesis is still today the primary citation for all studies involving variations in $^{13}C/^{12}C$ in natural materials, Cited in studies ranging from the establishment of food chains to identifying sources of ancient marbles for statues, this remarkable thesis was the harbinger of the impact Craig would have in various areas of geochemistry and cosmochemistry.

In the quest for the best measure, using meteorites, of the composition of the Solar System, the common assumption was that there was a uniform composition of the most likely nonvolatile raw material of the Solar System, chondrites. There was a need for criteria by which bad analyses of chondrites could be systematically identified and separated from the worthy ones; that is, meteorites modified by weathering had to be rejected so that the true makeup of meteorites, in particular the chondrites, could be ascertained. With his mentor Harold Urey, Craig discovered that once the veil of quality certification had been rent, the chondrites fell into at least two major groups. The Solar System was not so uniform after all. This discovery, later affirmed in several additional ways by others, gave us a totally new view of how and from what materials planets formed.

In 1955 the eastern universities were not yet ready to accept the strange new world of geochemistry heralded primarily by Harold Urey and his friends, but California was not afraid to go where no man had gone before. Caltech, through the wisdom of Robert Sharp and Harrison Brown, hired a bevy of University of Chicago geochemists. The Scripps Institution of Oceanography—mainly through the foresight of Roger Revelle, its director—brought in Craig from Chicago.

Back then, instruments were not built in a day. As Craig was tooling up, he solved the fundamental problem of the fate of carbon dioxide in the atmosphere and the oceans. His theoretical solutions are valid to this day. Indeed, they anticipated the program for atmospheric CO_2 measurements begun at Scripps by C. D. Keeling in 1957 at the instigation of Roger Revelle.

Craig decided that somebody had better figure out all the controls on oxygen and deuterium isotopes in the hydrologic cycle, especially if these isotopes were going to be used for paleoenvironmental reconstructions. In two elegant papers that resulted from his meticulous treatment of the problem for an appreciative Italian audience at Spoleto, he laid out the entire framework for discussing the role of kinetics and equilibrium in determining the isotopic composition of the hydrosphere, including the oceans. (These papers are not generally available in the common literature; neither are J. Willard Gibbs's classic thermodynamics papers, which were published in an obscure Connecticut journal.) These Spoleto papers are the fundamental documents that all atmospheric geochemists as well as hydrologists and oceanographers turn to for guidance in many aspects of light isotope geochemistry.

He established the meteoric water line, which defines the unique linear relationship between hydrogen and oxygen isotope ratios in natural terrestrial waters. He also discovered the oxygen isotope shift in geothermal and volcanic fluids, which showed (contrary to prevailing ideas) that the water in these fluids is overwhelmingly meteoric in origin. This work provided the basis for studies of water-rock interactions in geothermal systems and in hydrothermal vents.

Craig and his students subsequently studied the isotopic composition of atmospheric and dissolved oxygen and variations in the composition of dissolved gases. This work led to a method for determining biological oxygen production and consumption in the ocean mixed layer, as distinct from physical effects, and thus to a better quantification of biological primary production rates in the oceans.

In 1967 Henry Stommel suggested to a bunch of geochemists at a meeting at Woods Hole that it was about time that some scientists implemented a systematic study of the geochemistry and oceanography of all the oceans. With the new tracers and chronometers available to geochemists, this was the right moment to embark on this daunting enterprise. George Veronis let the group of geochemists get together with his theoreticians meeting at the Geophysical Fluid Dynamic Summer Institute to begin the planning. It became obvious to all who participated in the summer session that the leaders of what ultimately was to be called the Geochemical Ocean Sections Study (GEOSECS) should be Wallace Broecker of the Lamont-Doherty Earth Observatory, Harmon Craig of Scripps, and Derek Spencer of the Woods Hole Oceanographic Institution. With the help of many other geochemists the program did not self-destruct as some people thought (or hoped?), but rather accomplished its main goals.

GEOSECS spawned a number of important projects, many of which continue as follow-ups to this day. Craig was interested in the rate of turnover of the oceans. Fritz Koczy had suggested that ^{226}Ra with a 1,620-year half-life might be a good tracer of circulation, being introduced at the ocean bottom from sediments and making its way up with the water to the surface, decaying along the way. Edward Goldberg of Scripps had suggested that the daughter of ^{226}Ra, ^{210}Pb, could be measured as a surrogate. When Craig and his colleagues pursued this path, they discovered that ^{210}Pb was particle reactive and removed from the ocean by settling. Indeed, all the elements in the ocean that were particle reactive like ^{210}Pb would have similar distributions and the removal from surface to depth and ultimately into the sediments occurred.

Another incorrect assumption was the expectation that ^{4}He would be released from the ocean bottom. The expectation was to use atmospheric helium with its ^{3}He dissolved in seawater in an isotope dilution experiment to measure the excess ^{4}He putatively released from sediments. When Craig collected an ocean water profile and the talented Brian Clarke of McMaster University—who developed a technique for measuring ^{3}He/^{4}He—measured this profile, the astounding result was that it was ^{3}He that was in excess—not ^{4}He. This discovery of primordial ^{3}He in the oceans was made at the same time that I. N. Tolstikhin discovered primordial ^{3}He in hot springs in the Kuriles. The consequences of the oceanic discovery impacted not only the tracing of ocean circulation but also the understanding of the way the mantle expresses itself at ocean spreading centers and ocean island basalts. The discovery of excess ^{3}He in the oceans from this productive collaboration was exploited in every way by Craig, his students, and his postdocs with many additional remarkable discoveries resulting.

Craig's interests were not restricted to the oceans and the rocks at their boundaries; he also sought to understand the record of atmospheric changes recorded in cores from the Antarctic and Greenland ice sheets. He was one of the earliest workers to study gases trapped in glacier ice, and he showed that atmospheric methane has roughly doubled due to human activities over the past 300 years. He was also one of the first to study the geochemistry of atmospheric nitrous oxide and to work on the production, rate of increase, and isotopic budget of this natural and anthropogenic modulator of Earth's protective ozone layer. More recently his work focused on the physics and chemistry of gases in polar ice cores, including pioneering work on the gravitational separation of gases and isotopes within the permeable firn layer, and on the gravitational separation of rare gas isotopes as a measure of firn temperatures and thicknesses. This work is fundamental to the reconstruction of past atmospheric composition and isotopic variations based on measurements of gases in polar ice, and plays an important role in continuing efforts to understand past climatic change.

In one of his last papers Craig made sense of the ^{32}Si measurements made in the Geochemical Ocean Sections Study. Some scientists saw in the original measurements a hopelessly flawed set of data when tested with a simple model. Craig and his coauthors—including Somayajulu, who initially made the measurements and was rightly indignant that the quality of his measurements was challenged—wrote a paper titled "Paradox Lost: ^{32}Si and the Global Ocean Silica Cycle," wherein the role of mixing of two sources of silica trapped by the collecting fibers explained the results and justified the measurements made by Somayajulu. So we see the man who's eye for recognizing quality measurements first showed up in the paper on meteorites was active in deciphering a major marine geochemical problem.

Craig influenced many areas as a result of his brilliance as a field observer, his skill and meticulousness as a measurer, and his genius as a profound theoretical thinker. These qualities, when found in one person, make that person able to improve our understanding of Earth in all its facets with the strength of a whole army. Yet this one-man army was not acting alone. In everything he did he was accompanied and encouraged by his wife, Valerie. Her patience with Craig's perennially searching mind, his friends with diverse qualities and interests, and the system in which she and her husband ultimately triumphed made the Craig enterprise one of inevitable success.

Craig's success was recognized and rewarded by a number of prestigious awards, including the Balsan Prize, the Vetlesen Prize, the V. M. Goldschmidt Medal of the Geochemical Society, the Arthur L. Day Medal of the Geological Society of America, and the Arthur L. Day Prize and Lectureship of the National Academy of Sciences. He was elected to the American Academy of Arts and Sciences in 1976, and elected to the National Academy of Sciences in 1979. The University of Paris awarded him an honorary degree (an interesting follow-on to his father having received the Croix de Guerre from the French for his bravery in World War I). His alma mater, the University of Chicago, also awarded him an honorary doctorate while denying him an ex post facto bachelor's degree.

I thank John Craig III and Valerie Craig for insights into Harmon Craig's career throughout his productive life. I have borrowed extensively from an obituary that I wrote for *Nature* and one that Ray Weiss wrote for the *Transactions of the American Geophysical Union (EOS)*.

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Photo Courtesy of the American Institute of Nutrition Archives, Vanderbilt University

George K. Davis

GEORGE KELSO DAVIS

July 2, 1910–October 27, 2004

BY ROBERT JOHN COUSINS

GEORGE KELSO DAVIS WAS an internationally recognized animal nutritionist whose training in biochemistry and physiology gave him the background to approach applied questions from a fundamental perspective. His pioneering use of trace elements to improve animal performance through diet supplementation was the key to the development of productive cattle industries in Florida and in Argentina. He directed the first use of radioisotopes for nutrition studies in large domestic animals, which led to many seminal findings in mineral metabolism.

George Davis was born in Pittsburgh, Pennsylvania, on July 2, 1910, the son of Ross Irwin Davis and Jennie ("Jeanne") Lovinia Kelso Davis. In 1922 Davis's mother died of pneumonia. George and his brother John went to live with an aunt in Lakewood, Ohio, and a younger brother Robert went to live with an aunt in New York City. His father remarried in 1923 to Constance Sibray, and the family reassembled. His schooling was in the public schools of Pittsburgh. He graduated from the Samuel Pierpont Langley High School and gave the valedictory speech. After taking a business course, he enrolled at the Pennsylvania State College (now Pennsylvania State University) in the fall of 1928, majoring in dairy science and agronomy. George's interest in agriculture was stimulated by his work, during most childhood summers and vacations, on the farm of his grandfather, George James Davis, located near the mill town of Aliquippa, Pennsylvania.

George said he "was the greenest of green freshmen going to Penn State in 1928." After spending the summer of 1929 in charge of the maternity barn at a dairy farm, he changed his major to agricultural biochemistry. While at Penn State, he was active in the Christian Association (vice president) and was an associate editor of the *Penn State Farmer,* the freshman handbook, and *La Vie*, a campus paper, for which he was fraternity editor. Although he had won his letter in football and other sports in high school, he was much too lightweight (145 pounds at that time) for college football and so, during his college career, he restricted his sports to intramural participation in boxing, tennis, golf, lacrosse, and baseball. George was awarded a number of scholarships as an undergraduate, and these were especially appreciated since this was during the depths of the Great Depression. George realized how good a high school education he had received when he found how easy some of his college courses were. In those days at Penn State the dean of men posted the rank of the students at the end of the semester. During his first year, George discovered that he was third in the freshman class.

George became something of a cynic about the fraternities, realizing that students like him (that is, with good grades) were attractive to fraternities since they helped the fraternities gain a sufficiently high scholastic average to allow social functions, such as dances and house parties. Eventually, during his sophomore year, he was urged by Professor Andy Borland, professor of dairy husbandry, to join the

Alpha Zeta fraternity, which was both honorary (pledging only sophomores) and social (they had a house on campus).

George received the B.S. degree with honors from Penn State in 1932. Owing to the Great Depression, the class of 1932 found a very slim job market. At the urging of his major professor at Penn State, R. Adams Dutcher, Davis accepted a scholarship at Cornell offered by Leonard A. Maynard, a future National Academy of Sciences member. He started his graduate work with Maynard as his mentor in the fall of 1932. George had spent the summer building a house on land inherited by his father. The rental fee from that house would provide a supplemental stipend during his time at Cornell.

Very soon after getting to Cornell on the nine-month scholarship offered by Maynard, another faculty member, L. C. Norris, who was one of Maynard's first graduate students, offered George an assistantship as a chemist to help run studies with poultry, quail, and pheasants. This involved many Kjeldahl nitrogen analyses, but the pay was better than that for the scholarship and it was on a 12-month basis. When it began to look as if George might become a poultry nutrition convert, Maynard stepped in with an assistantship in the animal nutrition laboratory that allowed George to carry out his own graduate research. It should be pointed out that at that time at Cornell, all graduate students in nutrition spent some time working with Maynard, Clive McCay, and Sidney Asdell on projects that these professors had underway. In those depression days, graduate students felt fortunate because, in addition to receiving a stipend, their tuition was waived.

As part of his assistantship, George served as purchasing agent for the Maynard laboratory . He learned a number of lessons in that role that stayed with him over the years. One lesson was to always have a wish list of needs with accompanying costs available for every opportunity. He also learned to avoid spending all of the funds immediately, just to get rid of the accounting task. During his graduate program, George saw his assistantship stipend increase to $1,400 per year. At the time public school teachers earned about $1,200 annually.

George stated, "Dr. Maynard always insisted that investigations into the nutritional requirements should be repeated on more than one species," and so, in carrying out his doctoral research on the effect of fatty acids on muscle function, he developed diets that were fed to guinea pigs, lambs, goats, and calves. By incorporating cod liver oil into the diets of these herbivores to supply vitamins A and D, Davis found that a muscle dystrophy developed that involved both heart and other striated muscles. Because of the heart damage Davis observed, he and his colleagues had the idea of running electrocardiographs on the animals. The goats were not always cooperative. He discovered that soaking a piece of cheesecloth in salt water and letting the goats suck on it would keep them still long enough to get the EKG.

George was involved in Presbyterian activities on the Cornell campus. One of the programs the Presbyterian student pastor developed to interest students was that of theatrical productions. One such play, called *LoMo*, had two lead characters played by Ruthanna Wood and George. As Ruthanna recalls it, she realized on stage that George's eyes were blue and completely forgot her lines. After Ruthanna graduated in 1934, she left Cornell for her dietetics internship at Columbia Presbyterian Hospital in New York City. George traveled there to visit her as often as he could. George Davis and Ruthanna Wood were married on January 25, 1936, in East Orange, New Jersey, after she had completed her training as a dietitian.

While George was a graduate student, a major project at Cornell was what they called "the old age project." Maynard and McCay had observed that rats kept on a restricted diet from a short time after weaning appeared to live much longer than rats that were allowed to eat *ad libitum*. The logical research question was, "What was the basis for this occurrence?" Special care was taken to see to it that the diets contained all the required nutrients, and multiple observations were made on the activity and body changes that occurred. Experiments to explain the influence of caloric restriction on longevity have continued into the early part of the twenty-first century but without definitive biochemical explanation. George's travels to New York City resulted in some problems for him as a student involved in this project, as he was on occasion tardy in his duties. Nevertheless, based on a narrative that George wrote in his later years, it was clear that he had an eventful and useful graduate experience that was accentuated by the personalities of Maynard, McCay, Norris, and others.

Davis's Ph.D. final examination went smoothly, with Maynard, Dye (physiology), and Olafson (pathology) participating. Following Davis's successful defense, E. B. Forbes of Penn State offered George an academic position, as did George Brown of Michigan State University. A pharmaceutical company in Des Moines, Iowa, also offered him a position. This success at securing employment was very impressive, as job openings during the Depression were scarce. He accepted a position as assistant professor and research chemist at Michigan State in the Experiment Station's Chemistry Department. There he worked with Vern Freeman on swine nutrition, particularly with the problem of necrotic enteritis, as well as with other faculty on projects involving horses, sheep, and beef cattle. Davis remained at Michigan State for five years and made significant contributions in the areas of nutrition related to disease in animals. These included the water-soluble B vitamins and *Salmonella* infections in swine; riboflavin and vitamin C in "moon blindness" in horses; trace minerals and enterotoxemia in lambs; and vitamin C as a factor in the sterility of bulls. The Davis family grew in East Lansing with daughters Dorothy Jeanne, twins Mary Ellen and Ruth-anna Marie, and Virginia Kay being born there.

His trace element studies led in 1942 to an invitation to join the faculty at the University of Florida as a professor of nutrition and animal nutritionist in the Florida Agricultural Experiment Station. He was hired by A. L. Shealy, head of the Animal Industry Department, and Wilmon Newell, provost of agriculture. After the Davis Family moved to Gainesville, they added two sons, Robert Wyatt and George William Ross.

During World War II, when special emphasis was placed on food production, Davis was challenged by the abundant nutrition-related problems facing the cattle industry of Florida. Most notable of these was "salt sick." In this condition animals that were on pasture for more than three months would rapidly become ill and die even though the pastures produced tremendous tonnages of forage. The philosophy at the time was that purebred cattle could not survive the environmental conditions in Florida. Faculty members Wayne Neal, whom George succeeded, and Raymond Becker suspected cobalt deficiency might be involved, based on research done in Australia and New Zealand. Shortly after he arrived, Davis noticed that this particular problem was unique to specific regions in Florida. Consequently, one of his first projects was to try to analyze forage from "salt sick" and healthy areas for, among other things, iron, copper, and cobalt. He soon discovered that the chemical methods for analysis of cobalt were not sensitive enough to detect cobalt in any of the samples of forage. This led him to contact colleagues in agronomy at the university, who had a working spectrograph, asking them to run some analyses. They reported that the forages contained 0.04 ppm cobalt plus or minus 200 percent.

Seeking a method that would enable them to determine whether cobalt was a factor in "salt sick," Davis, at the point when chemical determination of cobalt content failed, considered the possibility of using radioactive tracers. He purchased a Herbach and Radiman Geiger counter (one of the earliest gamma counters), and started a search for someone who could provide the Florida research team with radioactive cobalt. Davis had been aware of reports of the work on radioisotope production with cyclotrons, and he wrote to six people working with cyclotrons, asking them to collaborate and supply his lab with radioactive cobalt. Those who replied indicated that they could not consider helping, but two noted that they thought the Massachusetts Institute of Technology might be able to help. He called John Irvine at MIT and was told that they were 110 percent booked with their cyclotron. Irvine also said, "Why don't you come see us and we can discuss what can be done?" The invitation from Irvine led to a trip by rail to Boston, where George experienced typical wartime travel difficulties.

The conference went smoothly, and Irvine indicated that MIT would supply the cobalt by bombarding stable iron and separating out the radioactive cobalt. Of course, Davis was interested in cost, having a $300 budget. Irvine just laughed. About three weeks later, the first shipment arrived in Jacksonville, Florida. It was enough radioactive cobalt for 1 cow and 100 rats; but the half-life was limited and they had to work rather fast. Two weeks later, they got a larger supply that was enough for a year. Davis later stated:

> What I did not know at the time, but learned after World War II, was that these people were working on the atomic bomb. The people in Washington, D.C., were so concerned with secrecy of the Manhattan Project that they reasoned that, 'Normally, if a professor at a university asks for collaboration from a professor at another university, it will be given if at all possible.' The folks at MIT were told to collaborate, and the atomic weapons budget would pick up the costs.

The Florida team was the first to use radioactive isotopes in large animals. As pioneers, they had limited equipment and were learning a lot about both the design of experiments using radioisotopes and protection from radiation exposure as they went along. Cyril Comar and Davis isolated a number of cobalt-labeled compounds from the rumens of the cows given the isotope. Davis later commented, "No doubt one of them was vitamin B_{12}."

The success of the Florida team's research on cobalt led to experiments on the metabolism of other minerals (calcium, copper, iodine, magnesium, phosphorus, and zinc). These radioisotopes were supplied by Irvine. At the close of World War II, George's $300 collaboration with MIT ended. Fortunately, Oak Ridge National Laboratory continued to supply Florida with the necessary isotopes. At a conference to discuss the availability of isotopes from Oak Ridge, Davis stated that it was clear from the presentations made that of all the universities that received radioisotopes during World War II, only the University of Florida's program produced results. Between 1942 and 1960, about 700 scientific papers were published from the nutrition lab at Florida. Davis was an author on 250 of those papers.

Subsequent to starting the cobalt research, George and his coworkers evaluated the poor performance of cattle on ranches in the Ocklawaha region of north-central Florida.

They observed some signs of copper deficiency, so Davis recommended applying copper sulfate (initially 50 pounds per acre) to the pasture. Following the application of copper sulfate to the pasture land, cattle that, when placed on pasture prior to market had averaged gains of six pounds per animal, improved to an average gain of 156 pounds per animal, with an increase in grade. Practical improvements such as these literally built the cattle industry in Florida, with lasting commensurate financial rewards.

During the 1960s, George Davis was very active in nutrition research in South and Central America. Much of this effort was localized in Argentina and focused on the condition in cattle and horses called "enteque seco." The problem at the time was thought to be related to a phosphorus deficiency and had resulted in a decades-long stagnation in beef production. Davis coordinated funding through the Food and Agriculture Organization of the United Nations and Instituto Nacional Technologico Agricultural (Argentina), after being turned down by another U.N. funding group. George's group found that the problem was not phosphorus deficiency. Instead, they observed that in affected animals, elastin-rich tissues (e.g., the lungs and diaphragm) were calcified, an atypical situation. Working with Argentinean counterparts, the research team concentrated on the plant *Solanum malacoxylin* after extensive evaluation of the forage available for these cattle. During the dry months, which coincided with severity of the condition, cattle foraged heavily on this plant,. Excess vitamin D_3 was known to produce the same metastatic calcification in cattle. Davis sent material to Hector DeLuca at Wisconsin and Bob Wasserman at Cornell, both future National Academy of Sciences members. With their help and that of others, the active principal was established as a water-soluble glycoside of the hormonal form of vitamin D_3. Through bypassing usual control for production of this hormone through oral consumption of the plant, excess calcification occurred. Davis was particularly proud of this international team effort. In autobiographical information, he wrote:

> I shall always be proud of the group that we assembled for the FAO-INTA project. They tackled a problem that had limited animal production. It had been described in their literature for many years, but never systematically approached until this work, and in five years we had not only solved the puzzle but in addition had shown that green growing plants can produce an active form of vitamin D.

In 1960 Davis assumed the additional responsibility of Director of Nuclear Sciences at the University of Florida. He was responsible for the construction of the nuclear sciences building and programs in the Departments of Chemistry, Physics, Radiation Biology, and Nuclear Engineering. He spearheaded a Center of Excellence development grant from the National Science Foundation, which, with matching funds from the State of Florida, provided over $10 million for the upgrading of programs in theoretical engineering, chemistry, physics, and radiation biology. In 1965 Davis was made director of biological sciences, with responsibilities for microbiology, botany, zoology, biological sciences, and biochemistry.

In 1970 Davis became director of all sponsored research at the university. During his tenure in that position (1970-1975) sponsored research support grew rapidly, particularly in the new Health Science Center. In accord with University of Florida policy of the time, he resigned as director at age 65 and returned to the laboratory as a professor, carrying out research on the relationships of organic compounds that influence the availability of minerals from feed components.

One of George's major activities upon retirement was serving as president of the twelfth International Congress of Nutrition, held in San Diego in August 1981. Four years of planning went into the congress, which was sponsored in part by the International Union of Nutritional Sciences, at that time affiliated with the National Academy of Sciences. Along with his colleagues responsible for the Congress, Davis raised about $100,000, a significant sum for an international congress at that time. An excess of about $50,000 remained, which continues today as a source of funding that allows younger scientists to attend these international congresses. An additional post-retirement activity for George was service to the U.S. Department of Agriculture in the late 1970s as program manager for the competitive grants program in human nutrition. He served as the first director of this program, which is now part of the National Research Initiative of the USDA.

George Davis received over 30 honors and awards. These included, at the national level, the Borden Award of the American Institute of Nutrition, the Herbert A. Spencer Award of the American Chemical Society, the Eli Lilly Lectureship, the Burroughs-Wellcome Lectureship, and election as a fellow of the American Institute of Nutrition. He was honored as a Distinguished Alumnus of Penn State in 1982. At the University of Florida he received the Senior Faculty Award of Gamma Sigma Delta and Faculty Award of Florida Blue Key Honorary, among other honors. His honorary societies included Alpha Zeta, Gamma Sigma Delta, and Sigma Xi. He was a member of over 10 professional organizations, and served as president of the American Society of Animal Science, American Institute of Nutrition, and Society for Environmental Geochemistry and Health. He served on about 50 national committees and 20 international committees or panels.

In 1976 Davis was elected to the National Academy of Sciences, the first scientist from the University of Florida to be so honored. The University of Florida granted him the title of Distinguished Professor of Nutrition Emeritus in 1979, and further honored him in 1996 with its Distinguished Achievement Award.

George and Mrs. Davis were members of the First Presbyterian Church of Gainesville for over 60 years. The church was an important part of their lives, as George taught a Sunday school class for most of that time. They had 6 children, 11 grandchildren, and 14 great-grandchildren. The Davises remained active in University of Florida affairs until they both passed away. Ruthanna Davis died on May 25, 2002. An endowment established through the University of Florida Foundation in 1993 to support graduate education in nutrition across the campus serves as a lasting legacy of their commitment to the University. The George K. and Ruthanna W. Davis Scholarship Fund provides graduate students selecting nutritional sciences as a graduate major with a $5,000 annual salary supplement. Thus far, over 10 students have benefited from the generosity of the Davises.

George Davis died at age 94 on October 27, 2004. When asked to comment on his work, I made the following statement to summarize his research philosophy. “Much of his work addressed fundamental questions, but he always tried to balance it with applied research. He wanted to do work that offered a tangible benefit to society.” I believe that George Davis would have agreed with that summary.

I WISH TO THANK THE Davis family for providing the information upon which this memoir is based. That included considerable autobiographical text that George prepared in his later years. Much of this is available in the University of Florida archives at http://web.uflib.ufl.edu/spec/archome/MS24.htm. That information includes his mention of numerous colleagues at the University of Florida with whom George worked very productively over his long career at this university.

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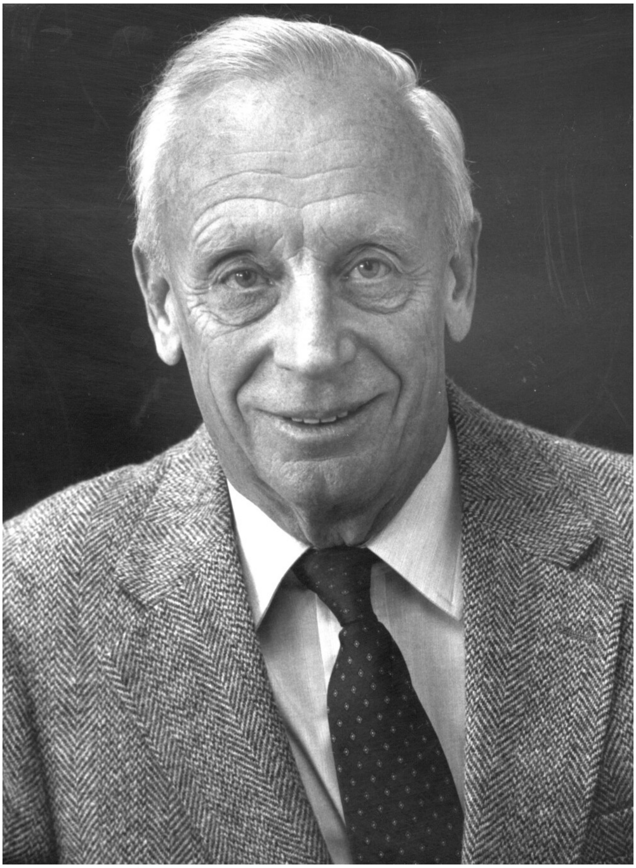

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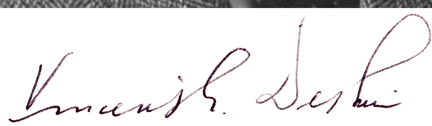

VINCENT GASTON DETHIER

February 20, 1915–September 8, 1993

BY ALAN GELPERIN, JOHN G. HILDEBRAND,
AND THOMAS EISNER

VINCENT DETHIER WAS A man of many facets—scientist, writer, musician, historian, explorer, and paragon of civility. His interests and activities ranged broadly, from the biophysics of chemosensation and the comparative architecture of renaissance cathedrals, to the ecology of natural populations and the tonal structures of baroque cantatas. Just as it takes a village to raise a child, it took a university—nay, several universities—to provide the depth and diversity of colleagues and coworkers to engage fully Vince's varied interests in science and the arts.

Thanks to his exceptional vitality, Vince paid little heed to advancing years. When he was stricken with his sudden, final illness on September 8, 1993, he was in the classroom inaugurating another course for a group of lucky college students, fully 54 years after the start of his teaching career. He was the Gilbert L. Woodside Professor of Zoology at the University of Massachusetts, Amherst, a position that did not carry a teaching responsibility. Even among his friends and colleagues, few could believe that Vince was 78 years old. He had just returned from a summer spent in his beloved family home in East Blue Hill, Maine, where he wrote many of his more than 170 scientific papers and 16 books, as well as numerous short stories.

ORIGINS OF A SCIENTIST

Vince was born on February 20, 1915, on the outskirts of Boston, Massachusetts. His parents conveyed a rich tradition of, and appreciation for, education, scholarship, and music—activities that permeated the family's household. His father had graduated with first prize in piano from the Royal Conservatory of Liège, and then emigrated to America where he became a music teacher and church organist and choirmaster in Norwood, Massachusetts. Vince's mother, who traced her lineage from an Irish royal clan, had taught in public schools in Boston before she married. This rich intellectual, aesthetic, and spiritual nurture, together with his extraordinary innate curiosity, prepared Vince for a fateful encounter with the insect world. He described this epochal event, which took place in a small park called "the oval," in an autobiographical essay (Dethier, 1985):

> My first acquaintance with a live butterfly resulted entirely from the initiative of the butterfly. I had wandered up to the oval late one hot, humid, summer day. The long, slanting rays of the sun illuminated my white shirt. Suddenly, something rocketed across the street, made a few zigzags, and landed on my shirt, just above the pocket. I stood stock-still and slowly lowered my head to see what it was. There with its wings slowly expanding clung a brown butterfly with a red band extending down each wing. This red admiral was the first live butterfly I had ever seen at close range, and I was fascinated.

Vince never lost that childhood fascination. From that time on, he collected, reared, and studied butterflies and developed a love for living creatures and their behavior. In his early butterfly-rearing efforts, Vince found that all went well when he knew which plant was the insect's preferred food. He was struck by his observation that some caterpillars starved to death rather than eat nutritious but nonpreferred plants, and he wondered why. As a teenager, he performed simple behavioral experiments that led him to conclude that butterfly larvae possessed keen senses of smell and taste that were vital to their food plant selection. These observations gave early evidence of an independence of spirit and ability to draw important conclusions from simple and elegantly designed experiments. This ability was a recurring theme throughout his scientific career. Captivated by his early observations on caterpillar food selection, Vince found his calling even before he entered Harvard College, where he majored in biology with the expectation of becoming a high-school teacher.

THE HARVARD YEARS

Vince's interest in the natural world survived Harvard's tedious biology courses (rote memorization was *de rigueur*) and close contact with pickled, rank specimens delivered for dissection. Harvard's tutorial system made all the difference. It was Vince's good fortune to have as his tutor the physiologist T. J. B. Stier. Stier not only encouraged his interest in caterpillar food-plant selection but also encouraged Vince to prepare his findings for publication. The result: a pair of papers ("Gustation and Olfaction in Lepidopterous Larvae" [Dethier, 1937a] and "Cannibalism among Lepidopterous Larvae" [Dethier, 1937b]) that went counter to the established belief that creatures with supposedly simple nervous systems like caterpillars could not possibly possess sophisticated chemosensory abilities. Vince—typically, as time would prove—broke new ground that would eventually draw whole contingents of insect sensory physiologists following in his tracks.

When Vince reminisced about his Harvard years, though, it was the adventures with fellow students that he related, not his scientific accomplishments. He and his friends loved, for example, to spend winter break hiking up the sloping backside of Mount Washington, carrying skis and lunch. It took most of the day to reach the summit. Once there, they exchanged snowshoes for skis, untied and loosened the laces of their boots (there were no safety bindings at that time), and schussed down the steep front slope. Vince reported, with typical humility, that he never made it all the way down without a near-catastrophic spill.

Vince stayed at Harvard for graduate studies (Ph.D. in 1939), during which he pursued his commitment to the study of host-plant selection by lepidopterous larvae. His advisor, C. T. Brues, was an expert on insect feeding habits. One of us (A.G.) recalls Vince commenting that one of Brues's most attractive features was that he required progress reports only once a year, thereby giving Vince the freedom to exercise his judgment on how best to proceed with his experiments. This mentoring style suited Vince perfectly in the heady atmosphere of the Harvard biology community of the 1930s. When Vince and his fellow graduate students needed to learn insect physiology and found no courses on the subject, they organized themselves and taught each other. Among the cadre of fellow students at Harvard at that time were luminaries including Carroll Williams, who were to become leaders in the world of insect study.

One of us (A.G.) profited greatly from Vince's non-authoritarian mentoring style, first in graduate school as Vince's doctoral student and later as Vince's junior colleague on the faculty at Princeton.

At the time when Vince, at Harvard, was studying the chemical senses of caterpillars, no insect chemoreceptor had been functionally identified. His behavioral and morphological studies showed him how difficult it was to overcome the technical problems inherent in the study of insect chemoreceptors. After learning of Adrian's achievements in electrophysiological exploration of sense organs, Vince approached C. Ladd Prosser at Clark University, who was applying such techniques to the study of earthworms, on how one might go about obtaining recordings from insect chemoreceptors. Despite his best efforts during a stimulating summer in Prosser's laboratory, Vince was unable to achieve the necessary technical breakthroughs. In fact, it would be more than 15 years before it became possible to record action potentials from the primary chemoreceptor neurons of insect chemosensillae. The breakthroughs came none too soon for Vince, who had already decided at the time of receipt of his Ph.D. to make the study of insect chemoreception his lifetime passion.

THE WAR YEARS

After a brief appointment as a junior faculty member at John Carroll University in Cleveland, Ohio, Vince joined the Army Air Corps in the Africa-Middle East theater of operations during World War II. Turning adversity into opportunity, he wrote his first book (Dethier, 1947) in the bomb bay of a B-25 using a captured Italian typewriter. Once, while stationed at an isolated airport, Vince hit upon a stratagem for keeping boredom at bay. Through military channels and for no reason other than to see what might happen, he put through a requisition for "anhydrous water." In a matter of weeks the requisition came back, enriched by a whole stack of appended forms, plus a request that he specify the concentration at which the chemical was needed. "Ninety-nine-point-nine percent" was Vince's reply, which prompted a further query. "What kind of container should be used?" "Stainless steel," was Vince's answer, and so the exchange continued, growing in absurdity and in bureaucratic involvement each step of the way. Vince was sure that he had stirred into action a sizeable fraction of the Army Air Corps.

Vince later became liaison officer to the chief of the Chemical Warfare Service in Washington, D.C. This brought him into contact with Kenneth Roeder, arguably the leading insect physiologist in America at the time. On visits to Roeder's laboratory at Tufts University, Vince saw that great strides were being made in insect sensory electrophysiology and realized that he would himself have to make use of the techniques involved.

In 1946 Vince returned from active duty and joined a research group at the Army Chemical Center at Edgewood, Maryland, working with chemicals that affected insect behavior. This group included D. Bodenstein, L. Chadwick, H. Frings, and C. C. Hasset. Their early attempts to relate chemical structure to stimulating effectiveness matched Vince's interests perfectly. At Edgewood he began his research partnership with the black blowfly, *Phormia regina*, and continued his quest to understand the transduction mechanism of insect chemoreceptors. Stimulating the tarsal taste hairs of a hungry *Phormia* elicited reflex proboscis extension, which then served as a quantitative index of stimulating effectiveness of a taste solution. By using very large series of sugars, alcohols, acids, and inorganic salts, Vince began to define the molecular requirements for the binding sites on the chemoreceptors providing input to the proboscis extension reflex. These molecular insights from behavioral studies would later prove invaluable when it became possible to make electrophysiological recordings from the taste cells that populate fly taste hairs.

THE HOPKINS YEARS

After his brief but inspiring research stint at the Army Chemical Center, Vince accepted a professorship of zoology and entomology at Ohio State University. A year after establishing himself in Columbus, he startled his friends by resigning this tenured position to accept a nontenured post as associate professor at Johns Hopkins University. The years at Hopkins (1947-1958), he later said, were among the most productive, educational, and adventuresome of his career. He was part of a group of neuroscientists and physiological psychologists who shared his broad perspective and appreciation for multiple approaches to animal behavior and its neural mechanisms. Notable among these Hopkins colleagues was Eliot Stellar, who later moved to the University of Pennsylvania and spearheaded a successful drive to lure Vince to Philadelphia.

A major breakthrough in the study of insect chemoreceptors occurred when Vince's first graduate student, E. S. Hodgson, during a postdoctoral stint with K. D. Roeder and with assistance from J. Lettvin at the Massachusetts Institute of Technology, developed an electrophysiological technique for recording the responses of single chemosensory neurons to aqueous stimuli applied to the tip of the taste hair (Hodgson et al., 1955). This tip-recording method led to a seminal series of papers characterizing the responses of single cells in taste hairs of *Phormia* and later, with L. Schoonhoven, of caterpillars. Vince had shown in behavioral experiments with *Phormia* that a taste solution elicited the proboscis extension reflex only when the tastant contacted the tip of the taste hair. Now he could listen in on the neural responses of the small set of contact chemoreceptor neurons associated with dendrites in the hollow channel of taste hairs.

A neuroethologist by instinct, Vince considered at every turn the nature of the stimuli encountered by fly chemoreceptors in the natural world. This led him to use as taste stimuli a wide array of substances, many derived from leaf surfaces, rather than just the salt, sweet, sour, and bitter compounds commonly used in studying vertebrate chemoreception. As the range of chemostimuli broadened, the picture of the taste code became more complex, even as viewed from the limited repertoire of sensory cells in a single *Phormia* taste hair. Vince grappled with the complexities of chemosensory coding with characteristic concern for the Umwelt or sensory world of the fly. (The issue of taste coding in insects is still an active area of research and debate, as it is in mammalian taste coding.)

THE PHILADELPHIA YEARS

Vince moved to the University of Pennsylvania in 1958, joining several of his former Hopkins colleagues in the Institute of Neurological Sciences in the School of Medicine. While Vince's primary appointment was in the biology department, the interdepartmental and interdisciplinary assemblage of behavioral scientists, neuroscientists, and physiological psychologists gathered in the Institute of Neurological Sciences was a lively and intense group that provided each participant with widely ranging perspectives and technical approaches. In this milieu, Vince and his colleague Eliot Stellar wrote the landmark book, *Animal Behavior* (1961), which appeared in three editions and ten languages.

Among the issues with which Vince and others grappled during weekly seminars was motivation. Was it a useful concept? Was it a general concept? Do insects have motivation? Some argued that this behavior separated invertebrates from vertebrates, thereby providing a basis for excluding insects, but in due course Vince performed experiments showing clearly that insects had that key feature. While some of Vince's colleagues found this exasperating, their mutual respect overcame their disagreements. These heated exchanges were educational for the cadre of graduate students in attendance, who learned from the debates that disagreements in the realm of science need not in any way affect the bonds of friendship.

Vince was renowned for his wit and charm. At a memorably bombastic departmental faculty meeting, passions ran high over opinions strongly held. Among the actors in this drama was the department chair, whose normal speaking voice could reverberate across campus, and who reportedly had the shortest fuse in the history of the university. As the verbal exchange heated up, Vince (himself quite feisty) couldn't resist a few rapier thrusts. "Vince, only an ass would say that," the chairman bellowed. "Yes, Mr. Chairman, I know that," replied Vince, "but I thought you were about to say it, and I wanted to save you the embarrassment." The meeting erupted in laughter and the two men departed the best of friends.

The Philadelphia years marked advances in understanding the response properties of fly gustatory receptors and the regulation of feeding in the fly. The students involved in this work included Frank Hanson, Joseph Larsen, Margaret Nelson, and many others, including one of us (A.G.). The work on *Phormia* was collected in Vince's magnum opus, *The Hungry Fly*, published in 1976, which makes clear that our understanding of fly feeding behavior is more complete than for any other species then under study. (While this is still the case, the mammals are advancing.) With postdoctoral associates Louis Schoonhoven from the Netherlands and Tibor Jermy from Hungary, Vince was able at last to carry out a detailed electrophysiological analysis of caterpillar chemoreceptors, as he had long yearned to do.

THE PRINCETON YEARS

Vince moved to the biology department at Princeton University in 1967, to take up an endowed chair. There, his electrophysiological investigations of flies and caterpillars continued apace, yielding evidence of the importance of both labeled-line and across-fiber coding in the gustatory pathways of insects. Vince's devotion to insect-plant interactions and chemosensory function was undiminished after more than 30 years of work.

When one of us (A.G.) joined the biology faculty of Princeton in 1968, Vince was as supportive as he had been as a "doctor father," even as his former student's research turned from insects to mollusks. Vince, as so many were to learn on their own, was in every respect an ideal colleague.

At Princeton, Vince returned to his interest in learning in flies. The lack of evidence for learning in these insects led Vince to speculate in a remarkable paper entitled "Microscopic Brains" (Dethier, 1964) that perhaps flies could not learn. Two developments were later to prove otherwise. In 1974 Chip Quinn, Bill Harris, and Seymour Benzer published a demonstration of odor-conditioned behavior in *Drosophila* (Quinn et al., 1974). About 10 years later, T. Fukushi showed reliable and robust one-trial color-food conditioning in walking flies (Fukushi,1985). Ironically, the walking flies learned the food-color association as they did the search "dance" that Vince described in his 1964 paper. The molecular dissection of fly learning continues to be a major research topic in neuroscience.

Vince also tested the idea that polyphagous caterpillars would be more likely to show food-aversion conditioning than monophagous caterpillars, a suggestion one of us (A.G.) made in a paper on comparative aspects of food-aversion conditioning. Vince found that one species of polyphagous caterpillar did show such conditioning, while a species of monophagous caterpillar did not.

The phenomenon of learning completes a picture in which food selection behavior comprises three-tiers: (1) a peripheral system, sensitive to multiple chemical stimuli;

(2) an internal chemosensory system that measures the quality and quantity of absorbed food constituents and which may modify the insect's behavior via its input to the central nervous system; and (3) a modifiable integrative center in the central nervous system that decodes sensory patterns and commands the feeding motor-control center, integrating feedback from previous postingestive consequences associated with responses to a chemosensory code.

Vince was at the forefront of the scientific endeavor to unravel food selection behavior in herbivorous insects. He set his enduring mark on the basics of the first two components of the three-tiered system. The third component, the brain, remains even now terra incognita. Analyzing it will not be an easy task, although recent progress in unraveling key aspects of how insects process olfaction, vision, and audition provides encouragement.

THE AMHERST YEARS

Vince increasingly heard the call of two of his lifelong passions—his beloved summer home in East Blue Hill, Maine, and his avocation as a creative writer. He dreamed of early retirement, of living in Maine and building on his already-established success as a celebrated writer. But with Jehan and Paul, the children of his midlife marriage to Lois Crow, still in college, it was not to be. Instead, in 1975 Vince made a last move, this time to assume the Gilbert L. Woodside Professorship of Biology at the University of Massachusetts in Amherst.

Established in his new laboratory, joined by postdoctoral associates Elizabeth Bowdan, Mary Behan, and Roberto Crnjar, and reunited with Martha Yost, who had been his first research assistant at Hopkins and who now lived in Amherst, Vince redoubled his efforts to crack the gustatory code in caterpillars. His prefatory chapter in the *Annual*

Review of Neuroscience for 1990 summarizes his long history of work in chemosensory neuroscience during a time of transition from descriptive to functional studies, along with more philosophical comments on the nature of reality and comprehension made possible by our sensory receptors and filtered by our contemporary intellectual ambience (Dethier, 1990).

His new position was a research professorship, and Vince intended to focus exclusively on his research program, but events distracted him from unfettered focus on research. The university needed his leadership and humanitarian touch—first to serve as founding director of the new Neuroscience and Behavior Program, and later, during a particularly unsettled period in the university's history, to chair the Chancellor's Commission on Civility (Dethier, 1984). At the same time, the university began a series of courses heavily emphasizing writing skills. Appealing to his love of clear and elegant exposition, Vince found that teaching was a commitment he couldn't break.

To acknowledge his accomplishments on the Commission on Civility and his strong commitment to civility issues generally, the university established posthumously the Vincent Dethier Award for the faculty member who best exemplifies the ideals to which Vince aspired. For an academic whose civility was intrinsic to his very nature, this may indeed be the ultimate accolade.

BEYOND THE REALM OF SCIENCE

Vince wrote a number of evocative and lyrical books on natural history, including *To Know a Fly* (1976), *The Tent Makers* (1980), *The Ecology of a Summer House* (1962), and *Crickets and Katydids, Concerts and Solos* (1992). He also wrote celebrated books for children (*Fairweather Duck* [1970] and *Newberry, The Life and Times of a Maine Clam*

[1981]) and satires (including *Buy Me a Volcano* [1972] and *The Ant Heap* [1979]).

In *Newberry* Dethier showed his talents as a storyteller par excellence. During summers with the family in Maine, he observed and absorbed in great detail the world of creatures on the surrounding coastal shores. He portrayed this world with warmth and wit in the daily adventures of a clam named Newberry, to whom a local doctor gave a purple woolen muffler to tie around his long neck to cure an ache. Newberry's adventures and involvements bring alive, and charmingly so, the essential biology of many shore creatures, from clams and starfish to sandpipers and gulls.

Vince's short story "The Moth and the Primrose" (Dethier, 1980) was selected for inclusion in *The Best American Short Stories of 1981*, one of several awards Vince received for his varied works of fiction. In part a poetic lesson in insect-plant interactions, the story is a moving tale of Old Prout, a clam digger, "one of the least of all creatures" who did "great good simply because he did no harm. Yet in simply living his life he affected the lives of all others." Prout built a road across a peninsula or, rather, "by use he had created the road. Although he did not know it, with the road he had made possible a whole cosmos. He had made possible the primroses. Without the road, there would have been none because they grew nowhere else. Without the primroses, there would have been no moths because they too could exist nowhere else." In this bittersweet tale, Vince portrays the interwoven lives of Prout, the primroses, and the moths. No account here could possibly capture the depth of understanding for the flowering and ebbing of life nor the pathos expressed in this story. Prout died one night in a violent storm; without him, the road gradually disappeared, and with it went the primrose and the moths. At the burial the parson pondered the meaning of the old hermit's life. Being ignorant of the interplays of nature Prout made possible, the parson would not understand that "in some unfathomable manner Prout was perfected in the being of the moth and the primrose. Perhaps it was right that all these things ceased in time to exist after the old man had gone. For nothing really survives the man . . . a trace perhaps, a pyramid, a web; but these must be empty symbols."

Particularly notable among these works that intertwine literature and philosophy is *Ten Masses*, in which Vince gives voice to his personal philosophy and the role of faith in his life. Vince could have been a poster child for Stephen Jay Gould's Principle of NOMA—"Non-Overlapping Magisteria." Critical scientific thought and religious faith belong to distinct "magisteria," or domains of knowledge, Gould says; they are not mortal enemies. We do not know what Vince would have said on this issue, but his life and writings are a testament to the potential creative synergy of these two magisteria.

Vince's exceptional qualities as a scientist of breadth, insight, and creativity, as an inspiring and beloved teacher, as a masterful writer, and as a gentleman of good humor, generosity, and uncommon yet natural civility endeared him to everyone who knew him and brought him richly deserved recognition. Among his many honors were election to the American Academy of Arts and Sciences (1960), The National Academy of Sciences (1965), and the American Philosophical Society (1980); membership in the Explorers Club; and fellowship in the Royal Entomological Society of London. In 1993 he received the John Burroughs medal for distinguished nature writing.

Of the many honors he received and meetings he attended, the yearly gathering of fellows of the American Philosophical Society, rich with artists, authors, musicians, and scientists, held a special attraction. At his inaugural meeting in 1980, he found great delight in having lunch with a fellow inductee, the opera singer Beverly Sills. His address to the members, "Sniff, Flick and Pulse: An Appreciation of Interruption" (Dethier, 1987), was vintage Vince, weaving together how the principles of sensory perception and biophysics, elucidated in the realm of odor perception, are directly tied to human perception and the appreciation of art, architecture, and music.

Vincent Gaston Dethier was loved—and is remembered—for his passion for nature, his elegant science, his deep desire to understand, and his dedication to lucid and esthetic communication of that understanding; for his friendly manner, keen wit, lively sense of humor, and love of family; and for his humanity. In her contribution to the Festschrift honoring Dethier on his seventieth birthday, Miriam Rothschild gave voice to these feelings in her own perfect and inimitable way. Referring to a painting by Van Gogh, she wrote:

> Two White butterflies twirling in freedom and winged delight. For me they are the symbol of daydreaming—the poetry that Vince Dethier insinuates so cunningly into our factual information and knowledge. For the gift, of these special white butterflies—along with all your official and unofficial students, past, present, and future—Vince Dethier, I tender you my most heartfelt and grateful thanks (Chapman et al., 1987).

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Photograph by Whitley-Strawbridge Inc.

WALTER GORDY

April 20, 1909–October 6, 1985

BY FRANK C. DE LUCIA
AND
BRENDA P. WINNEWISSER

WALTER GORDY WAS ONE of the founding fathers of microwave spectroscopy, a man of vision, scientific taste, disarming humor, and great personal warmth. In 1948 he wrote the first comprehensive review of the field for *Reviews of Modern Physics*, envisioning much of its future. Early in the development of the field he recognized the scientific and technical importance of expanding into the millimeter and submillimeter spectral regions and devoted a significant portion of his efforts over the next 30 years toward this end. His vision of the importance of the shorter wavelengths has come to fruition in the explosion of fields as varied and vital as interstellar radio astronomy and investigations of the most fundamental atomic and molecular interactions, as well as the current enthusiasm for the terahertz spectral region. Gordy also had the foresight to see the possibilities of applying microwave techniques to the study of biological problems and in doing so became one of the pioneers in biophysics. He opened the field of study of electron spin resonance of radiation damage in both amorphous and later crystalline organic and biological materials. In the course of developing these two fields, Gordy supervised the Ph.D. theses of 75 students, mentored

57 postdocs, wrote 4 books, and published more than 250 papers.

PERSONAL HISTORY

Walter Gordy was born on April 20, 1909, the son of Walter K. and Gertrude Jones Gordy. He was reared on a farm and attended elementary and high school in rural Newton County, Mississippi. The first seven grades were in a one-teacher school, with five-month terms arranged around the busy seasons of the local farms. He did not graduate from high school until 1929, but when he did so, it was as valedictorian. Whatever the nature of his education up to that point, he did not waste any time after that. He spent one year attending Clarke Memorial Junior College, which was closely associated with small but highly respected Mississippi College. He entered Mississippi College in 1930, graduating after only two years, even while teaching during one of those years in the high school of the town of Dixon. His bachelor's degree was awarded "with special distinction" in 1932. Gordy did not forget Mississippi and Mississippi did not forget him. He was awarded an L.L.D. by Mississippi College in 1959 and a Special 50th Anniversary Award by the Mississippi Academy of Sciences in 1980.

He enrolled immediately at the University of North Carolina in 1932, received an M.A. a year later and in 1935 a Ph.D. under the direction of Earle K. Plyler. Gordy held Plyler in exceptionally high regard, and later was instrumental in working with fellow Plyler student George E. Crouch in the establishment of the Earle K. Plyler Prize for molecular spectroscopy of the American Physical Society. He himself would be awarded the prize in 1980. In Chapel Hill he met Vida Miller, an English instructor at the University of North Carolina in Greensboro (in those days, "Women's College"), whom he married in 1935. She ventured with him to Texas, where from 1935 until 1941 they both taught at a women's college, Mary Hardin-Baylor College (later university). Like Mississippi College, Mary Hardin-Baylor College was a private institution, associated with the Baptist Church, with high academic standards. During these depression years, Gordy worked with optimism, self-sacrifice, and great energy in his pursuit of physics, serving as associate professor and head of the department of mathematics and physics, while Vida taught Latin. Salaries were nominal: For the duration of the depression, faculty let the salaries be set after the enrollment for each year was known; and the vegetable garden was essential. During the summers, he traveled to the University of North Carolina and to Ohio State University, using their infrared spectroscopic facilities to pursue his interests in spectroscopic studies of hydrogen bonding. As a result of this work, he was awarded one of just two National Research Fellowships given in physics in 1941 and went to work with Linus Pauling at the California Institute of Technology.

World War II interrupted the tenure of the fellowship. In February 1942 he joined the staff of the MIT Radiation Laboratory, where he participated in the development of microwave radar. This vital work brought no personal scientific credit but had a dramatic effect on his subsequent career. Once acquainted with the new microwave technology, he immediately deduced its untapped potential for the study of molecular spectroscopy.

At the end of the war he joined the Physics Department at Duke University, rising to its highest rank, James B. Duke Professor, in 1958. His two children, Eileen and Terrell, were raised in Durham. He was elected to the National Academy of Sciences in 1964, and won the Jesse W. Beams Award of the American Physical Society in 1974. He retired from Duke in 1979 and worked intensively on a second edition of his book on microwave spectroscopy, finishing it a year before his death on October 6, 1985.

GORDY—THE MAN

Gordy's life was focused on his science, but he had a remarkable breadth of interests and a strong commitment to the well-being of others and to society. He also had strong opinions about how society could best improve itself. He never forgot the standards and values he learned in rural Mississippi, and his Mississippi speech cloaked all his lectures and conversations in a naïve but highly intelligent idiom. As his students, we were educated not only about physics but also about life, social systems, and world demography. His Saturday classes in particular, in the 1960s, were infamous for beginning their spectroscopy portion only after he had been reminded that the period was over by the closing bell.

In 1962, when faced for the first time with the prospect of a female doctoral candidate (B.P.W.), Gordy bravely proclaimed, "I've never had a girl graduate student before, but I'm willin' to try." Her introduction to the group was, "I want you to treat her like just one of the boys." Which they did, to her relief; but it was Gordy himself who reached to help her carry an oscilloscope one day.

Gordy was the epitome of a southern gentleman; even as a junior graduate student it was almost impossible to pass through a door after him. He was loyal to all his people and knew how to fight fiercely but subtly for them, without pushing them into positions they could not handle. His files are replete with letters promoting his students, counseling them about their careers, sharing the joys of their personal and professional successes, and commiserating with them in times of trouble. Both Walter and Vida Gordy took great joy in keeping up with the large "microwave family."

Many an American Physical Society meeting in Washington reached its high point for members of that family on the evening designated for the Microwave Dinner, to which a few outside guests were privileged to be included, and where Gordy addressed the assembly, sharing his pleasure in physics and his pride in the accomplishments of each of his students just finishing or after leaving Duke.

Once, as Gordy and his wife were about to depart for a weekend event, Gordy stopped first in front of the department, just to pick up his mail. Vida remained in the car, probably working on a stuffed toy for some lucky child, as we often saw her. She had to wait an unusually long time, and she was becoming unusually concerned, before Walter finally emerged from the department. In his mail he had found a letter from a young man in Germany who was asking for a postdoctoral position. After looking at his qualifications, Gordy realized there was no time to lose if he wanted to have this young physicist come, and sat down there and then to write and send a telegram to Hans Dehmelt, who indeed came, and went on to win a Nobel Prize in 1989. More than one postdoc came to Duke because Gordy answered first.

His wife, Vida, was an integral partner in his scientific life. As a former English instructor, she had a legendary role in the editing of Ph.D. theses. The Gordys were concerned not only that the science from the Duke laboratory was correct and significant but that it was conveyed to the community in a style that was clear, elegant, and syntactically correct. The teamwork was fascinating to watch and participate in at both a personal and professional level. The harshest words I ever heard from Walter Gordy came after a long struggle about a particularly difficult point to his wife, “That may be good English, but it’s damn poor physics!” Each manuscript evolved from Gordy’s energetic but illegible scrawl to Vida Gordy's coherent transcription to the secretary's clean copy, only for the cycle to begin again—often to the horror of a student or postdoc—with ruthless alterations. Several cycles were always necessary to polish a presentation to Gordy's satisfaction.

Gordy served on committees of many organizations, including the Council of the American Physical Society. The enduring image we are left with in reviewing his files is not so much the volume of this activity but the clear commitment he had toward its worthy goals. He did not collect committee assignments to expand his resume or to enjoy power over his colleagues but as a way of giving back what had been given to him. His letter declining an invitation to serve on the NASA Research Advisory Committee on Electrophysics states, "After examining the package of blanks which I am required to fill in before serving on the Committee, I have decided that it would not be advisable for me to accept the invitation. My strong interest in research and teaching conflicts, I fear, with an activity so involved as this one seems to be in red tape and security regulations." For the committees that he did serve, he worked hard and effectively. He was not a proselytizer, but rather he understood the connections and long-term effects and how to make the arguments. He wrote impassioned letters about the job crisis for physics students in the early 1970s, not just because of the impact it had on the students who had committed their lives to physics but also because he knew that it would make it difficult to recruit students in the future, that this would damage the discipline of physics, and in doing so deprive society of the best that physics could offer.

When Gordy, as a winner of the North Carolina Medal of Science, received a letter from the governor seeking his advice on the establishment of a residential high school (now the North Carolina School of Math and Science), he seriously and carefully considered the issues, consulted with knowledgeable people, and wrote a thoughtful letter. He told the governor that while the state had many talented students whose educational needs were not being met, he believed that on balance it was more important for young people to be with their families. Gordy believed that talent, hard work, family support, and character were much more important than the circumstances of secondary school education. One of us (F.C.D.) vividly recalls Gordy's bemused understanding of the political process when he received a reply from the governor announcing the establishment of the school and thanking him for his support.

Postdocs spread the expertise of the Gordy lab with lasting effect to Europe. In England, Italy, Germany, and Yugoslavia, laboratories were initiated by visitors in the 1950s and 1960s, which in the case of the latter three countries are still contributing significantly to millimeter wave spectroscopy and electron spin resonance studies.

Having built his career at Duke, Gordy was fiercely loyal to the institution, and played an important role in its development; however, he was also an academic realist. In transmitting a report on ways to further improve the university, he included a short note to his friend Markus Hobbs, the dean of the graduate school, "Enclosed is a plan for beating the Ivy League. However, I doubt that it will work. From my years of plowing in Mississippi I gleaned this wisdom. You can't make a good crop without plenty of feed for your horses and you can't have plenty of feed for your horses without making a good crop. That's why I quit farming." Clearly, Gordy's wisdom exceeded that of most academic planning committees.

PROFESSIONAL HISTORY

Gordy's early use of infrared spectroscopy to study hydrogen bonding gained him a worldwide reputation. He found the first evidence for hydrogen bonding between unlike molecules, a concept central to studies of liquids, droplets, and gaseous complexes today. Gordy determined the hydrogen bond strength, which is related to the electron-donor property, through wave number shifts in infrared bands of many organic substances. At a time when we had less data than we do today, and the concept of the chemical bond was still under discussion, his formulation of the electronegativity scale of bonded atoms, based on force constants and internuclear distances, was a guiding concept for chemical physicists, and could be correlated with the corresponding work of Pauling and Mulliken.

Gordy's most lasting legacy has been the foundations he laid for science and technology in the millimeter and submillimeter (also known as terahertz) spectral region (100 GHz-10 THz, or 3 mm-0.03 mm). Gordy went to Duke in 1946, and with great energy established one of the major centers for the new field of microwave spectroscopy. The genesis of this field was the wartime development of microwave radar, but it was greatly aided by a fortuitous (for microwave spectroscopy at least) accident. Toward the end of the war, a previously unknown rotational transition of water (6_{16}-5_{23}) immediately in the middle of the frequency band that was being developed as the next new radar band (K-band) rendered this band unusable for radar. Not only did this make vast quantities of sophisticated K-band equipment—ordinarily beyond the means of university researchers—immediately available as army surplus (K-band klystrons at a dollar a pound) but it also established the relevance of rotational spectroscopy. In his 1948 article in *Reviews of*

Modern Physics, Gordy paid homage to this juxtaposition: "So often in the past has an instrument or a body of knowledge developed by the pure scientist found use in practical affairs that it is gratifying to find an outstanding example of reciprocity."

For most of Gordy's career at Duke, his passion was the extension of microwave techniques to ever-shorter wavelengths *and* the immediate exploitation of each new technical advance for studies of the rotational spectra of small fundamental molecules. In fact, a strong argument can be made that this focus on science led Gordy down paths to enduring technological advances: the microwave harmonic generator, electronic frequency marker systems, quasi-optical propagation, and the exploitation of sensitive detectors to compliment the harmonic generation sources.

Early and often Gordy extolled the scientific reasons for his drive toward shorter wavelengths and the complimentary technical attributes of nonlinear harmonic generation as an energy source in this spectral region:

1. Molecular absorption coefficients increase very rapidly with frequency, and the generation of even small amounts of microwave power result in very sensitive experiments.
2. The very high resolution of microwave spectroscopy can only be obtained in very low-pressure gases, which are easily saturated at modest power levels.
3. Harmonic generation sources provided a natural link to the electronic frequency standards that were necessary for precision measurements.
4. Simple but scientifically powerful systems with large bandwidth can be built. (Having gone to school in the depression, he even noted in one of his reviews, "This advantage is largely a monetary one.")

5. The spectra of many of the small and most fundamental molecular species lay there.

This last attribute—combined with the widespread adoption of the harmonic generation technique—has led to a golden age in the application of submillimeter wave spectroscopy. The molecules in our atmosphere and in interstellar space are precisely those that have been or can be studied and monitored with microwave and millimeter wave techniques. Most visible have been the large and sophisticated radio astronomy and atmospheric remote sensing experiments based on this approach that are today being deployed on balloons and in aircraft and satellites. We are sure that he would be excited to see instruments such as the *Submillimeter Wave Astronomy Satellite* and *Microwave Limb Sounder* that follow in his legacy. He would be astounded at the scale and ambition of projects like the *Atacama Large Millimeter Array* that is placing sixty-four 12-meter antennas in a desert at an elevation of 16,400 feet in Llano de Chajnantor, Chile. He would be equally impressed by the European Space Agency's *Herschel*. This cryogenically cooled satellite has a 3.5-meter objective and harmonic-generation-based receiver channels up to almost 2 THz and will be located at the second Lagrange point of the Sun-Earth system, 1.5 million kilometers from Earth. These are a fitting confirmation of the importance of his original observation of millimeter wave radiation from the Sun, obtained at a rather less advantageous site atop the roof of the Duke Physics Department, using an old searchlight reflector, in 1955.

Gordy was wise in his selection of research projects. In August 1946 in his second monthly report on his new Air Force contract he reports that the first molecule that they planned to study was ozone, "because of its uncertain structure, because it should produce strong absorptions, and because of its presence in the upper atmosphere, it is obviously of importance to the Air Force." In his first report in July he had duly reported that almost all that had been accomplished had been as a result of work for gratis, because the contract had been in effect only four days. Even at the very beginning Gordy was canny in the selection of a project that combined a scientific unknown of broad general interest, an astute judgment of the requirements for the success of the project, and relevance in the real world and to the sponsor of the research. Indeed, for largely the same reasons, ozone is still of active research interest in many labs around the world today. When one of us (B.P.W.) was presenting her Duke thesis work in electron spin resonance of irradiated DNA at a seminar in Germany, her host, who also worked in this field, asked her, "How does Gordy do it? He always thinks of a new type of experiment before we have been remotely able to explore the possibilities in his last experiment."

Exploiting his laboratory's microwave expertise in 1955, Gordy pioneered the use of electron spin resonance—a brand-new technique that he had just begun to explore—for the study of radiation effects on biological substances. He led in this field with studies of amino acids, and did not shy away from proteins, DNA and RNA, and animal tissue. His laboratory also made the first measurements of a single crystal of an irradiated organic substance, allowing the full *g*-tensor to be determined. By 1965 he and his colleagues were observing the electron spin resonance spectra of single crystals of constituents of DNA, which led to the identification of a hydrogen addition radical in thymidine. Through studies of oriented radicals produced in irradiated single crystals of amino acids and simple peptides, he found the explicit structures of the stable or moderately stable free radicals preferentially produced in a number of proteins and could relate them to the protein structures. His studies helped to clarify the mechanism of the oxygen effect in radiation damage and the action of certain chemicals (cystine) in providing protection from radiation damage to biological systems. These biological studies led to the initiation of similar work by groups in medical schools studying nuclear radiation effects as well as in universities and national research laboratories in various countries. His laboratory also became infamous in the physics department at Duke because his students had to go to the local slaughterhouse for fresh bone, horn, and assorted bovine and poultry tissue.

After the surprising identification of a hydrogen addition radical in thymidine, Gordy and his coworkers explored observations of the electron spin resonance of free radicals produced in powdered samples of the nucleic acids and their constituents that had been bombarded by hydrogen atoms. Later, the method was applied to show the effects of the interaction of hydrogen atoms and of OH radicals with proteins and their constituents, as well as other substances. The identification and characterization of radiation-induced radicals through electron spin resonance has remained a cornerstone in the understanding of radiation damage of all types. Toward the end of his career Gordy wrote a major book on the methods of analysis of electron spin resonance spectra.

The scientific writing of Walter Gordy is fluent and clear, and his books have enjoyed wide readership and appreciation from students and experts alike. He published the first book on microwave spectroscopy in 1953, and then in 1970 compiled, in collaboration with Robert L. Cook, the even more significant volume *Microwave Molecular Spectroscopy*, which underwent a major revision and expansion in 1984. This was an extraordinary effort, but a labor of love and in many ways a legacy for the next generation. One of us (F.C.D.) can still recall the joy and relief in 1983 occasioned by the mailing of the final manuscript to the publisher, followed by Gordy's despair upon learning that the price of the book would be $175, clearly beyond the means of the students who he hoped he could help train. There followed a carefully crafted letter to the editor saying how disappointed he was, which elicited an equally carefully worded letter from the editor saying how he shared Gordy's disappointment. Walter would be pleased to know that this long out-of-print book has a current Internet price of $700.

More than 20 years after his death, Walter Gordy's legacy is large and expanding. In a conference devoted to the exploitation of the submillimeter spectral region, he once observed, "Thus, in 1946, the spectral region from wavelengths of 4 mm to 0.3 mm represented a rich, underdeveloped natural resource. To our newly formed microwave laboratory at Duke, it represented an exciting challenge." This "gap in the electromagnetic spectrum" that he identified early on as a worthy scientific home is now home to a diverse community going under many names: the millimeter, submillimeter, or near millimeter wave, terahertz,or far infrared region. The techniques that he pioneered and the scientific results he achieved have provided the foundations for much of this work. Similarly, he reveled in the unknown possibilities in the applications of electron spin resonance to complex biological systems, the aesthetic antithesis of the discreet millimeter wave spectroscopy of gases. Both fields are active today, 50 years later, with new technological tools and broader applications.

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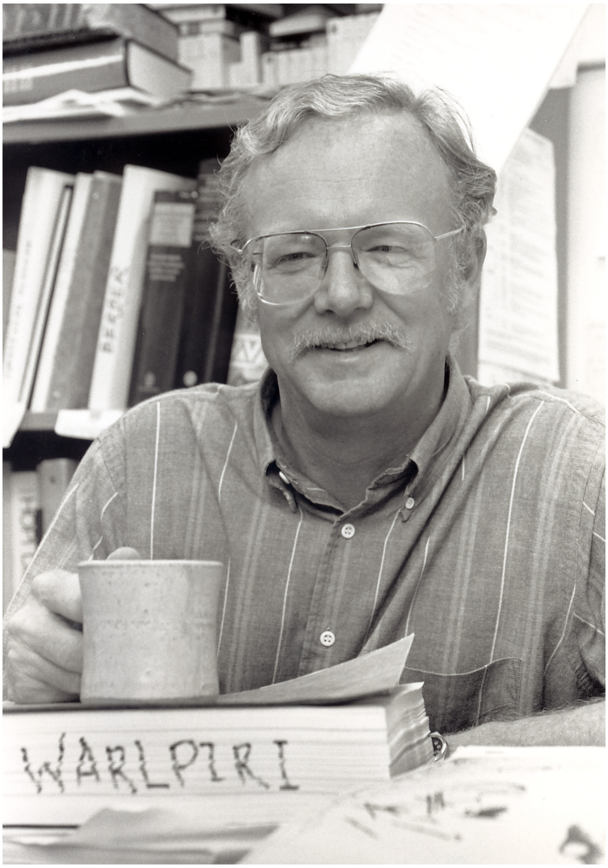


Photograph by J. D. Sloan.

Kenneth Hale

KENNETH LOCKE HALE

August 15, 1934-October 8, 2001

BY MORRIS HALLE AND NORVIN RICHARDS

KEN HALE WAS A DESCENDANT of Roger Williams, the founder of Rhode Island, whose political and religious views led to his banishment from Massachusetts by order of the General Court of the Colony. Williams made special efforts to be on good terms with the indigenous Indians, and his 1643 book *Key into the language of America* is one of the earliest studies in English of a Native American language. Hale felt great affinity for his seventeenth-century ancestor, not only for the latter's interests in the language and culture of the indigenous population among whom he had come to live, but also for his radical political views.

Hale was six years old when his father, who had been a banker in Chicago, changed careers and became a rancher in Arizona. Growing up on the family ranch, Hale came in contact with speakers of Native American languages and discovered that he had an extraordinary talent for acquiring languages quickly and thoroughly, a talent that he was fortunate to retain throughout his life.

Hale did his undergraduate work in anthropology at the University of Arizona in Tucson. For graduate study he transferred to Indiana, where he worked with C. F. Voegelin, who had been an associate of Edward Sapir (NAS 1934).

Hale obtained his PhD in 1959 at Indiana University with a thesis *A Papago Grammar*. He then spent two years doing fieldwork in Australia, during which time he collected the basic linguistic data (morphology and core vocabulary) of around 70 languages and made a more intensive study of many of these. Hale's field notes and records of those years have served as the raw material for linguistic research at all levels, from numerous Master's and PhD theses written by students at universities in Australia and the US to the most advanced research currently underway.

Upon his return from Australia Hale taught at the University of Illinois at Urbana and at his alma mater, the University of Arizona. It was at this time (in the 1960s) that Hale became an active contributor to the work in transformational and generative linguistics that had been initiated by Noam Chomsky (NAS 1972) at MIT. This, in turn, led to his appointment in 1966 to the linguistics faculty at MIT, where he remained to the end of his life.

Hale was sensitive to the unequal relationship that often obtains between researchers, who usually have enormous material resources at their command, and the individuals whose languages are being studied, who often are barely surviving on the margins of our modern world. He was deeply concerned about "the sheer lethal incompatibility between the dominant Anglo-Saxon people's empire and an Aboriginal society of almost inconceivable antiquity,"[1] and he made major efforts to provide tangible benefits to the groups whose languages he was studying. In Australia, in Nicaragua, and particularly in the American Southwest, he was instrumental in starting programs in elementary education in several local languages. He tried in a great many instances to provide training to individuals from the groups whose languages he was studying, including admission to graduate programs in linguistics with financial support.

These efforts, alas, were less successful than Hale had hoped. Mainly as a result of political changes over the last quarter century, many of the educational programs Hale established lost financial support and had to be abandoned after a few short years, well before they could have worked their planned effects. These setbacks did not discourage Hale. They only clarified for him the great difficulty of the task, which he hoped would never be abandoned but would be continued by subsequent generations of linguists.

An essential part of the research in linguistics consists of the collection of appropriate data. In many cases this involves extensive one-to-one contact with a speaker of a particular language. This is especially true of languages without copius written records, where fieldwork with native speakers is the only means of gathering necessary data. Hale was justly famous among linguists as a superb collector of linguistic data.

However, data collection was never the primary goal of his work. For Hale, as for many modern linguists, the central aim of linguistics was the elucidation of the mental capacities of humans by virtue of which they are able to learn to produce and understand utterances in one (or more) languages. Like any other science, linguistics aims to go beyond the recording of facts to the discovery of the principles that govern these facts. One important result of the work of the last half century is the conclusion that the grammars of languages do not vary virtually without limit, as had been widely assumed; rather, the cross-linguistic differences that we find are all variations on a theme, with a common core of linguistic properties that appear to be universal in human language. On one widely held view (subscribed to by Hale and the authors), this linguistic uniformity is due to the fact that the computations involved in putting words together into sentences employ neurophysiological machinery that is uniform in the human species. It is this machinery, sometimes called Universal Grammar,[2] that allows humans, but not chimpanzees, to learn English, or Warlpiri, or any other language, and it is due to the nature of this machinery in *homo sapiens* that human languages have certain properties and lack others. On this view, the subject matter of linguistics is the nature of the human mind, of this neurophysiological machinery which is part of what makes us human, as revealed in the patterns of the languages of the world. This approach to human language began with Noam Chomsky's pioneering work in the 50's, and has driven several decades of fruitful work in linguistics—work to which Ken Hale made profound and varied contributions.

Partly because of his talent as a polyglot, Hale was able to shed light on these profound questions of human nature by drawing on data from a phenomenal number of languages from all over the world. In addition to studies of the native languages of Australia and the American Southwest (especially Navajo, Hopi, and Tohono O'odham [formerly called Papago]), Hale's bibliography includes papers on two native languages of Nicaragua (Ulwa and Miskitu), on Irish, on Igbo, on Dagur (a language of Mongolia), on Hocąk (Winnebago), on K'ichee' Mayan, and on numerous others. In what follows we have tried to present one of Hale's many contributions to linguistic theory in a manner accessible to readers without extensive familiarity with the technical literature. We must emphasize that Hale's contributions were so profound and far-reaching that we can discuss only a small fraction of them. We have picked a particular area in which he was active, and will go into this area in some detail, merely as an illustration of the impact of his work. It is our hope that the following pages provide those who have never been exposed to modern linguistics with some insight into the problems and a few of the results of this area of scientific inquiry.

ONE OF HALE'S QUESTIONS: ARGUMENT STRUCTURE

A recurring theme in Hale's long and fruitful research career had to do with the nature of what linguists refer to as *argument structure*. This is the power of certain kinds of words (for example, verbs) to determine certain other aspects of the structure of the clause. Traditional grammar recognizes, for instance, that verbs may be *transitive* or *intransitive*, requiring or forbidding the presence of a direct object (here and below, examples marked with an asterisk, like (1b) and (2b), represent inadmissible sequences of words):

(1) a. The dragon devoured the villagers.
b.* The dragon devoured.

(2) a. The knight fainted.
b.* The knight fainted the danger.

Transitive verbs like *devour* require a direct object, while intransitive verbs like *faint* cannot occur with an object. In some cases, the demands imposed on the structure by the verb may be more elaborate than this; verbs like *put*, for example, require the presence not only of a direct object but of a locative prepositional phrase as well, as (3) shows:

(3) a. The dragon put the villager upon the plate.
b.* The dragon put the villager.
c.* The dragon put upon the plate.

In all of these examples, the argument structure is determined by the verb. Once the role of verbs in determining argument structure is recognized, a host of questions arises. What kinds of verbs can there be? We have seen above that verbs like *devour* require direct objects, and verbs like *put* require locative prepositional phrases as well as direct objects. Are there any limits on the kinds or numbers of things that verbs can require? Must the properties of argument structure be restated for each language, or are there principles that hold universally?

One of the important results of the work of the last half century is that the properties of argument structure across languages do not simply vary without limit, but are narrowly constrained by general principles of Universal Grammar. This is particularly interesting, since argument structure interacts with an aspect of linguistic knowledge that is plainly not universal, namely the properties of the individual words of the language. Part of the task of a child learning her first language is to learn the vocabulary. But, what does "learning the vocabulary" entail? At a minimum, a child learning English must learn, for example, that the word pronounced *faint* can be a verb with a particular meaning (something like "lose consciousness"). Here Universal Grammar is clearly of no help, as the pairing of sound and meaning is arbitrary; no universal principles predict that the word with this pronunciation ought to have this meaning rather than a different one. As (2) shows, any English speaker also knows at least one other fact about *faint*, namely that it is intransitive (that is, that it cannot have a direct object). Is this an independent fact that must be separately learned? Or does it follow from other properties of the verb's meaning? This puzzle is one of many to which Hale contributed answers.

Let us consider the nature of transitivity somewhat more closely. There are verbs in English that differ from the ones considered above in that they can appear either with or without an object (that is, they may be either transitive or intransitive):

(4)	a. The ice melted.	[*intransitive*]
	b. I melted the ice.	[*transitive*]
(5)	a. The pot broke.	[*intransitive*]
	b. I broke the pot.	[*transitive*]

By contrast, the intransitive verbs in (6-7) lack transitive counterparts:

(6)	a. The baby laughed.	[*intransitive*]
	b. *I laughed the baby.	[*transitive*]
(7)	a. The engine coughed.	[*intransitive*]
	b. *I coughed the engine.	[*transitive*]

With respect to their argument structure, verbs fall into at least three classes: transitive (1), intransitive (6-7), and alternating (4-5). Argument structure is one of many complex aspects of language that we use instinctively. English speakers do not make mistakes about the facts in (1-7); English classes in high school do not dwell on them, and they are not discussed in popular newspaper columns about language. Because our mastery of these facts is so effortless, it is easy to assume that the explanation for these facts must be straightforward. We might think, for instance, that (6b) and (7b) are impossible because the sentences in question are meaningless.

But the problem with (6b) and (7b) is not a straightforward semantic one. It is easy to imagine what a sentence like (6b) could mean if it were grammatical (something like "I caused the baby to laugh," just as (4a) roughly means "I caused the ice to melt"). (6b) cannot mean this, however; such meanings must be expressed via more complex syntactic structures involving multiple verbs, like the ones in (8):

(8) a. I <u>made</u> the baby <u>laugh</u>.
b. I <u>made</u> the engine <u>cough</u>.

Moreover, as Hale never failed to note, these are not parochial facts about English. Navajo, for instance, has a class of intransitive verbs that add a prefix ł to form their transitive versions:

(9) a. Tin yí-yį́į́’
ice 3 melt.PERF
‘The ice melted’
b. Yas yí-**ł**-hį́į́’
snow 3.1s **ł** melt.PERF
‘I melted the snow’

(10) a. Tóshjeeh si-ts’il
barrel 3 shatter.PERF
‘The barrel shattered’
b. Łeets’aa’ sé-**ł**-ts’il
dish 3.1s **ł** shatter.PERF
‘I shattered the dish’

With another class of verbs, the transitive cannot be formed so simply; this latter class includes Navajo verbs like the ones meaning *laugh* and *cough*. With these verbs, more complex structures, roughly analogous to the English ones in (8), must be used to express causation of the event.

We find a very similar situation in Miskitu, a Misumalpan language of eastern Nicaragua and Honduras on which Hale did extensive work. In this language, there is a class of verbs that may appear in either transitive or intransitive forms (with the difference indicated by a suffix); these include the verbs for *melt* (transitive *slil-k*, intransitive *slil-w*) and *break* (transitive *kri-k*, intransitive *kri-w*). And, again, just as in English and Navajo, there are verbs that may only be intransitive, including the verbs for *laugh* (*kik*) and *cough* (*kuhb*).

In all three of these unrelated languages, then, some intransitive verbs may be made transitive, while others may not. Moreover, the particular verbs that fall into these classes are startlingly similar across languages, as we see in the charts below:

(11) VERBS THAT CAN BE TRANSITIVIZED

English	Miskitu		Navajo	
	intransitive	*transitive*	*intransitive*	*transitive*
boil	pya-w-	pya-k-	-béézh	-ł-béézh
break	kri-w-	kri-k-	-ii-dłaad	-ii-ł-dlaad
crack	bai-w-	bai-k-	-ii-ts'il	-ii-ł-ts'ił
dry (up)	lâ-w-	lâ-k-	-gan	-ł-gan
fill	bangh-w-	bangh-k-	ha-di-bin	`ha-di-ł-bin
float	â-w-	â-k-	di-'eeł	di-ł-'eel
melt	slil-w-	slil-k-	ghį́ į́ h	-ł-ghį́ į́ h

(12) VERBS THAT CANNOT BE TRANSITIVIZED

English	Miskitu	Navajo
cry	in-	-cha
cough	kuhb-	di-l-kos
laugh	kik-	ghi-dloh
play	pul-	na-né
shout	win-	di-l-ghosh
sing	aiwan-	ho-taał
sleep	yap-	i-ł-ghosh
snore	krat-w-	i-ł-ghą́ ą́’

As the charts show, all three of these unrelated languages have transitivity alternations in their words for *boil*, *break*, *crack*, *dry up*, *fill*, *float*, and *melt*, while all of them lack transitivity alternations of the same type in their words for *cry*, *cough*, *laugh*, *play*, *shout*, *sing*, *sleep*, and *snore*. As we saw in (8) above, the problem is not a straightforward semantic one, since it is clear what the transitive versions of these latter verbs would mean—yet the fact is that they cannot be made to mean this.

We are confronted, then, with a question: what constrains the ability of verbs to alternate between transitive and intransitive versions? We have seen that the answer to this question cannot be based on facts that are peculiar to English; what is needed is a theory that predicts that a verb that means *cough*, whatever language it finds itself in, will be unable to take a direct object, while a verb that means *float* will be able to do so.

In several decades of collaborative research with our colleague Samuel Jay Keyser, Hale developed a solution to this puzzle which has become standard in the field. Their answer to the puzzle is based on the idea that these verbs have a more complex structure than is immediately apparent. In particular, they attributed this more complex structure to the second, non-transitivizable type of intransitive verb (including verbs with meanings like *laugh* and *cough*). In order to explain their proposal, we will need to consider some general properties of the structure of words, and how they can vary cross-linguistically.

To begin with, words are not always atomic; they can consist of smaller parts, referred to as morphemes. Thus, the English words *un-faith-ful-ness* and *trans-it-iv-iz-able* are each composed of several morphemes, here separated by hyphens; similarly, the Swahili verbs below each consist of four morphemes, a verb preceded by three prefixes:

(13)	a. ni-li-ki-pata	I PAST it get	'I got it'
	b. wa-li-ki-pata	they PAST it get	'They got it'
	c. ni-ta-ki-pata	I FUTURE it get	'I will get it'

We can determine how these verbs are decomposed into morphemes by comparing minimally different pairs of words and observing the changes in meaning and form. (13a) and (13b), for instance, differ in form only in their first syllable (*ni-* vs. *wa-*), and differ in meaning only in their subject (*I* vs. *they*). We can tentatively conclude, then, that this first syllable is a morpheme, a prefix that indicates the identity of the subject—and further research into Swahili would back this up. Similarly, (13a) and (13c) differ only in their second syllables, and in tense, and we can rightly conclude that this second syllable is a prefix denoting tense. The study of morphemes, and the rules of their combination, is called morphology.

Two results from the study of morphology are of interest to us. One has to do with a cross-linguistic morphological difference. Languages vary in how much material may be put in a single word; we often find that one language communicates with a single word what another language requires several words to express. The Mohawk verb in (14a), for example, has the same meaning as the English sentence in (14b):

(14) a.Wa'- ke- nakta- hnínu -'
PAST I bed buy PUNCTUAL
b. I bought a bed.

As we can see in (14), Mohawk allows a verb and its object (here *hnínu* 'buy' and *nakta* 'bed') to become parts of a single word; in English the verb and its object must be separate words in this case.

The other fact about morphology that is relevant for our purposes is that morphemes may sometimes be inaudible. In English, for instance, the past tense on verbs is most commonly marked with a suffix *–ed*, but some verbs fail to take this suffix, taking a null suffix instead:

(15) a. play-*ed* (e.g., 'The band <u>played</u> yesterday')
b. put-*Ø* (e.g., 'The dragon <u>put</u> the villagers on the plate yesterday')

We can convince ourselves that there is in fact a past-tense morpheme in (15b) by considering the negative forms of these verbs. As shown in (16), negation in English is expressed with the word *not*, and if no auxiliary is present, the word *not* is preceded by the verb *do*. Moreover, this auxiliary *do* always takes whatever morphology the main verb would have taken if negation were not present:

(16) a. play-*ed* → a'. di-*d* not play
b. play-*s* → b'. doe-*s* not play

The main verb *play* is bare (suffix-less) in the negated examples in (16a') and (16b'); the morphology that would appear on it if negation were not present (*-ed* in [a'] and *–s* in [b']) appears on *do* instead. We can use negation, then, to see what kind of morphology appears on an English verb; whatever morpheme it is, it should appear on *do* when the verb is negated. Applying this diagnostic to *put*, we see that this verb does have a past tense morpheme attached to it when it is in the past tense, although this morpheme idiosyncratically fails to be pronounced on this particular verb:

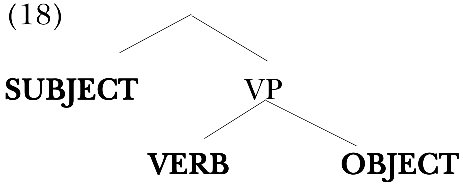
(17)	a. put-*Ø* →	a'. di-*d* not put
	b. put-*s* →	b'. doe-*s* not put

The past tense morpheme is not the only unpronounced morpheme in English; another such morpheme is the plural suffix on nouns like *sheep* and *moose* (we say "three sheep" or "five moose" but "three dog**s**" and "five cat**s**").

We have seen that words can consist of smaller pieces called morphemes; that languages can vary in whether they leave these morphemes as free-standing words or combine them into a single word; and that morphemes may be unpronounced. Let us end this section with one further observation about the nature of sentences: The words in a sentence are not simply concatenated, but are put together in hierarchical structures.

A clause containing a transitive verb, for example, has a tree structure (diagram) something like that shown in (18):

(18)



In this tree, the verb does not have the same kind of structural relationship with the subject that it has with the object; in particular, the verb and the object form a unit (labeled "VP", for "verb phrase", in the tree in (18)) which excludes the subject. This way of depicting the structure allows us to account for the fact that the verb and its object are treated as a unit by a number of syntactic operations, unlike the verb and its subject. We will give just one such operation as an example. The sentences in (19) are all more or less synonymous, with the differences between them having to do with emphasis:

(19) a. Mary bought a car.
b. The one who bought a car was Mary.
c. What Mary bought was a car.
d. What Mary did was buy a car.

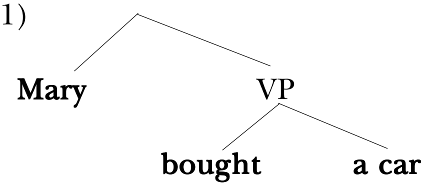
(19b-d) are all instances of what are known as pseudoclefts. Pseudoclefts are used to emphasize some particular part of the sentence. (19b), for instance, involves emphasis on Mary; this is the kind of sentence that might be uttered to contradict someone who had just asserted that John had bought a car. The emphasized material is placed at the end of the sentence, after the copula; in what follows, we will say that this post-copular material (i.e., *Mary* in [19b], and *buy a car* in [19d]) has been pseudoclefted.

The interesting property of pseudoclefts, from a linguist's perspective, is that pseudoclefting does not simply affect any randomly chosen string of words. (19b) above involves pseudoclefting of the subject (i.e., *Mary* is the subject of the verb *bought*); (19c), pseudoclefting of the object, and (19d), pseudoclefting of the entire verb phrase—that is, of the verb together with its object. It is, however, impossible to pseudocleft the subject together with the verb:

(20) *What did a car was Mary buy

This property of pseudoclefts may be captured straightforwardly if we recognize that (19a) is not simply a string of words, but a hierarchical structure like the one depicted in (21).

(21)



Associating the words of this sentence with a tree of this kind amounts to a claim about which word sequences are units that syntactic operations may affect; in particular, it illustrates that only sequences that are exhaustively dominated by single nodes in the tree are syntactic units. There is such a unit that consists of the string of words *bought a car*; this is the VP, which is connected by lines which point down from it to these words, and to no others. But there is no such unit connected just to the words *Mary bought*.

This approach represents the argument structure of a verb in terms of syntactic structure; words are organized into hierarchical structures, represented above as trees, and the properties of these structures are partly determined by the verbs that appear in them. A speaker's knowledge of a word consists not only of knowledge of the word's sound and meaning but also of its argument structure (i.e., the place occupied by the word in tree structures like the ones above). Some of the most important advances in linguistics that have been made during the last half century by Hale and others involve operations on the argument structures of words and sentences. Since these computations reflect claims about computations performed by actual speakers in producing actual sentences, the question may well arise as to the nature of the neurophysiological substrate of these computations. Our answer must be that at this time very little is known about this matter. This aspect of the present situation in linguistics is comparable to that of chemistry in the middle of the nineteenth century, where many aspects of chemical compounds were explained in terms of valence even though the physical basis of valence was not properly explained until many decades later (by Linus Pauling [NAS 1933]). In fact, as a result of the work of the last half-century we now have a different and much richer picture of the nature of argument structure than ever before. We see these discoveries as providing boundary conditions that the neurology of the future must satisfy.

HALE'S ANSWER: DERIVED INTRANSITIVITY

We can now consider Hale and Keyser's proposal about the nature of the untransitivizable intransitive verbs (e.g., *laugh*, *cough*). Their proposal is that verbs of this type have a more complex structure than is immediately apparent. A verb like *laugh*, on their view, has an underlying structure something like that of *do a laugh*, consisting of a transitive verb with a meaning like *do* that takes a noun *laugh* as its object (cf. *do a handstand*, *do a double take*). The verb *laugh* differs from the phrase *do a laugh* in two respects, both of which relate to properties of morphology that we have just discussed. One is that, like the Mohawk verb *wa'kenaktahnínu'* 'I bought a bed' in (14a), the verb *laugh* in English combines into a single word the morphemes that remain separate in the phrase *do a laugh*. The other is that several of the morphemes that combine to make the verb *laugh* are unpronounced in English; in fact, the only morpheme that we hear pronounced is *laugh*, which is actually the direct object of an unpronounced verb meaning *do*.

This idea came to Hale and Keyser from their observations about the nature of these verbs in a variety of languages. In Basque, for instance, the composite nature of verbs of this type is obvious, since the relevant verbs consist of a nominal element attached to a verb *egin*, which means something like 'do':

(22)	a. negar egin	cry do	'cry'
	b. barre egin	laugh do	'laugh'

In other words, verbs like *laugh* and *cry* in Basque involve expressions not unlike *do a handstand* or *do a dance* in English. Such expressions describe an action in the way that a verb would, but the verb itself contributes little to the meaning of the expression, which mostly comes from the noun associated with the verb.

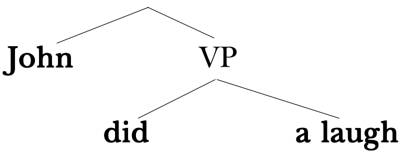
Similarly, in languages like Navajo (and in English, for that matter), this kind of verb is often transparently related to a corresponding noun:

(23)	a. ghi-dloh	'laugh (v.)'
	b. dlo	'laugh (n.)'
	c. di-zheeh	'spit (v.)'
	d. -zhéé'	'spit (n.)'

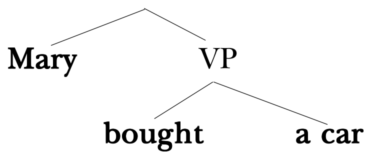
Hale and Keyser's solution to this problem involves attributing to these verbs meanings (and argument structures) something like *do laughing* or *do spitting*. These verbs have a complex argument structure that is effectively that of transitive verbs, consisting internally of a verb (like *do*) with a nominal object (like *laughing*, *spitting*). Because they are already transitive, they cannot be "transitivized" as other, truly intransitive verbs can. "He laughed the baby," in this view, is unacceptable for the same reason that "He did a dance the lady" would be.

Hale and Keyser's claim about the argument structure of a verb like *laugh* is a claim about the kinds of tree structures in which this verb may participate. According to Hale and Keyser, *John laughed* is to be associated with a tree very similar to the one in (21) above—repeated here as (25)—for *Mary bought a car*:

(24)



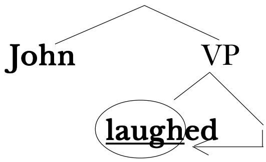
(25)



In (24), as in (25), the verb phrase consists of two main elements, a verb and its object. The claim made and defended by Hale and Keyser is that some verbs—in particular, verbs of the *laugh* class—are stored in speakers' memories with complex structures of this type. In their approach, such verbs are not simply atomic units; rather, they consist of a verb with little semantic content (referred to in the literature as a "light verb," and represented here with the English verb *did*), combined with an object that contributes much of the meaning of the verb (in this case, *laugh*).

In a language like Basque this is a straightforward representation of how these verbs appear in sentences (cf. (22)). In English, a special, language-particular condition affects the way this transitive verb is incorporated into the structure of a sentence. In English the object of this verb must become part of the verb, obscuring its underlying transitivity:

(26)



This account of Hale's requires us to be willing to entertain the possibility that the structure of a sentence of English (or Navajo, or Miskitu) might not be exactly what it appears to be. In this particular case, the facts of Basque suggest that some apparently intransitive predicates are in fact transitive; and, as Hale pointed out, if we make the assumption that all languages, including English, share this property of Basque, we arrive at a straightforward explanation for why such verbs cannot be made transitive. In Basque, the transitive nature of these verbs is obvious, while in English, it has to be inferred by the study of phenomena like transitivization.

Hale's account thus applies lessons learned from the study of Basque to the analysis of English, Navajo, and Miskitu. This kind of move, and the empirical success to which it has led, is one of the triumphs of an idea to which we alluded earlier: Underlying the obvious diversity of human languages are some invariant principles, which reflect the fact that all humans employ the same neurophysiological machinery to speak and understand what others say. Since all of us share the same neurophysiology, it is hardly surprising that all languages are constructed on principles of a single kind, those of Universal Grammar. Hale and Keyser's proposal is that properties of Universal Grammar guarantee that verbs with meanings like *laugh* and *spit* will be, on some level, transitive verbs.

The account that Hale and Keyser develop is based on an approach to argument structure that is dynamic rather than static. In this approach, the computation of a sentence involves multiple steps, and the answer to a question like "is this verb transitive?" can change in the course of the computation. Principles of Universal Grammar determine that in all languages, verbs with the meaning of *laugh* will be stored in the memory of the speaker as transitive verbs, involving a structure something like that of (27):

(27) John did a laugh

Hale and Keyser thus argue that it is possible for a verb that is transitive in the mental lexicon of the speaker to become intransitive when it is made part of a sentence. Statements about the argument structure of verbs, then, will have to be made with this possibility in mind; we cannot simply declare a verb to be 'intransitive', without stating whether we are discussing the representation of that verb in the speaker's memory or its (potentially distinct) representation as part of the syntactic structure of a sentence. In what follows we will refer to verbs like *laugh* as underlyingly transitive (that is, transitive in the speaker's memory) but surface intransitive in a language like English (where such verbs are made intransitive as part of their incorporation into the structure of a sentence.) In Basque, by contrast, these verbs are both underlyingly transitive and surface transitive. Similarly, it will be useful for us to distinguish between the underlying object and the surface object of a verb; *laugh*, for instance, has an underlying object in its representation in the speaker's memory, but no surface object in a language like English (while in Basque, the underlying object of this verb is also its surface object). In the same way, we will refer to verbs as having underlying subjects and surface subjects.

TYPOLOGIES OF VERBS

Hale then turned to the next logical question: What are the aspects of meanings like *laugh* and *cough* which determine that verbs with these meanings will be underlyingly transitive? To answer this question, we must consider the nature of the process of transitivization that can apply to verbs like *break* and *melt* (cf. (4-5)); what exactly is happening to these verbs?

(28) a. The hammock broke.
b. Mary broke the hammock.

(29) a. The butter melted.
b. John melted the butter.

Hale was able to build on a long tradition of syntactic work on alternations like those in (28-29). The meanings of the (a) and (b) sentences above are clearly connected; for the (b) sentences to be true, the (a) sentences must also be true. But the (b) sentences add another item of information; they tell us who is responsible for causing the event described by the (a) sentences. It would seem that making these verbs transitive involves adding a subject, who is described as causing an event to happen: *Mary*, for example, in (28b), and *John*, in (29b).

This is not how we are used to thinking about transitivity; normally, verbs are described as transitive or intransitive depending on whether they have an object. And it is true that the intransitive (a) sentences above lack an object, while the transitive (b) sentences have one. But this way of describing the situation overlooks an important fact:

the subjects of the (a) sentences above are the objects of the corresponding (b) sentences.

Why is *the hammock* the subject of (28a), but the object of (28b)? We have just offered the hypothesis that transitivization involves adding a subject that causes the event to happen, and this hypothesis answers part of this question; transitivization causes *Mary* to be the subject of (28b), which means that *the hammock* must be the object, since (for reasons that we will not try to explore here) there cannot be two subjects. But if *the hammock* is the object of (28b), why is it not also the object of (28a)?

The perhaps obvious answer is that if *the hammock* were the object of (28a), the resulting sentence would lack a subject:

(30) *broke the hammock

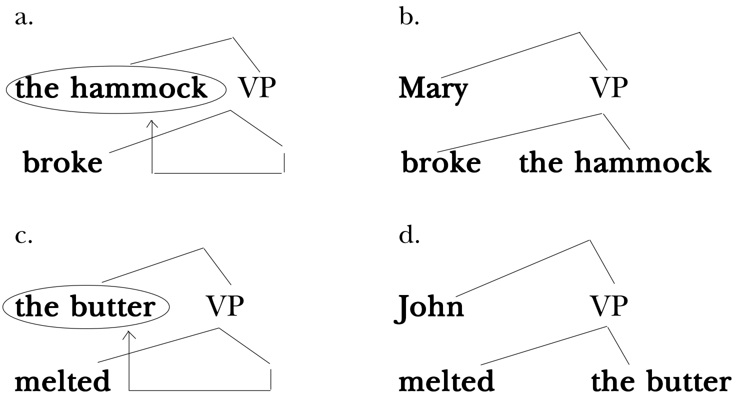
In fact, there are no grammatical English sentences without a subject. Apparently there is some principle requiring all clauses (of English, at least) to have subjects, and (30) violates this principle. We can posit that *the hammock* is the subject of (28a), not because of properties of the verb *break*, but simply because the clause must have a subject and *the hammock* is the only available noun phrase. Verbs like *break* and *melt* invariably take an underlying object, which denotes something that has undergone a change of state as a result of the event described by the verb:

(31)

a. b.

In (28a) and (29a), these underlying objects (*the hammock* and *the butter*) have been forced to become surface subjects, since the clauses must have surface subjects and no other noun phrases are present. In the (b) sentences, transitivization has provided an alternative subject (*Mary* and *John*, respectively), allowing the underlying objects of these verbs to retain their object status:

(32)



This conclusion about the nature of the intransitive verbs of this class—that their surface subjects are actually underlying objects—has a long tradition in syntactic theory, and is richly supported by data gathered by Hale and others from a variety of languages. One English piece of evidence comes from the behavior of what are called *resultatives*, some of which are exemplified in (33) (the resultatives are italicized):

(33) a. They pounded the metal *flat.*
b. She smashed the vase *into smithereens.*

The examples in (33) involve some object changing state; the resultative denotes its new state. For instance, in (33a), the metal goes from being non-flat to being flat. In transitive sentences, resultatives invariably denote the new state of the object; sentences like the ones in (34) sound odd:

(34) a.* They pounded the metal *sweaty.*
b.* She smashed the vase *very satisfied.*

(34a), for example, cannot be used to mean that they pounded the metal until they became sweaty.[3] Resultatives, then, apparently have some kind of privileged relation with the direct object, which we have stated in (35):

(35) A resultative denotes the end state of the direct object.

However, we have now seen that identifying nouns as direct objects of verbs is not entirely straightforward; in the terms introduced above, we need to distinguish between underlying objects and surface objects. For instance, a noun may start as a direct object and then become something else (such as the subject, or part of the verb), yielding an apparently intransitive verb on the surface. In a theory that posits syntactic operations of this kind, we need to find out whether terms like "direct object" in (35) refer to underlying objects or to surface objects. Let us consider the interaction of these resultatives with the different types of intransitive verbs.

We considered two of Hale's arguments above for the conclusion that some intransitive verbs have an underlyingly transitive structure; first, these verbs are transparently transitive in languages like Basque, and second, the assumption that these verbs are underlyingly transitive in languages like English makes it possible to explain why such verbs may not be transitivized. This kind of reasoning, drawing information from one language to shed light on the mysteries of another, was one of Ken Hale's greatest talents. On the view embodied in this kind of work, different languages offer windows onto different parts of a single puzzle, namely the nature of the human language faculty.

We must be careful not to overlook differences among languages when they do arise, and the reader may be concerned that we have been too quick to conclude that English and Basque have deep syntactic properties in common. In fact, the behavior of resultatives offers a new kind of argument for the conclusion that in English, as in Basque, some apparently intransitive verbs are underlyingly transitive. If we consider the behavior of resultatives in intransitive sentences, we find two major types. For one type, resultatives cannot appear at all:

(36) a.* I laughed hoarse.
b.* She coughed dizzy.

These sentences do not have resultative readings; for instance, (36a) cannot mean that I laughed until I became hoarse. For a second class of intransitive verbs, the resultative denotes the end state of the <u>subject</u>:

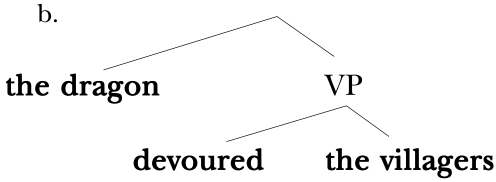
(37) a. The vase broke *into smithereens.*
b. The butter melted *into a puddle.*

These are the two classes of intransitive verbs that Hale and Keyser are concerned with; verbs like *laugh* and *spit* underlyingly have direct objects that ultimately become part of the verb, while verbs like *break* and *melt* have direct objects which change into subjects. The facts in (36-37) follow, and are instances of the condition in (35). In (36), the resultative attempts to modify the subject, in violation of (35). In (37), by contrast, (35) is satisfied because the subjects to which the resultatives apply are underlying objects. We may state (35) more precisely as (38):

(38) A resultative denotes the end state of the underlying object.

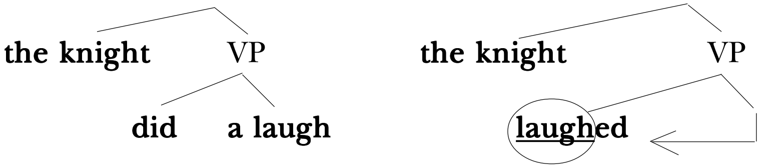
To summarize, then, Hale and Keyser posit three major types of verbs. Ordinary transitive verbs have both a subject and an object:

(39) a. The dragon devoured the villagers.
b.



In addition, we find two categories of verbs that appear to be intransitive in English. Hale and Keyser posit one set of verbs (including *laugh* and *spit*) that are underlyingly transitive with a 'light' verb that contributes little to the meaning of the clause. For these verbs, the underlying object of the verb becomes part of the verb, yielding a surface intransitive verb:

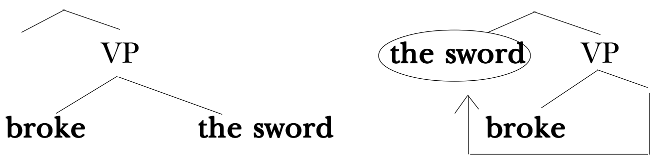
(40) a. The knight did a laugh → The knight laughed.
b.



Finally, there are verbs that underlyingly have no subject at all (*melt* and *break*.) The underlying object of such verbs must become the surface subject, satisfying the requirement in English that all clauses have a surface subject:

(41) a. broke the sword → The sword broke.

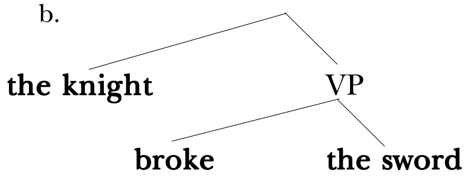
b.



Transitivization, then, involves adding an agent responsible for causing an event to take place, which becomes the surface subject of the clause:

(42) a. The knight broke the sword.

b.



A subject can be added to verbs like the one in (41), but not to verbs like the ones in (39-40), which already have underlying subjects. The account therefore correctly divides surface intransitive verbs into two types; verbs like *break*, which can be transitivized by adding a subject, and verbs like *laugh*, which are in fact already transitive and therefore cannot be transitivized.

Hale's typology of verbs involves two main principles (43), and at least three processes (44) that sometimes make discovering these principles difficult:

(43) a. All verbs must have underlying objects.

b. All clauses must have surface subjects.

(44) a. In English, light verbs sometimes 'absorb' their underlying objects: *laugh*, for example, in (40).
b. 'Transitivization' adds an underlying subject to a verb that lacks one: *The knight broke the sword* in (42).
c. If a verb has no underlying subject, the underlying object may become the surface subject, as in *The sword broke* in (41).

We can return now to the question with which we began this section. We have seen that *break*, in a sentence like (41), combines with a single noun phrase that starts out as its underlying object, before later becoming its surface subject. With verbs like *laugh*, on the other hand, the surface subject—*the knight*, in (40)—is also the underlying subject (that is, these verbs are stored in the speaker's memory as requiring subjects). Why do these verbs differ in this way? What is it about *break* and *laugh* that causes them to behave syntactically as they do? We have seen that the behavior of these verbs is remarkably consistent across languages, so our answer should not simply be that the verbs are arbitrarily classified, as exhibiting this particular behavior.

A better answer to this question may be inferred from the nature of transitivization. We have seen that this operation adds an underlying subject, which is the agent responsible for causing an event to take place. A crucial difference between *laugh* and intransitive *melt* is that laughing is something an individual can do on purpose, while melting is not—that is, the subject of *laugh* is an agent, unlike the subject of *melt*. Hale claimed that this fact about the meanings of *laugh* and *melt* has repercussions for the way these verbs are associated with syntactic structure. Only agents, in his view, may be <u>underlying</u> subjects; non-agents may become surface subjects, but must be underlying non-subjects.

Hale's proposals about argument structure are proposals about the nature of Universal Grammar. To Hale, all of us are born knowing general principles like (43a) and the requirement that underlying subjects be agents. No matter what language a young child is acquiring, she is able to correctly deduce that the single argument of a verb with the meaning of intransitive *break* must be an underlying object. Since it is not an agent, it cannot be an underlying subject, and it becomes a surface subject only because of a requirement that the clause have a surface subject.

This approach succeeds in reducing to a minimum the task faced by children learning the vocabulary of their native languages. As we mentioned above, the mapping between sound and meaning varies arbitrarily across languages; there are no principles of Universal Grammar that guarantee that a verb pronounced *break* must mean what *break* means in English. Clearly, learning a word must involve learning its pronounciation and its meaning; Universal Grammar is of no help in these tasks. What Hale established is that once the child has learned what a verb means, she has also learned its argument structure. Having learned what *laugh* means, for instance, she is in a position to conclude, on the basis of conditions like those in (43-44), that it is underlyingly transitive, and cannot be straightforwardly transitivized. The argument structure of words need not be learned independently, but follows from the general principles that map meaning onto structure. If Hale is correct, then a number of questions arise. Why must clauses have surface subjects? Why must verbs have underlying objects? What properties of agents constrain their syntactic behavior? It is perhaps one of Ken Hale's greatest legacies that he left us with questions like these to answer.

Hale's proposals about these general principles are proposals about the nature of the human mind. Part of what it is to be a human being is to have a mind that constructs grammars incorporating the requirements in (43), and every normal human being is born with such a mind. Ultimately, of course, we would like to have a theory that explains how principles like the ones in (43) are implemented in the neurophysiology of the brain, and work intended to develop such explanations is under way. Hale's career is a testament to the fact that one can make progress on questions about the properties of the mind without directly investigating the implementation of those properties in the brain. In fact, it would be impossible to develop such theories of neurological implementation without a clear understanding of what is to be implemented, and as we have tried to show, the properties of human grammar are more complex than they might appear at first sight. Our understanding of these complex properties owes an enormous debt to Hale's work.

Limitations of space and time make it impossible for us to fully describe the extent of Hale's many other contributions to linguistic theory. Hale worked on historical reconstruction of the Australian language families, on intonation in Tohono O'odham, on stress in Hocąk, on agreement in Irish and K'ichee,' on the phonology and semantics of a sacred initiation language of the Lardil called Damin, and on countless syntactic issues in languages from Warlpiri to Dagur to Navajo. He produced dictionaries of Lardil and of Ulwa, and contributed extensively to a dictionary of Warlpiri, and to educational materials in countless other endangered languages. He was the first, and in many cases, the only researcher to document the vocabulary and structure of dozens of aboriginal languages of Australia. He lived the kind of life that no set of writings can do full justice to. He was a great man, and we count ourselves fortunate to have known him and worked with him.

In writing this piece we benefited greatly from comments by Noam Chomsky, Heidi Harley, Jay Keyser, Mary Laughren, David Nash, David Pesetsky, Norvin Richards, and Jane Simpson, and we are very grateful for their help. Responsibility for any remaining errors is entirely ours.

NOTES

1.J. Simpson, D. Nash, M. Laughren, P. Austin, and B. Alpher. Forty years on: Ken Hale and Australian Languages. *Ogmios 2.5*, no 17, summer 2001, p.3

2.The name is perhaps an unfortunate one, since it is not intended to refer to the grammar of any particular language, but rather to properties which universally hold of human languages.

3.(34a) may have another, irrelevant reading, in which they pounded the metal while they were sweaty, with no change of state implied. This reading treats sweaty not as a resultative but as a *depictive*, which is subject to different conditions.

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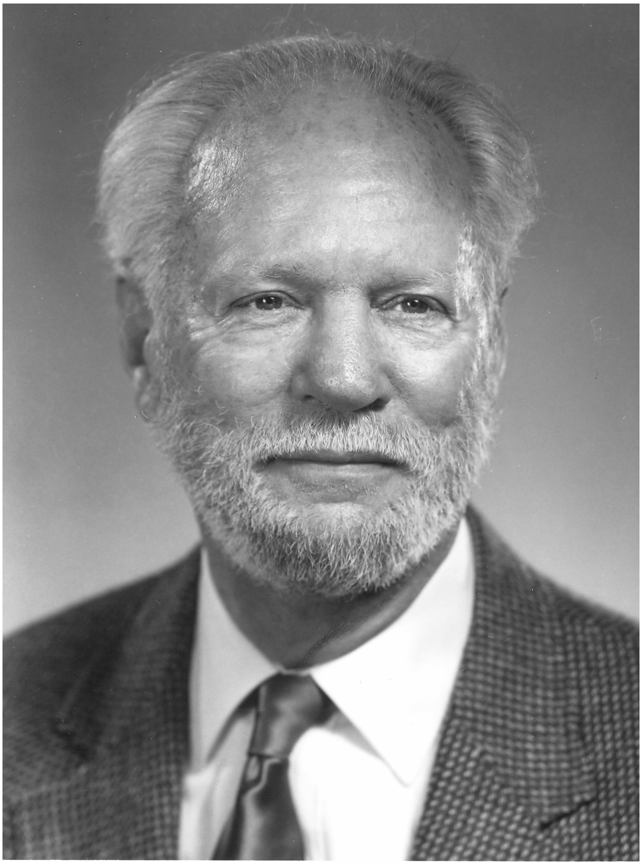

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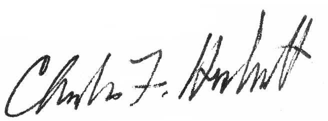

CHARLES F. HOCKETT

January 17, 1916–November 3, 2000

BY JAMES W. GAIR

CHARLES F. HOCKETT—KNOWN to friends, students, and colleagues as "Chas"—was a leading figure in American structuralist linguistics, which flourished particularly in the four decades from the 1930s to the 1960s and did much to define linguistics as a science. Structuralist linguistics was sometimes referred to as Bloomfieldian linguistics from one of its pioneering figures, Leonard Bloomfield, who produced the seminal 1933 work *Language.* Hockett considered Bloomfield his master, and referred to his own influential 1958 work *A Course in Modern Linguistics* as "a commentary on *Language.*" Hockett was considered by many to be the brightest young contributor to linguistic theory in the framework of structural linguistics, to which he contributed a number of basic concepts and issues. But he was by no means narrow in his scope, and he firmly believed linguistics to be a branch of anthropology, to which he also made serious contributions.

Hockett was the fourth child of Homer Carey Hockett, who taught American history at Ohio State University, and Amy Francisco Hockett. He entered Ohio State in 1932 at

This obituary is largely drawn, with permission, from one by the same author that appeared in the Linguistic Society of America journal *Language* 79(3) (Sept. 2003):600-613.

the age of 16, and in the spring of 1933 took George M. Bolling's linguistics course in which the textbook was the newly published Bloomfield work referred to above. Subsequently he took the only course in anthropology available at the time, and those experiences set him on the path to his future academic career. Hockett received his B.A. (summa cum laude) and M.A. simultaneously in ancient history at the age of 20, with a dissertation on the use of the Greek word *logos* in philosophy through Plato. Years later he described the introductory section of that work as showing "despite some weird use of terms . . . the Bloomfieldian impact" (1977, p. 1). He continued at Yale University, studying anthropology and linguistics with Edward Sapir, Franklin Edgerton, George P. Murdock, and Leslie Spier, also having Morris Swadesh, George L. Trager, and Benjamin Whorf as teachers and associates. Hockett received his Ph.D. in anthropology in 1939, with a dissertation based on his fieldwork in Potawatomi. His paper on Potowatomi syntax was published in *Language* in that year (1939), and the dissertation, in streamlined form, was published as a series in the *International Journal of American Linguistics* in 1948. After a summer of fieldwork in Kickapoo and an autumn in Michoacán, Mexico, he went on to two years of postdoctoral study, including two quarters with Bloomfield at Chicago, followed by a stay at Michigan.

Hockett was drafted into the U.S. Army in February 1942. After basic training in antiaircraft artillery and a few months helping to prepare other recruits for officer candidate school, he was transferred to Army Service Forces, where his linguistic capabilities were put to work on Chinese. In late 1942 he accompanied General Stillwell's officers to their headquarters in Bengal, India, supervising their learning of Chinese while en route. Afterward Hockett was stationed in Washington and then in New York City, where he worked under Major Henry Lee Smith in the dedicated and productive group preparing language-training materials, language guides, and dictionaries for military personnel. This unit numbered among its personnel or associates a number of the leading linguists of the time, and the effort allowed the application of a Bloomfieldian structural linguistic approach to language teaching on an unprecedented scale. It thus served as a testing ground and laboratory for the applicability and effectiveness of that approach. The materials produced there were later put to use in many postwar civilian programs, particularly in the less commonly taught languages, and they became the model for many subsequent texts. In the course of this work Hockett, with C. Fang, produced a basic course in spoken Chinese (1944) and a guide's manual for it, as well as a Chinese dictionary (1945) that included an introductory sketch of Chinese that was notable for both conciseness and clarity. He was commissioned as a second lieutenant in 1943, and after the Japanese surrender in 1945 was dispatched to Tokyo as a first lieutenant to help train U.S. troops in Japanese. In February 1946 he was separated from the army with a terminal leave promotion to captain.

After a short association with the *American College Dictionary*, he began his university teaching career in 1946, as an assistant professor of linguistics in the newly formed Division of Modern Languages at Cornell, a pioneering unit designed specifically to unite linguistics and language teaching on the university level following the model of the successful wartime effort. The division, which later morphed into the Department of Modern Languages and Linguistics, was given the responsibility for basic language teaching for virtually all languages at Cornell, a function it retained in a widening number of languages until recently. It also served as the home for the graduate and subsequently the undergraduate program in linguistics. Hockett was in charge of Chinese and continued to run the Chinese program for 15 years, while teaching a range of linguistics courses and directing students. Along with him were some of the leading names in structural linguistics, both descriptive and historical, including William Moulton, Robert Hall, Frederick Agard, and Gordon Fairbanks, all of whom directed and taught in language programs and carried out productive research and teaching in linguistics. Hockett once described the situation as "in effect, a linguistics institute in permanent session" that "permitted me to spend most of my time just as I have wanted to, in linguistics and anthropology alike"(1980, p. 104). His Cornell obituary describes him as having been "the soul of the linguistics program from his first years until his retirement in 1982, serving on the committee of almost all students enrolled in linguistics during his time and serving as director of 25 Ph.D. dissertations." (He played a major role in the training of many more.) In 1957 he was invited to become a member of Cornell's Department of Anthropology, and he was later named the Goldwin Smith Professor of Linguistics and Anthropology at Cornell, where he remained until his 1982 retirement to emeritus status.

Hockett was elected to the National Academy of Sciences in 1974; he was also a member of the American Academy of Arts and Sciences. He served as president of the Linguistic Society of America in 1964. In 1982 he was president of the Linguistic Association of Canada and the United States and in 1986 he was the distinguished lecturer of the American Anthropological Association. He held visiting positions at a number of institutions, and throughout his career he gave invited lectures at a number of U.S. and foreign institutions. Starting in 1986, he was first visiting professor and then adjunct professor of linguistics at Rice

University in Houston, Texas, an appointment still in effect at the time of his death.

Hockett had a long and productive career. His Festschrift (Agard et al., 1983) contains the last available full bibliography. It lists 133 published items; he also produced many privately reproduced items presented to students and colleagues. He continued to publish after his retirement, though at a much reduced pace, as he turned his attention increasingly to other interests, especially music.

Though Hockett studied and associated with several leading figures in American structural linguistics, Bloomfield was unquestionably the major influence on and model for him. Hockett was widely considered Bloomfield's chief disciple, and the most prominent explicator and elaborator of Bloomfield's works. He was also the direct inheritor of Bloomfield's unfinished work, and he collected, edited, reworked, and published much of that work, including *Eastern Ojibwa Grammar, Texts and Word Lists*, and *The Menomini Language*. In 1970 he produced *A Leonard Bloomfield Anthology*, with a slightly revised version of his own "Implications of Bloomfield's Algonquian Studies," which had originally appeared in *Language* two decades earlier. In addition, he considered his own works on Algonquian languages, extending throughout his career, to be a tribute to the master.

Like Bloomfield, Hockett was himself a master of linguistic description, producing numerous principled, meticulous, and perspicacious descriptions of an array of languages, including not only the Algonquian studies that he was most recognized for but also Chinese, Fijian, and English. American structural linguistics, consistent with its empirical orientation, always had a strong descriptive component. Much of its impetus and many of its concepts grew out of and were inspired by the work of language description, particularly of "exotic" languages like Amerindian ones that exhibited structures very different from those found in the more common ones of Europe. Those languages were consequently resistant to analysis in terms developed for the latter and required the development of new armament. Thus, in common with the practice of other linguists in that school, Hockett's descriptive works often served as the vehicle for the presentation of theoretical proposals, as in his "Peiping Phonology" (1947), "Componential Analysis of Sierra Popoluca" (1947), and "Peiping Morphophonemics" (1950), among numerous others.

His directly theoretical productions were legion, and many of them were legendary, working their way into much of the work of structuralist linguists and becoming part of the conceptual equipment of several generations of students. The neo-Bloomfieldian structuralist linguistics of the 1940s and 1950s was developed by a number of productive linguists, including Bernard Bloch, George Trager, Henry Lee Smith, and Zellig Harris, but arguably Hockett was the single most productive and wide-ranging figure in the establishment of the parameters of the enterprise, and in discerning, defining, and elaborating issues that needed to be faced in that work. It is instructive that the volume *Readings in Linguistics* (Joos, 1957), which was intended to be a kind of representation of the status of the field, contained seven of Hockett's papers, more than any other contributor (runners-up were Bernard Bloch and Zellig Harris, each with four).

Hockett's *A Manual of Phonology* (1955), though a solidly structuralist work, was to a degree revolutionary, characteristically original, and rich in content. It attempted a principled typology of phonological systems in the spirit of Troubetzkoy and the Prague school, argued for immediate constituents in phonology in a framework that included the syllable, and developed a system of phonology based on distinctive features and the recognition of long components. As he duly acknowledged, many of its elements were already present in the field in some form, but their combination and development were innovative and, typical of much of his work, went counter to much of the prevailing structuralist practice and doctrine. They also foreshadowed elements in later work in different frameworks, to an unfortunately often unrecognized extent.

Hockett had a remarkable gift for mathematics and for comprehending and working with mathematical and formal systems. In 1953 he produced a review of Shannon and Weaver's work on communication theory, and the information-theoretical approach became, as he put it in *The View from Language* (1977, p. 19), part of his standard intellectual equipment. One result was the inclusion in *A Manual of Phonology* (1955) of an introductory section presenting a finite state, Markovian view of speech communication and grammar, essentially of the kind that Chomsky famously critiqued in *Syntactic Structures* (1957). Hockett quite soon rejected that approach as not fitting the nature of human language, while retaining the view that information science had important contributions to make to linguistics.

In 1966 he produced an extensive paper "Language, Mathematics and Linguistics" in which he attempted to explore the formal properties of natural language that were susceptible to mathematical treatment. Ultimately, he also came to reject that endeavor as futile, except for some implications for sound change (1977, p. 19).

Hockett's best-known work was undoubtedly the 1958 textbook *A Course in Modern Linguistics*, which was widely used for many years. He considered this to be essentially a commentary on and updating of Bloomfield's *Language* and to a great extent, the pattern of topics covered in the book echoes that earlier work, covering a wide range of areas in the study of human language, but introducing some new topics and omitting others. Though he considered the tenor of the work to be "conservative," and presenting "the generally accepted facts and principles of the field" (p. vii), when compared to other introductory texts it appears as a highly personal, original, and sometimes challenging work. It incorporated many of his own interests and much of his work, and consistent with his anthropological orientation, he included a chapter "Man's Place in Nature" (1973), which contained the first publication of seven of his design features of communicative systems.

Hockett's treatment of grammatical analysis, especially syntax, in *A Course in Modern Linguistics* is especially interesting, and hindsight endows it with an element of dramatic irony, since generative grammar was looming on the horizon. It was in that book that Hockett introduced his concept of "surface and deep grammar." It was a direct exemplification of his ability to perceive and isolate phenomena that had to be accounted for in any full account of language but that were at the time not amenable to or expressible in the canons of scientific linguistics to which he subscribed. In this case the stimulus was the result of his not failing to notice the pervasiveness and unavoidable importance of syntactic relations between noncontiguous elements, of a kind that would later be called "long distance dependencies." As he recalled years later, "At that period in American linguistic theory . . . if two forms stood in a construction, then we expected them to be adjacent and to be parts of a larger form that we called the constitute. Apparent connections at a distance were therefore embarrassing" (1997, p. 160). Such connections were admirably amenable to transformational treatment, and thus given later developments in transformational-generative grammar, Hockett's use of terms was to a degree prophetic.

A Course in Modern Linguistics turned out to be the last major textbook summary of American post-Bloomfieldian structuralism, since its appearance essentially coincided with the appearance of Chomsky's *Syntactic Structures* (Chomsky, 1957) and Lees's laudatory review in *Language* (Lees, 1957) that foreshadowed the ultimate dominance of generative grammar. Though neither Chomsky nor Lees appear in the index of the work or are treated in the text itself, they are listed in the bibliography, and there is a note at the end of one chapter that the transformational approach of Chomsky, Harris, and Lees came too late to be worked into the treatment (1958, p. 208).

In 1961 Hockett published a paper "Linguistic Elements and Their Relations" that in hindsight marked a turning point in his own views on language and its investigation, and to a degree signaled the end of structural linguistics as it had existed. It was an elegantly conceived attempt to solve the fundamental problem faced by structuralist descriptive linguistics: the fact that the elements that structuralist descriptive linguistics recognized as basic, such as phones, phonemes, and morphemes, did not occur in a linearly parallel and compositional hierarchy of levels as many structural linguists had envisioned. Hockett's solution was to propose grammatical and phonological strata, with the "composed of" relation holding only between elements within each stratum, and the strata linked by a mapping relation between them, but he ultimately rejected that as well.

That 1961 paper closed with a characteristically Hockettian passage raising the possibility that the kind of linguistics that led to the problem in the first place, and hence the paper itself, might be misdirected and inadequate to deal with natural language:

In closing this paper, I must for the sake of honesty mention a suspicion that cannot be followed through in detail here, but that if verified, is due to undermine the logic of most of our accomplishments in descriptive linguistics since Saussure, Sapir, and Bloomfield, or even an earlier period" (1961, p. 52).

What was at issue here was the underlying assumption that every occurring utterance in any given context had a specific "determinate grammatical structure involving an integral number of grammatical elements in specifiable structural relations with each other," which he saw as making linguistics as it stood inadequate to deal with such inescapable and natural phenomena of language in action as blends, which "are not rare, but extremely common," and "occur not only as "slips of the tongue" (whatever that means) but also as planned puns, double entendres, plays on words, and variously in poetry and advertising." In dealing with these, there were three possibilities that he saw: (1) linguistics as it was then practiced could allow them to be ignored, (2) they could be regarded as deviations to be explained with additional special machinery, or (3) they could be used as evidence for "some new and very different theory of the generation of speech that would provide at once for such "deviant" utterances and for all "regular" utterances."

At the time, Hockett was already shifting his perspective to an insistence on a more dynamic approach that focused on the hearer's competence and behavior in real time. In part, this shift was stimulated by work that he had done in the 1950s in a project with the psychiatrists Robert Pittenger and Jack Danehy that involved a number of other anthropologists, linguists, and kinesicists and produced a fine-grained analysis of the first five minutes of a psychiatric interview published in 1960. As he remarked is his 1977 preface to the reprinting of his 1960 paper "Ethnolinguistic

Implications of [Recent] Studies in Linguistics and Psychiatry":

It was Birdwhistell's kinesics, Smith and Trager's paralinguistics, and the psychiatric-interview context that gradually rendered me uncomfortable with post-Bloomfieldian 'marble slab' grammar with its atomic morphemes and that forced me to try to look at language in action" (1977, p. 107).

The metaphor in "marble slab grammar," of course, invokes anatomy and the dissection of a cadaver; his discomfort with that approach to grammar led to an increasing conviction that what had to be central to an adequate linguistic theory had to be a hearer-centered, dynamic approach.

Ultimately, pursuing this line of thought led him to reject what he saw as the three pillars of post-Bloomfieldian linguistics that he himself had played such a large part in developing and defending. As stated in the 1968 *The State of the Art,* these were "the characterization of language as a 'rigid' system; the adoption of an item and arrangement model; and the consensus that grammar and semantics were separable and should be separated"(pp. 31-32). His proposals leading toward the new kind of linguistics that he suggested in the finale of his 1964 presidential address were already present in the 1960 paper "Ethnolinguistic Implications of [Recent] Studies in Linguistics and Psychiatry," as well as in the paper "Grammar for the Hearer" of the same year, and in subsequent works. The developing stance that he expressed there remained a dominant theme in his work from then on. In his last book he stated (emphasis as in the original), "Our fundamental question can be phrased as follows: WHEN WE HEAR SOMEONE SAY SOMETHING IN A LANGUAGE WE KNOW, HOW DO WE KNOW WHAT IS SAID" (1987, p. 2.)

The thread that ran through all this later work was his rejection of the concept of language as a well-defined system that he saw as central to generative linguistics and in part at least as having its origins in the kind of structuralist theory that he had come to reject. He made this clear at the end of his 1967 paper "Where the Tongue Slips, There Slip I," in which he took three basic mechanisms to be the fundamental elements in the generation of speech: "analogy; blending (=unresolved conflicts of analogies); and editing" (1977, p. 255). His conclusion was that:

> beyond the design implied by the factors and mechanisms that we have discussed, a language has no design. The search for an exact determinate formal system by which a language can be precisely characterized is a wild goose chase, because a language neither is nor reflects any such system. A language is not, as Saussure thought, a system 'où tout se tient.' Rather, the apt phrase is Sapir's 'all grammars leak' (1977, p. 256).

Hockett's last book was the 1987 monograph *Refurbishing Our Foundations*. Its title reflected his intention to work further toward the formulation of a theory of hearing and speaking and redirect linguistic science to reexamine the nature of language in terms of how it operated in, and in fact was created within, the speaking-hearing-understanding situation. The book presented his thoughts and observations on many of what he saw to be the basic properties of language in action, and characteristically, it included not only some new proposals and insights but also proposed some unresolved questions. While many of these were original, unorthodox, and invited examination and challenge, the whole never eventuated into a clear and specific research program that others could take up and follow. For this and other reasons, it never attracted the attention that it could possibly repay, not least by stimulating new thoughts about the nature of its object and raising questions that the science of language would ultimately have to address. As he saw it, this development of a "theory of hearing and speaking" would require operating in a region between linguistics and psychology as both were currently conceived and constrained, that is, by a kind of psycholinguistics, though he did not use that term.

Especially with the appearance of his 1968 *The State of the Art*, Hockett became known as the most vocal and prominent critic of Chomsky and generative grammar, a role in which he was cast, for example, in Mehta (1971) and elsewhere, as in the subheading of his obituary in the *New York Times* describing him as "one who did not buy Chomsky's revolution" (Fox, 2000). Actually, when transformational grammar emerged in both the Harris and Chomsky variants, Hockett, like many other structuralists, welcomed it as an important and innovative development in syntactic theory that held great promise for dealing with problems, such as nonadjacent relations, that had proved intractable in immediate constituent syntactic analysis. In his 1964 presidential address to the Linguistic Society of America, he went so far as to characterize Chomsky's *Syntactic Structures* as "one of only four major breakthroughs in the history of modern linguistics." In his 1964 Linguistic Institute lectures on mathematics and linguistics, he made a number of approving references to Chomsky's work, but by the time the work was published in 1966 he had added a footnote to the proof in the preface that those "conciliatory remarks" were the result of his "misunderstanding," and that "[while not passing] detailed judgment on Chomsky's frame of reference . . . let the record show that I reject that frame of reference in almost every detail" (1966, p. 156).

The footnote itself noted that the turnaround arose from a reading of Chomsky's *Aspects of the Theory of Syntax* (Chomsky, 1965) that had appeared in the interim. *The State of the Art* gives as a major reason for his rejection of the generative framework: that it viewed language as a well-defined system in the computational sense, as opposed to his view, characterized above, that it was ill-defined "though characterized by various stabilities" (1968, p.88). In his own changed perspective he had come by then to incorporate as crucial what he saw as the "true" Bloomfieldian conception that grammar, though not phonology, was inescapably and inextricably entwined with meaning, and unable to be analyzed apart from it.

Another factor that entered here was that *Aspects* made clear Chomsky's rationalist orientation in a way that *Syntactic Structures* had not. As a true Bloomfieldian, Hockett found that mentalistic stance thoroughly unpalatable, considering it to be unscientific by his own canons of science.

There was also a more personal and attitudinal element. In his 1964 address to the Linguistic Society of America, Hockett had included a statement deploring the aggressive confrontational style that marked even some of the earlier transformational work he cited. Despite the heated and not infrequently personal rhetoric that marked the argument on both sides and his own sometimes intemperately expressed personal feelings (1977, p. 61; Mehta, 1971, pp. 217-221), he characteristically strove, as his view of science demanded, to keep an open mind to the possibility that the other view might be right. As late as 1997 he remarked:

> Indeed, Chomsky's paradigm may turn out to afford the best path toward the ultimate solution to our collective scientific problem; namely, the determination of the place of language in the universe. Some people are convinced that that is so, but no one can know for sure. My own impression is quite otherwise (1997, p. 162).

The State of the Art did not, of course, slow the march toward dominance of the generative paradigm in its successive forms, and it would be unfortunate if Hockett were to be remembered primarily as one who fought a futile rearguard action against the dominance of the generative paradigm. To the extent that such is the case it can lead to easy dismissal and failure to take account of and appreciate his original and extensive contributions to the field of linguistics and beyond.

His work in linguistics was by no means limited to synchronic theory and description. Throughout his career he continued to hold to the belief—in common with Bloomfield, Sapir, Saussure, and their nineteenth-century forbears—that the investigation of language change was an integral part of the science, and his output included important historical analyses, notably in Algonquian but in other languages as well, including Central Pacific languages and Old English.

Hockett maintained the view that diachronic investigation had laid the essential foundation for synchronic study, and that the latter had returned the favor. The two enterprises thus informed each other, and any synchronic theory and description had to be at the very least compatible with what we knew and discovered about language change. In his later work the interaction between the diachronic and the synchronic was even more intimate, to the point that the distinction essentially disappeared. In the tradition of historical linguistics in which he worked, the three central mechanisms of change were sound change, analogy, and borrowing—and analogy accompanied by editing—had become the fundamental mechanism in his dynamic theory of language generation.

Hockett's talents, scholarly interests, and productivity extended well beyond linguistics proper. He cast a wide net in his consideration of topics for investigation, including among other things the Whorfian theory, slips of the tongue, scheduling of linguistic and nonlinguistic events, animal communication, jokes, and the nature of writing systems and their relation to speech. He collected a number of his papers on those topics in the 1977 book *The View from Language*. Its title was emblematic of his belief that the study of language constituted a unique locus for gaining insights and knowledge in other fields of inquiry. From the range of the papers included in it one can gather the range of his interests beyond what is generally considered to be linguistics as well as the power of his intellect in pursuing them.

His Ph.D. was in anthropology, and he never ceased to consider linguistics as a component of that field, that is, as "linguistics wrapped in anthropology" and as that branch of science devoted to the discovery of the place of human language in the universe. He believed that, as he remarked in his 1980 autobiographical interview "Preserving the Heritage," "Linguistics without anthropology is sterile; anthropology without linguistics is blind" (p. 100).

Hockett considered his most important work to be his 1973 anthropology text *Man's Place in Nature*. That book, with a bow to Thomas Huxley's work of the same title, raised and addressed very basic and challenging questions related to that enterprise. Hockett brought to bear on them an impressive quantity of scholarship from several fields and numerous original insights. In the chapter on language entitled "Man as Chatterer: the Tongue Is a Fire" he set forth his conviction that the wherewithal for acquiring language is "locked into" the genes, and that its "appearance is as inevitable as menarche or the sprouting of axillary hair," but that it also required "nurture in the bosom of an ongoing social group" so that "neither of these suffices without the other" (p. 101). The investigation of language was crucial because "an understanding of language is . . . essential for any understanding of man's place in nature" (p. 98).

Here and in a later chapter titled "The Emergence of Man: Fire and Talking" he proceeded to present his views on the architecture of language and on productivity and change, differences among languages and universals, and the origin of language. The scope was daring and the issues and challenges remain, but the work remains well worth reading, if for no other reason than to set some more recent works by others in perspective.

One of his most important contributions was his origination and development of the design-feature approach to the comparative study of animal communication, including the human. He began with seven features appearing in the 1958 textbook and in the 1959 paper "Animal 'Languages' and Human Language." Those seven subsequently underwent numerous expansions and revisions, eventuating in 13 features as they appeared in his 1960 paper "Logical Considerations in the Study of Animal Communication" and the popularized version in *Scientific American* entitled "The Origin of Speech." These were part of a wider and more daring effort to determine the origin of human language—a subject of inquiry that was far from popular when he undertook it, and which led to the much reprinted 1964 paper with Robert Ascher on the human revolution.

Hockett's design features have not only found their way into linguistics texts but also have been crucially incorporated into work within the field of animal communication. A Google search of the Web under his name will immediately show by the sheer number of citations how widely they have been called upon in several fields. For Hockett the most important of the features that marked human language was duality of pattern, by which all of the meaningful elements of language were expressed in terms of meaningless elements: in the case of human language, phonology. Though his concept of the architecture of grammar changed radically, his belief in this basic feature of language as crucial remained throughout, though differing in detail.

Hockett did not limit his productions to dry academic presentations. In 1955 he contributed a clear popularized account of structural phonemics field methods under the guise of "How to Learn Martian" to the magazine *Astounding Science Fiction*. Among his conclusions in *Man's Place in Nature* was that "the most important special factor in primate learning and behavior is play" (1973, p. 74), and not only did he recognize its presence in language but also exercised it. His writings, even among the most serious, reflect his valuing of the aesthetic capabilities of language, and the delight that he took in finding and using the right turn of phrase. Metaphors abound in his work, and are used to effect, often serving to make clear a difficult point. His output is also studded with apt quotations and examples from literature ranging from the Bible and Shakespeare through T. S. Eliot, Nero Wolfe, Winnie the Pooh, and Dr. Seuss, and there are numerous oblique allusions as in the title "Where the Tongue Slips, There Slip I" (1967). He included openness, aka creativity, as one of the basic design features of human language, but he also delighted in the actual creative use of language and the witty turns that it made possible—in short, in having fun with it.

In a somewhat less serious vein, he indulged in such conceits as giving birth (in his head, as was the case with Athena) to one Casimir Cauchemar, adjunct professor of Etruscan rhetoric of the University of Psonch. Hockett's alter ego, Cauchemar, then presented him with a paper "Innovation and Creativity," as a tattered offprint from *The Harvard Journal of Teleology and Cornucopia*, which the recipient duly edited and published in *The View from Language* (1977)—a tongue-in-cheek effort that afforded him the delicious opportunity to comment on himself. Cauchemar also included among his publications a 1968 volume of verse, *Rugged Nuggets*, which included several poems from "The Red Boat," a version of the Rubaiyat in the form of limericks, which Hockett had earlier distributed to friends. The Cauchemar volume bore an introduction by one Charles F. Hockett and a dedication to several poets that he knew "in the hope that they will never lose sight of the humor intrinsic in all seriousity," which reveals much about the editor/ author himself.

Hockett contributed eight poems to the volume *The Linguistic Muse* (Napoli and Rando, 1979), and composed numerous others, especially lyrics for his own musical compositions.

Hockett is survived by a loving family. He had a long and happy marriage to the former Shirley Orlinoff, whom he wed while on furlough in 1942. She became a professor of mathematics at Ithaca College and the author of a half-dozen textbooks, typed by him, a collaboration that reinforced his own considerable capability in mathematics. They had five children: four girls (Alpha Hockett Walker, Amy Robin Rose, Rachel Hockett Youngman, and Carey Beth Hockett) and a son (Asher Orlinoff Hockett), as well as five grandchildren.

Music played a vital part in his life. He possessed a deep love for music and a keen ear, and he engaged in a lifelong practice of musical performance and composition. His compositions ranged from the witty and light to the serious and sophisticated, and from short pieces through chamber works, to a full-length opera, *The Love of Doña Rosita*, based on a play by F. García Lorca, *Los Títeres de Cachiporra*, which received its premier performance by the Ithaca Opera at Ithaca College.

Music was also a vital center of his home life. He and his wife, Shirley, were early members of the Ithaca Concert

Band, which closed every concert with "Stars and Stripes Forever," featuring Hockett on the piccolo, and the group often played his Ithaca-inspired composition "The Small Plum" (contra "The Big Apple"). Everyone in the family played an instrument, and they regularly conducted home musical performances, often of his compositions. Two of his children became professional musicians and a son-in-law is principal oboist with the Los Angeles Philharmonic. Throughout the last decades, as Hockett turned his efforts increasingly to music, he and Shirley were unstinting in their organizational efforts and financial support and indefatigable in the energy they devoted to bringing music to the Ithaca public. Their leadership and hard work were a vital part in establishing the Cayuga Chamber Orchestra, which after more than a quarter of a century continues to enrich the musical life of the Ithaca community. The effects of their dedication and generosity are lasting and tangible in the Charles F. Hockett Music Scholarship, the Shirley and Chas Hockett Chamber Music Concert Series, and the Hockett Family Recital Hall at Ithaca College. Fittingly, Hockett's memorial service was in great measure a concert at that institution that included several of his own compositions, some of them played by members of his family.

Roman Jakobsen was once quoted as saying, "It is very difficult for me to know what Hockett's position on any question is. . . He changes his mind every day" (Mehta 1971, p. 235). There is a kernel of truth in this, since throughout his career he changed his theoretical views and was not hesitant to reject positions that he himself had espoused, developed, and argued for. However, one can consider that as more of a virtue than a vice, as the inevitable result was an active, questioning, and restless mind that was incapable of accepting any theory as immutable and necessarily true when faced with evidence to the contrary. It was also the product of a questing temperament that was given to ranging into areas whose inclusion in or even relation to the field or subfield at hand were not immediately obvious. At times these qualities resulted in his apparently espousing two views at the same time in an overlapping fashion, with the demise of one preceded by the sprouting seeds of the other.

Hockett was in essence a "God's truth" linguist in Householder's terminology (Householder, 1952, p. 260), dedicated to discovering the nature of human language and its place in humanity and the universe, and willing to pursue any clues toward that end. However he changed his views, not least on his own work, he never wavered from his Bloomfieldian commitment to the idea that the only valid generalizations about language were empirical generalizations, and from a conviction that whatever hypotheses or theoretical leaps one might make it was an absolute requirement to be responsible to the observable data. In short, "Linguistics is either an empirical science or it is nonsense" (Chevillet, 1996, p. 183).

Coupled with that commitment to empirical science, however, was his love for language and his marvelous capacity for intuition into its structure, such that a colleague once characterized him by saying that in his Bloomfieldianism there was always a Sapir struggling to get out. That remark is insightful and essentially true, but without our over-psychologizing (which he would have abhorred), it was clearly more complex than that. To some of us who knew and worked with him, what appeared to be at work was an intersecting play of a first-rate intelligence, a lively intuition, and a conscious commitment to rigor and precision, not infrequently challenged by an honest inability to exclude interesting observations or ideas, even when they did not support the analysis he was pursuing. This could lead to a kind of internal tension that one could sense in much of his work, and even at times in personal interactions with him; one of its effects was that his work was often more interesting and sometimes more prophetic than that of many colleagues.

One may charge Hockett with being subject to change of mind but never with being intellectually dishonest, unoriginal, or uninteresting. This carried over into his classes. When attending his lectures, one always got the feeling that there was a first-class mind directly engaging some problem as new and compelling. He would not infrequently pursue some line of investigation, and then reject, sometimes abruptly, the analysis that he had been developing as proving inadequate or not properly accounting for the facts. This could be disconcerting to those students who wanted to fill their notebooks with accepted truth, but to others it was exciting as the model of how a scientific investigator proceeds and of the difficulty of arriving at whatever truth there existed to be found.

In 1993 he captured the fundamental approach that had remained constant throughout his long career. Fittingly, it was included in a paper on Algonquian, and invoked Bloomfield:

> Time and again, what at first appears to be a knotty problem of linguistic analysis smooths [sic] out if, approaching a language with patience and reverence, we relax and let it show us how it works—instead of trying to force matters into some conceptual frame of reference we have imported, perhaps without realizing it, from elsewhere, This is how Bloomfield dealt with the languages he studied" (1993, p. 4).

Hockett always had a sense of the science of linguistics as an ever-developing and social enterprise with a historical trajectory that demanded an attitude and behavior that he held up as a model for himself as well as others. In his presidential address to the Linguistic Society of America (1965, p. 204) he said:

> The scholar earns immortality only of the sort that he bestows on those that have gone before him. As we extend the power and flexibility of our new tools, let us always temper passion with humor; let us never favor, nor disfavor, the new simply because of its novelty; let us dedicate our talents to building our heritage, not to tearing it down, praising our predecessors for their wisdom and ignoring their folly—replacing a nail here or a plank there when we must, but always with humility rather than Schadenfreude when a bright old idea must give way to a bright new one.

For those of us who were fortunate enough to have learned from him, there can be no better model and remembrance.

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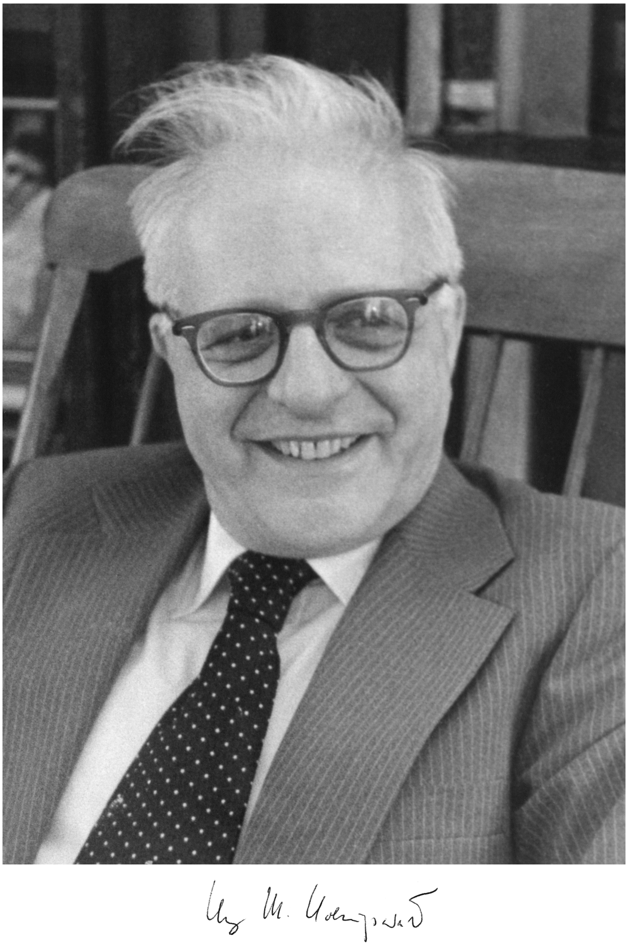

HENRY M. HOENIGSWALD

April 17, 1915–June 16, 2003

BY GEORGE CARDONA

HENRY M. HOENIGSWALD, professor emeritus of linguistics at the University of Pennsylvania, died on June 16, 2003, in Haverford, Pennsylvania. Henry began his career as a classicist. He contributed articles on Etruscan and Latin and important studies in Greek phonology, morphology, and metrics, the last of which he completed just before his death. He was, in addition, a well-versed Indo-Europeanist and contributed to Indo-Iranian linguistics; further, during the Second World War, he was engaged in modern Indo-Aryan and produced a handbook of Hindustani. Henry's greatest contributions to linguistics, however, are of a more general theoretical nature. He was a major figure in seeking to understand and clarify the principles that underlie great work in historical-comparative linguistics, especially as practiced by the nineteenth-century neogrammarians and their successors. Henry contributed fundamental studies in these areas, including an early article on sound change and its relation to linguistic structure, a basic study of the procedures followed in phonological reconstruction, an equally fundamental study of internal reconstruction, and a definitive monograph on language change and linguistic reconstruction.

Henry—named Heinrich Max Franz Hönigswald at birth—was born on April 17, 1915, in Breslau, Germany (now Wrocław, Poland), into an academic family, the son of Richard Hönigswald, an eminent professor of philosophy at the University of Breslau. Henry received a traditional education at the Johannes-Gymnasium in Breslau and, after his father moved to the University of Munich, at the Humanistische Gymnasium in Munich, which he entered in May 1930 and from which he graduated with honor and distinction in the spring of 1932. He went on to study at the University of Munich, where from 1932 to 1933 he pursued studies in the Department of Humanities, working with such scholars as Eva Fiesel, an authority on Etruscan, and the renowned Indo-Europeanist Ferdinand Sommer. The latter was also a friend of the Hönigswald family.

Henry became interested in the classics, Indo-European, and linguistics at an early age. Years later he reminisced (1980, p. 23) about how his interest in these areas was first aroused:

> My story is very different. I suppose I was a fairly typical product of German secondary education. We had a Greek teacher who must have had a course in Indo-European and who taught us some of the things he knew. I bought Kiecker's Historical Greek Grammar ("Sammlung Göschen"[1]) and one birthday I got Brugmann's *Kurze vergleichende Grammatik*. Since then I knew I wanted to be a classicist or, even better, a linguist.

As was true for many scholars of that time, Henry's family was subjected to the dictates of German Nazism. His father was nominally a convert to Christianity (his mother died when Henry was only six)—and Henry was confirmed in the evangelical church, but these were formalities to ensure tenure at a university and a place in civil society for an intellectual family that was ancestrally Jewish—though they rejected all religion and superstition—in a place in Germany (Silesia) that was quite intolerant of Jews. For this ancestry they paid a price. In 1930 Richard Hönigswald shifted from Breslau to the University of Munich, but by 1933, Jews were forbidden to attend German universities, so that Henry then began a period of scholarly wandering. He went first to Switzerland, where from 1933 to 1934 he studied in Zurich with the classicist and Indo-Europeanist Manu Leumann and was a fellow student of the Hellenist Ernst Risch, with whom he maintained a lifelong friendship. In the fall of 1934 Henry moved to Italy, where he continued his studies at the University of Padua and in the summer of 1935 passed an intermediate examination, again with honor and distinction. He proceeded to work on a doctoral thesis on Greek word formation while completing a preparatory paper on the relationship between Sanskrit and Avestan. When his mentor, Giacomo Devoto, moved from Padua to Florence, Henry followed and received his doctorate (D.Litt. summa cum laude) in 1936 from the University of Florence, with a dissertation on the history of Greek word formation (*Geschichte der griechischen Wortbildung*), a work that to my knowledge has never been published. He went on to receive the *perfezionamento*, a research degree, from the same university in 1937. From 1936 to 1938 he held his first academic appointment, as a staff member in the Istituto di Studi Etruschi, Florence.

Politics then intervened once more. Foreigners who had come to Italy after 1918 were obliged to leave the country, so that Henry could not remain in Florence for the winter semester of 1938-1939; he moved back to his family in Munich. On March 26, 1939, Henry left Bavaria for Switzerland in the company of his father, stepmother, and sister, taking refuge in Braunwald in Glarus in preparation for going to the United States. However, Henry was not included in the family permit for departure and had to remain behind. On

September 22 he finally obtained passage on a ship from Genoa and arrived in New York in October 1939. These early experiences left deep impressions, and in later years both Henry and his wife were devoted to the cause of human and civil rights and were active members of local and national organizations supporting these rights.

In the United States Henry at first continued a life of scholarly peregrination. Between 1939 and 1948 he held positions as research assistant, lecturer, and instructor at Yale University—where he was research assistant to Edgar Sturtevant—the Hartford Seminary, Hunter College, and the University of Pennsylvania, then associate professor at the University of Texas at Austin (1947-1948), in addition to a one-year stint (1946-1947) in the Foreign Service Institute of the U.S. Department of State. During this time, in 1944, Henry married Gabriele ("Gabi") Schöpflich, herself an accomplished classicist, whom he had met years earlier while they were both students in Munich. Gabi died in 2001.

In 1948 Henry joined the University of Pennsylvania, succeeding Roland Grubb Kent. Promoted to the rank of full professor in 1959, he made Penn his academic home for the remainder of his career, though he was invited to and visited several other universities in the United States (University of Michigan, Georgetown—where he held the Collitz Professorship in the Linguistic Institute in 1955—Princeton, Yale), in Europe (Katholieke Universiteit Leuven; St. John's College, Oxford; University of Kiel); and in India (Deccan College, Poona [now Pune]). During his long and distinguished tenure at Penn, Henry was the major force in strengthening the linguistics department, founded by Zellig S. Harris, which he served as chair from 1963 to 1970 and cochair from 1978 to 1979; he remained a Nestor for the department long after.

In America Henry interacted with many major scholars who had a strong influence on his thinking and work. He also encountered "innumerable new things to learn" (1980, p. 25), such as articulatory phonetics, phonemics, and the anthropological approach to linguistics. Of paramount importance for a young scholar coming from his background, there was the feeling of freedom and exposure to new vistas accompanying this. Henry put it well when he said:

> In 1939—half a year after Sapir's death—I found myself at Yale as Sturtevant's research assistant. Quite aside from the inextricable connection (for me) with my escape to personal freedom, I wish I could convey the headiness of the experience—no amount of picture painting of my Old-World inter-war background as I have attempted it can describe it.

Henry received his share of deserved honors. He was elected to the American Philosophical Society in 1971, the American Academy of Arts and Sciences in 1974, and the National Academy of Sciences in 1988. He was elected a corresponding fellow of the British Academy in 1986, and was a fellow of the Center for Advanced Study in the Behavioral Sciences (1962-1963) as well as a Guggenheim fellow in 1950. He was also elected president of the Linguistic Society of America in 1958 and the American Oriental Society in 1966. In addition, Henry received the Henry Allen Moe Prize of the American Philosophical Society in 1991. He also received honorary degrees from the University of Pennsylvania (L.H.D. in 1988) and Swarthmore College (L.H.D. in 1981). Upon his retirement in 1985 Henry was honored by colleagues and friends with a felicitation volume, published two years later (Cardona and Zide, 1987) and in 1986 the American Oriental Society dedicated a number of its journal to Henry.

Several threads are discernible in Henry's work, and he felt the need to express himself (1980, p. 27) on how he would "like to think that the various different tasks which I have tackled over the years and which keep me busy now, somehow hang together, however much each one of them may have depended on inevitable accident." To begin with, there is the philology; various papers dealing with topics in Etruscan, Greek, and Latin, as well as a smaller number of articles treating issues in Indo-Iranian and Sanskrit. To a very large extent, however, what motivates these studies is an underlying quest for generalization: methods and principles governing how languages change over time and how one goes about reconstructing an ancestral protolanguage. The need to find these principles and to make explicit the methods followed in historical and comparative linguistics occupied him throughout his career. Henry mentioned (1980, p. 24) his early preoccupation with such issues, including his wish that comparative evidence be presented "upward in time as inference, and not downward as history."[2] The close attention to principles and methods also led Henry to be involved closely with the history of the field to which he contributed. He was particularly careful to distinguish between the concrete work that such giants of nineteenth-century Indo-European linguistics as Karl Brugmann and Jacob Wackernagel carried out and the theoretical "preachments," as he occasionally called them,[3] of August Leskien, Brugmann, and others. This attention to methods and the history of his field complemented Henry's interest, in his later years, in the related area of cladistics (Hoenigswald and Wiener, 1987).

In view of Henry's constant preoccupation throughout his professional life with methodology and procedures for reconstruction—he went so far as to speak on occasion of algorighms[4]—*Language Change and Linguistic Reconstruction* may justifiably be considered his major work. This monograph is certainly the principal recapitulation of thinking that went back to his very early years, results of which Henry published in a series of articles (1944, 1946, 1950), the earliest of which appeared when he was not yet 30 years old. In accordance with the only procedure he thought proper—namely, presenting historical materials "upward in time as inference, and not downward as history" for purposes of reconstruction—Henry did not follow here the custom observed in the usual textbooks on the subject. It is noteworthy, for example, that he did not begin with any discussion about the regularity of sound change[5] or use Proto-Indo-European constructs and Grimm's and Verner's laws as illustration; moreover, the great majority of examples used to illustrate procedures and principles are from such well-attested languages as English, Latin, and Romance languages.[6] He also diverged from the usual practice by dealing first with morphological change and only later with sound change. It is only after treating grammar and semantics, ending with a chapter (7, pp. 68-71) on the reconstruction of grammatical and semantic features, that he proceeds to treat sound change and the comparative method with respect to phonology and its reconstruction.

In all this, Henry was rigorously formal and, it is important to emphasize, treated changes in terms of distribution, saying, for example (1960, p. 15), "Note that these four classes are defined entirely by their distribution of the segments *A* and *B*—and they may or may not have other distinguishing characteristics." While dealing with the distribution of elements, both phonological and morphological, he made use also of what he called "nil" and symbolized Ø.[7] Further, nil could be a primitive, not merely an absence due to loss. Thus, for example, while illustrating unconditioned sound loss with the example of early Latin *hortus* (garden), which in later Latin has no *h*-, Henry operates not only with the change $h > Ø$ but also with a change $Ø > Ø$, as in *ortus* (risen), which lacked any initial consonant in both early and late Latin. He notes in this context, "in fact, any conveniently assumed number of Ø's may be posited as occurring between any two segmental phonemes found in sequence. Thus, the environment of Ø in English includes *t—i,# —t*, but not *#—ŋ*." This emphasis on distribution went beyond phonology and morphology to include semantic change. Accordingly, Henry notes (1960, p. 45),

> The phrase "semantic change" or "change of meaning" is properly applied to morphs; if a morph at a later stage appears otherwise than as a part of a corresponding morpheme—if, in other words, it has changed its morphemic environment—it is quite rightly said to have changed its meaning. Thus *avunculus, cēace-cheek, flesh, meat,* taken as morphs (i.e., identified phonemically) have all undergone semantic change.

Earlier in the same work (1960, p. 29) the approach in question is made more explicit in a section entitled "One-to-One Replacement by Existing Morphs (Semantic Change)," in which are charted possible environments (I, II, III, IV) for old English *wonge* (cheek) and *cēace* (jaw) and their modern English counterparts, respectively *cheek* and *jaw.* This formal approach could appear deceptively simple, as when Henry dealt with what he called the principal step in comparative grammar in a remarkably short compass (1950).[8]

Henry's consistent probing into the methods and principles underlying concrete work in historical linguistics was also colored by a healthy skepticism. It is typical, for example, that the title of his contribution to a volume on universals (Hoenigswald, 1966) is a question, that he does not simply assume there are given universals merely to be exemplified. It is also typical of Henry's nature that he ends this essay with a view to the future, noting that transformational grammar "may also bring new principles of importance to an understanding of the universals of change."

Henry's healthy skepticism combined well with his background as a philologist and his search for principles and methods to produce insightful work on the history of linguistics. A citation from his paper on the history of the comparative method (Hoenigswald, 1966, p. 1) will serve to illustrate:

> Existing self-description, being itself a phenomenon in the history of scholarship, must not necessarily be taken at face value. On the other hand, the business of gleaning procedures, principles, and presuppositions from an analysis of the record is a slow process which has been engaged in for some areas but not for others. Yet it alone can yield the substance in which we are interested.

Among the "few strands" he had to offer in what he called "this rich tissue," one brings neatly to the fore Henry's attitude and insight: the interpretation of the famous statement made in 1786 by Sir William Jones, with which he dealt on more than one occasion (e.g., 1963, pp. 2-3; 1974, p. 349). Jones's words, which Henry cited almost in full (Hoenigswald, 1963, p. 2), are:

> The *Sanscrit* language, whatever be its antiquity, is of a wonderful structure; more perfect than the *Greek*, more copious than the *Latin*, and more exquisitely refined than either; yet bearing to both of them a stronger affinity, both in roots of verbs, and in the forms of grammar, than could have been produced by accident; so strong, indeed, that no philologer could examine them all three without believing them to have sprung from some common source which, perhaps, no longer exists. There is a similar reason, though not quite so forcible, for supposing that both the *Gothick* and the *Celtick*, though blended with a very different idiom, had the same origin with the *Sanscrit*; and the old *Persian* may be added to the same family, if this were the place for discussing the antiquities of *Persia*.

Henry's careful reading of Jones's proclamation, taking it in the context of its time, rules out any possibility that Jones had in mind a protolanguage as reconstructed through modern methods or a procedure for recovering such a source.

Contrasting the procedure followed in comparative linguistics with what Jones said and alluding to the possibility of wrongly reading such a procedure into this statement, Henry remarks (1963, p. 3), "We are asked to imagine that Jones had in some intuitive fashion subjected Greek, Latin, and Sanskrit to a similar process, and had been forced to conclude (as indeed we would now be forced to conclude) that the ancestor was unlike each of the three. But this cannot be right." He then goes on to demonstrate how this reading of Jones's statement could not be correct.

Henry was keenly aware of the intellectual legacies to which he was heir. Forty years after leaving Europe, he would say in recollection (Hoenigswald, 1980, p. 24):

> About the substantive work I learned from such masters as Sommer, Fiesel, Leumann, and Devoto and from fellow students like Ernst Risch. From Leumann, in particular, I learned more, namely that there are formalisms in historical linguistics which have little to do with sound laws, and that you can discuss them observing and analyzing the masterpieces of the Brugmanns and the Wackernagels, and, in general, the riches of the scholarly record. Leumann's paper on the mechanics (note the word!) of semantic change[9] seems to me to be one of the greatest methodological gems, for all its hardnosed factualness. My own first publications were case histories having to do with the "mechanics" of the word-formation.

It is evident that in Manu Leumann, who also was a Homeric scholar and a Latinist, Henry met not only a mentor at a time of need but also a kindred spirit. For Henry's work and reminiscences of his early school days show a brilliant intellect given to detailed investigations of problems whose solutions are amenable to formalism. It is just as evident that Henry later met with an equally sympathetic and brilliant spirit, Zellig S. Harris, with whom he had a long and close relation, personal as well as intellectual. The emphasis on distribution that permeates Henry's work is to be seen also in the theoretical linguistic work Harris carried out in the last century from the 1940s to the early 1990s.[10] Henry and Harris met regularly during the sixties, seventies, and eighties to discuss problems of common interest. It must not be forgotten that Harris began his career as a Semitist, so that Henry's investigations into historical and comparative linguistics could meet with a sympathetic and comprehending mind in these discussions.

Henry's association with Zellig Harris was but one in an extensive network of friends and colleagues. At the time he came to Penn, the linguistics department was in its infancy, with Harris and Leigh Lisker, a phonetician, as colleagues, later to be joined by the logician Henry Hiż. When I joined the department in 1965—after five years in the Department of South Asian Regional Studies—it was a very small close-knit group of scholars who not only regularly met to exchange ideas but frequently also attended one another's seminars. Henry was central to this group. He also showed extraordinary warmth and lack of pretense. I have personal memories of joint seminars we gave in which we both could freely exchange opposing views in search of better solutions to problems of common interest, though I was more than 20 years his junior. During those years, although our linguistics department was itself quite small, the University of Pennsylvania could boast of an outstandingly broad and distinguished array of programs in various allied areas, including the classics, Indic, Iranian, Baltic, Slavic, Germanic, Semitics, and Sumerology. Moreover, through the organization of teaching units known as graduate groups, members of the linguistics department regularly taught in other departments, so that there was an exhilarating interaction of colleagues and students, who could take courses across departmental borders. In this atmosphere, Henry thrived and, with a superb talent for social as well as intellectual intercourse, he maintained and promoted the study of linguistics, strengthening the department with his service as chairman over many years. These activities extended well beyond the confines of Penn, and over generations Henry was a prime defender and promoter of the fields he cultivated, in universities both here and abroad as well as in learned societies.

Henry was also extremely generous toward young scholars worthy of support, a generosity that was rewarded with feelings of intellectual admiration and personal warmth toward him on the part of an array of many scholars who went on to excel. In this spirit it is fitting, I think, that I end this essay citing the whole of a Sanskrit couplet whose last part was used to end the foreword to Henry's Festschrift:

vidvadvattvañ ca nṛpatvañ ca naiva tulyaṁkadā cana |
svadeśe pūjyate rājā vidvān sarvatra pūjyate ||[11]

I AM GRATEFUL TO Henry's sister Trudy Glucksberg, his daughters Ann and Frances Hoenigswald, as well as Roswitha Grassl and Prof. Anna Morpurgo Davies for details of Henry's early life.

A fairly complete bibliography of Henry's work through 1985 appeared in his Festschrift (pp. xiii-xix); a more up-to-date bibliography covering publications up to 1999 compiled by C. Justus with Henry's cooperation is available at http://www.utexas.edu.cola/depts/lrc/iedocctr/ie-pubs/hmh.

CHRONOLOGY

1918	Born April 17 in Breslau, Germany (now Wrocław, Poland)
1932-1933	Studied in the Department of Humanities, University of Munich
1933-1934	Studied at the University of Zurich, Switzerland
1934-1935	Studied at the University of Padua, Italy
1935-1936	Studied at the University of Florence, Italy
1939	Emigrated to the United States
1944	Married to Gabrielle L. Schöpflich
1945	Naturalized citizen of the United States
1954-1958	Editor, *Journal of the American Oriental Society*
1968-2003	Advisory board, *Language and Style*
1968-1992	Member, editorial board, *International Encyclopedia of Linguistics*
1968-1974	Member, corporate visiting committee for the Department of Foreign Literatures and Languages, Massachusetts Institute of Technology
1977-2003	Associate editor, *Indian Journal of Linguistics*
1978-2003	Consulting editor, *Journal of the History of Ideas*, *Journal of Indo-European Studies*
1978-1984	Chairman, overseers committee to visit the Department of Linguistics, Harvard University
1984-2003	Advisory board, *Diachronica*
1985	Retirement dinner, University of Pennsylvania, at which he was presented with a prepublication copy of *Festschrift for Henry M. Hoenigswald*
1985-2003	Consultant, *Biographical Dictionary of Western Linguistics*
1986	Member, comparative linguistics delegation, IREX
1987	Member, organizing committee, Colloque Meillet
2003	Died, Haverford, Pennsylvania

AWARDS AND HONORS

1942-1943	Fellow, American Council of Learned Societies
1950	Guggenheim fellow
1956	Newberry Library fellow
1958	President, Linguistic Society of America
1962-1963	Fellow, Center for Advanced Study in the Behavioral Sciences, Palo Alto, California (with fellowship from the National Science Foundation)
1966-1967	President, American Oriental Society
1971	Elected to the American Philosophical Society
1974	Elected to the American Academy of Arts and Sciences
1981	Awarded L.H.D. honoris causa, Swarthmore College
1986	Elected corresponding fellow, British Academy
1988	Elected to the National Academy of Sciences; awarded L.H.D. honoris causa, University of Pennsylvania
1991	Awarded the Henry Allen Moe Prize by the American Philosophical Society

PROFESSIONAL RECORD

1936	D.Litt., University of Florence
1937	Perfezionamento, University of Florence
1936-1938	Staff member, Istituto di Studi Estruschi, Florence
1939-1942	Lecturer, research assistant, Yale University
1942-1943	Lecturer, Hartford Seminary Foundation; Hunter College
1943-1944	Lecturer in charge, Army specialized training, University of Pennsylvania
1944-1945	Lecturer, Yale University
1945-1946	Instructor, Hartford Seminary Foundation
1946	Lecturer, Hunter College; visiting associate professor, University of Michigan (Summer Institute of Linguistics)
1946-1947	P-4, Foreign Service Institute, Department of State
1947-1948	Associate professor, University of Texas at Austin
1948-1959	Associate professor, University of Pennsylvania
1952	Visiting associate professor, University of Michigan (Summer Institute of Linguistics)

1955	Senior linguist, Deccan College Postgraduate Research Institute, Poona; visiting associate professor, Georgetown University (Collitz Professor, Summer Institute of Linguistics)
1959-1985	Professor, University of Pennsylvania
1959	Visiting associate professor, University of Michigan (Summer Institute of Linguistics)
1959-1960	Visiting associate professor, Princeton University
1961-1962	Visiting professor, Yale University
1963-1970	Chairman, Department of Linguistics, University of Pennsylvania
1968	Fulbright lecturer, University of Kiel, Germany; visiting professor, University of Michigan (Summer Institute of Linguistics)
1976-1977	Fellow, St. John's College, Oxford, and Fulbright lecturer, Oxford University
1978-1979	Cochairman, Department of Linguistics, University of Pennsylvania
1985-2003	Professor emeritus, University of Pennsylvania
1986	Visiting staff member, Katholieke Universiteit, Leuven, Belgium
1991	James Poultney Lecturer, Johns Hopkins University

MEMBERSHIPS

American Academy of Arts and Sciences
American Association for the Advancement of Science
American Oriental Society
American Philological Association
American Philosophical Society
Archaeological Institute of America
Friends and Alumni of Indo-European Studies, UCLA
Henry Sweet Society
Indogermanische Gesellschaft
International Society for Historical Linguistics
International Society of Friends of Wrocław University
Linguistic Society of America
Linguistic Society of India
Linguistics Association of Great Britain
National Academy of Sciences
New York Academy of Sciences
North American Association for the History of the Language Sciences
Società di Linguistica Italiana
Societas Linguistica Europaea
Studienkreis Geschichte der Sprachwissenschaft

NOTES

1. Henry is referring to E. Kieckers, *Historische griechische Grammatik*, Berlin: de Gruyter, 1925-1926.

2. In a typically self-deprecating manner, he went on immediately to add, "Not exactly original thoughts."

3. For example, "Here we are once more up against the gap between substantive practice and theoretical preachment" (Hoenigswald, 1978, p. 28) and earlier in the same paper (p. 21), "There were those who had no stomach for general talk and who preferred practicing to preaching."

4. For example, "In any event Rask is no closer than Jones to the idea of an algorithm for reconstruction" (Hoenigswald, 1974, p. 351).

5. In fact, Henry considered the regularity principle a defini-

tional matter and said, for example (Hoenigswald, 1978, p. 25), "But, one may ask, just what is a 'sound change' apart from its regularity?"

6. There are, of course, places where he could not avoid doing otherwise. For example, in dealing with differentiation (contrast developing from allomorphs) as well as what he termed "phonemic affinity in replacement partners from dialect borrowing," he found it necessary (Hoenigswald, 1960, pp. 39-40, 51-52) to use as an example the reconstructed Proto-Indo-European **leuk*^{w} and its reflexes in Indo-Iranian and Sanskrit.

7. As opposed to zero, which is (1960, p. 35, n. 8) an allomorph. In slightly different terms, "zero" denotes the absence of a morph in a context where a morph is expected (e.g., "fish" used as a plural is formally comparable to "dishes," with an overt plural marker, so that it can be said to have a zero allomorph of a plural morpheme or to have zero as a replacement for a plural marker. On nil, see also Hoenigswald (1959).

8. Henry's mode of presentation was always very concise, without verbosity or excessive use of examples, depending instead on formalism. This is evident in both *Language Change and Linguistic Reconstruction*—the text of which covers only 168 pages, including the bibliography and index—and, even to a larger extent, in his later collection of three articles (1973).

9. Leumann (1927).

10. For a perceptive appreciation of the contrast between Harris's distributionalist view and what Goldsmith refers to as the mediationalist view that has dominated theoretical work in American linguistics since the late 1950s see (Goldsmith [2005, pp. 719-724]).

11. Freely translated: Being a king can never be compared to being a learned man; a king is honored in his own country, a learned man is honored everywhere.

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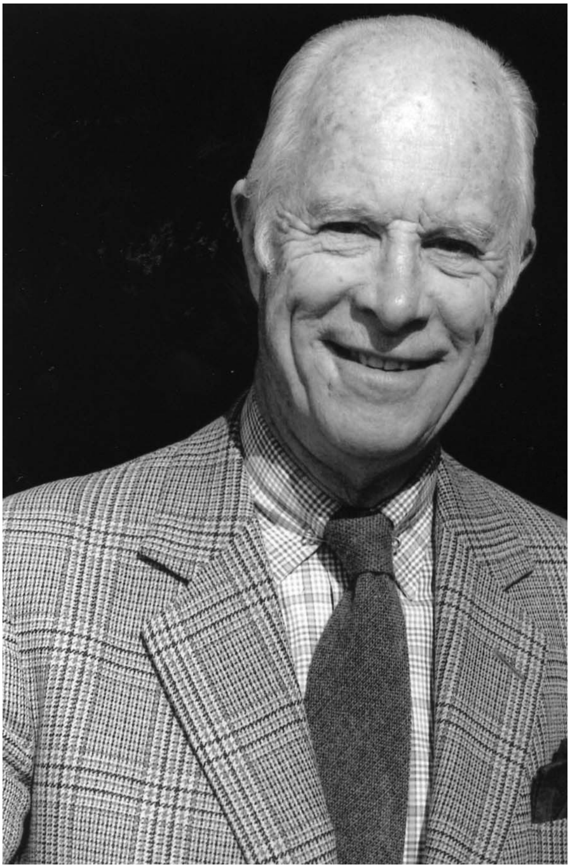

W. W. Howells

WILLIAM WHITE HOWELLS

November 27, 1908–December 20, 2005

BY JONATHAN FRIEDLAENDER
WITH CONTRIBUTIONS FROM
DAVID PILBEAM, DANIEL HRDY, EUGENE GILES,
AND ROGER GREEN

WILLIAM WHITE HOWELLS, ONE OF the most distinguished American anthropologists of the second half of the 20th century, and perhaps the most charming and elegant, died in Kittery Point, Maine, on December 20, 2005, at age 97. He brought anthropology to a wide audience through his general books and played a major role in transforming physical anthropology into a population-based biological science. From this perspective he helped free physical anthropology from its earlier preoccupation with typological classifications of human races. His work was marked by sophistication in multivariate statistics, a great breadth of knowledge in all subfields of anthropology, and a lucid and direct literary style that engaged the reader in what appeared to be an informal conversation.

Bill (to his friends) was born November 27, 1908, in New York City. He came from a family of prominent intellectuals. His father, John Mead Howells, was a successful architect, and his paternal grandfather was William Dean Howells, the distinguished 19th-century American novelist and man of letters. A brief anecdote: As a young baby, Bill was taken by his mother to visit his grandfather, who was being visited by his close friend Samuel Clemens. On being told by Bill's mother, "You *must* see little Billy," Clemens is said to have retorted, "Why *must* I?" This evidently was enough for Mrs. Howells; she had a distaste for Clemens forever after. In any case her son had what was once attributed to his grandfather, "the friendly eye," through which he saw life.

Bill's maternal grandfather, Horace White, was a journalist from an abolitionist background; he traveled with Lincoln during the Lincoln-Douglas debates and subsequently became an editor and co-owner first of the *Chicago Tribune* and later of the *New York Post*. Bill was very close to his aunt Amelia Elizabeth White, who after serving as a nurse in the First World War, moved in the 1920s to Santa Fe, where she became a passionate advocate for the Pueblo, promoting their public health and land rights and establishing a museum of Native American arts. She and her unique estate, El Delirio, were the center of a circle of writers, musicians, artists, and anthropologists and she became a major supporter of the School of American Research. At Bill's urging she left El Delirio and the museum (now the Indian Arts Center) to the School, rather than to him (for more on his aunt and their relationship, see Stark and Rayne, 1998).

As a boy, Bill was taken with cavemen and dinosaurs. He lived in New York and Kittery Point until going to boarding school first in Aiken, South Carolina, and then at St. Paul's School in Concord, New Hampshire. From there he entered Harvard, where he planned to major in English. However, after a look at the English Department's overly long recommended summer reading list, he decided to major in anthropology on something of a last minute impulse. He subsequently became enchanted with the appeal, both intellectual and esthetic, of anthropology's great breadth; he later wrote that he regretted the growing gulf between biological and cultural anthropology in recent decades, "a depressing fact" as he put it (1992).

The Harvard Anthropology Department in the 1930s consisted of Roland B. Dixon, Alfred M. Tozzer, and Earnest A. Hooton, none of whom limited themselves to a particular subdiscipline; for example, Hooton, the physical anthropologist, taught a course in African anthropology. Howells relished his time at Harvard. In his memoir (1992) he remembered Alfred Tozzer's personality in words that fit Bill equally well. "It is easy and pleasant to remember his face in action and the sound of his voice—the things that live on in the memory of one more generation after you die, before they are gone forever."

Howells hurried to finish his undergraduate requirements in three years so that he could marry his sweetheart, Muriel Gurdon Seabury (her mother would not permit the marriage until he graduated). He continued on with graduate study and received his doctorate under Hooton's direction in 1934, at age 25.

If Howells gained intellectual breadth from his teachers at Harvard, he was not too awed by them to recognize their feet of clay. Dixon's book, *The Racial History of Man* (1923), was devastatingly critiqued by Franz Boas (the best mathematical mind in American anthropology at the time). Dixon came to refer to it as "my crime." It was a typological reconstruction of human history, based on three simple ratios of cranial, nose, and face measurements. Hooton's work suffered from a similar typological perspective. He tried to identify distinct elements of racial mixing within skeletal populations, diagnostic traits within series of head shapes of criminals, and reified types in body composition. Hooton was pilloried by statisticians for his poor sense of sampling and for not understanding how to construct a statistical test of an hypothesis. While Howells's early work in Irish and Melanesian crania followed Hooton's typological scheme, he realized early on that the variation in these cranial series was best described by a series of normal distributions. There were simply no discrete subsets or types to find. As he said in his typically self-effacing way, "I was dubious about dissecting populations in this way, having some sense of normal variation. I take no credit for this; it seemed to be a limitation that seemed to enforce itself" (1992). This sense of normal population variation came to be the core of his perspective on human biology in subsequent years.

He took up his first post (as volunteer assistant) at the American Museum of Natural History back in New York, with fellow Hooton product Harry Shapiro. As Shapiro wrote:

> It became quickly evident to me that Bill had a sharp critical sense that got to the core of a particular problem. . . In his quiet way, he could be very firm in his convictions and not easily shifted. But this determination never led to acrimony. Often, he could turn a discussion that threatened to become a bit tense into quieter channels by his delightful humor" (Shapiro, 1976).

Part of the museum's appeal for Howells was its immense collection of 12,000 crania and particularly the recently acquired Von Luschan collection from Melanesia. Howells was looking for a large cranial sample that would provide statistical reliability and at the same time represent a single locale or population. The Tolai sample from East New Britain fulfilled his requirements and became the subject of his first population study. It was also during this period that he met and collaborated with Harold Hotelling (Howells and Hotelling, 1936), a brilliant young statistician who had just returned to New York from Great Britain, where he had studied with Ronald A. Fisher. While their paper still dealt with simple ratios for sex discrimination, Howells must have learned something of the potential power of multivariate statistics from Hotelling, who was to become particularly creative in the development of principal components analysis.

Before the Second World War, the American biological and social sciences generally and anthropology in particular were very much behind their British counterparts in quantitative methods. Between them, R. A. Fisher and Karl Pearson were revolutionizing evolutionary biology with their quantitative perspectives. In the process they developed many statistical approaches and techniques still at the heart of quantitative methodology. Pearson developed regression analysis, the correlation coefficient, and the chi square test. Fisher formulated the analysis of variance, discriminant function analysis, and the method of maximum likelihood, as well as a remarkable amount of population genetics theory.

Alone in his cohort of American anthropologists, Howells saw he had to master multivariate methods as well as proper statistical design. His keen critical sense made him realize the dead-end that American physical anthropology had reached in the 1930s. Mindless measuring had almost become an end in itself.

In 1937 Howells accepted a position as assistant professor at the University of Wisconsin at Madison, where (except for a period during World War II when he served in the Office of Naval Intelligence in Washington) he remained until 1954. This was a period of great maturation for Howells. During the years in Madison, he spent a considerable amount of time in the Statistics Department learning multivariate statistics. He told his children that it was a very hard task, but it simply had to be done to accomplish what he envisioned. The timing was propitious: High-speed computers became readily available around 1950, making the application of multivariate statistics to large datasets feasible for the first time. Howells first successfully applied factor analysis to body composition in a series of papers around 1950. Those results contradicted William H. Sheldon's essentialist scheme of three separate components (ectomorphy, endomorphy, and mesomorphy) and showed that the primary variant of physique was simply size, with a secondary component of fatness.

Howells did not just develop his research skills at Madison. He was always a conscientious and thoughtful participant in university affairs. He was a key participant in the development of Wisconsin's Integrated Liberal Studies program, which was a pioneering attempt to bring the interdisciplinary approach to undergraduate teaching. As chair of the Department of Sociology and Anthropology, he was known for his civility and thoughtfulness. One archaeologist (Chester Chard), who had been hired while Howells was chair, was impressed that shortly after he was hired, the Howellses had a dinner for him and his wife, inviting their own friends outside the department, to broaden the newcomers' social circle.

It was while he was at Madison that Howells began to publish books for the general audience. He felt it was an obligation for scholars and scientists to communicate their findings to a broader public. The first of six such books, *Mankind So Far* (1944), was written at the urging of Hooton, who had been approached by a publisher to write his own book on human evolution. The publisher rejected Howells's first chapters, but after Hooton urged reconsideration, suddenly decided the chapters had been "remarkably improved" (they were unchanged). The book was published 10 years before the Piltdown hoax unraveled (while Hooton and others still championed Piltdown's importance), but after discussing it, Howells set Piltdown aside, since to him it seemed to fly in the face of so much other evidence. This was typical of his quiet but firm belief in his own judgment. This success was followed by *The Heathens* (1948) on "primitive" religion, by *Back of History* (1954), *Mankind in the Making* (1959), *The Pacific Islanders* (1973,2), and finally *Getting Here* (1993). These books were all refreshing, slyly humorous, highly informative, and superbly informed. They contained few explicit theoretical arguments, but those that were there were memorable, such as the Candelabra, Hatrack, and Noah's Ark schools of human evolution. The books were adopted as texts in many introductory courses across the country and internationally, and they have been more widely translated than those of any other physical anthropologist. The last of his general books (1993) appeared in an updated form when he was 89.

By 1954 he had become established as a leader in the field because of his sophisticated research findings and well-received books (three by that time). He had been elected president of the American Anthropological Association in 1951, had served as editor of the *American Journal of Physical Anthropology* from 1949 to 1954, and was awarded a Viking Fund Medal in 1954. When Hooton died suddenly that year, Howells was picked to succeed him as professor at Harvard and curator at the Peabody Museum.

Howells was a member of the Harvard teaching faculty until 1973 and during this period he continued to publish and gain recognition. He was elected to the National Academy of Sciences in 1967 and received a Distinguished Service Award from the American Anthropological Association in 1978. Howells was elected to nine other scientific societies in the United States, Europe, and Africa.

It was during this period that many of us came to know him as graduate students. There was no identifiable Howells school of physical anthropology. His students went into many subdisciplines (see, for example, the variety of contributors to Giles and Friedlaender, 1976). He consciously did not steer students toward particular interests of his own but rather tried to ensure that they were broadly informed and had the proper tools to address their own research questions. Howells did, however, produce a number of students in craniometrics and in the human biology of the Pacific. He had an abiding research interest in that region: His doctoral thesis was on crania from Melanesia (1934); one of his general books was on the Pacific Islanders (1973,2); and he helped develop the Harvard Solomon Islands project (Friedlaender, 1987). Although he was always pleasant, polite, and affable, we regarded him with some awe. He was always Dr. Howells. He was fair and considerate and could gracefully tell students when they had done poorly. A typical remark accompanying a C-grade paper was, "You can do better than this—WWH." After hearing a halting oral translation of a German text for a language exam, he simply closed the book with a wan smile and told one of us (J.F.), "Why don't you just do some more practice and come back in a couple of months to give it another go?"

At Harvard, Howells was an extremely popular undergraduate lecturer. As his student Michael Crichton (1976, p. xxiii) wrote,

> His style was disarming and he lectured quietly, in a relaxed, conversational manner, with occasional long pauses to look at his notes. The effect was one of complete spontaneity. . . He was a master of what Noel Coward once called "coming out of a different hole each time"—he played on the unexpected element in his lecturing. . . He kept his audience off balance, and they adored him. . . He was a gifted performer, and his imitations of primate gaits were justly famous. But those imitations, like those jokes and puns and anecdotes and newspaper stories sprinkled through his lectures, all made a certain point and were all the more appreciated.

In fact, Howells was an accomplished amateur actor and playwright. He was, with Harvard archaeologist Gordon Willey, among the most active members of Boston's Tavern Club, where he wrote or coauthored 21 plays and directed or performed in at least 18 others, often as the female vocal lead in musicals (this was before women were admitted). Many of these won special prizes, called Bruins.

Although he never railed against typological thinking as his colleague Ernst Mayr so famously did, Bill was clearly a committed population biologist. While Frank Livingstone (1962) made the widely quoted remark, "There are no races, there are only clines," Howells wrote, more accurately, "There are no races, there are only populations" (1995). He did not explicitly teach theory, but simply set aside arguments that were not supported by convincing data, properly analyzed. His advanced courses included excellent and easily understood sections on the proper application of multivariate statistics to anthropological data. For Howells, the correct analysis of the accumulating data on human paleontology and contemporary variation would eventually allow the proper relationships to emerge. He avoided pontificating and was adept at the deflating quip. After a colleague made a particularly pompous prediction on the direction of the field in a department faculty meeting, he replied that he sincerely regretted he lacked such an Olympian perspective. He said of another (in private), "That man wouldn't know a Dryopithecus tooth pattern if it bit him." Howells deflected what he viewed as improper inquiries in the same way. When a graduate student breathlessly pressed him for details on comparative primate genital sizes and shapes, Bill deadpanned, "We only study the hard parts."

Besides his expertise in osteometrics, Howells was a stalwart fieldworker as well. He and Muriel took part in the Harvard-Peabody Museum Solomon Islands project in Malaita in 1968, and he was a member of the 1972 trip to Ulawa and Ontong Java aboard the *Alpha Helix*. Bill was one of the hardest workers on the project, often doing his painstaking cranial anthropometry long after everyone else had retired to their ration of a single bottle of warm Guinness stout. He had the ability to roll and turn over his tongue, and this gave him the opportunity to score this genetic trait on subjects during their examinations. The sight of the distinguished Harvard professor making bizarre movements with his tongue and coaxing perplexed villagers to imitate him was truly wonderful, and he reveled in the interaction. The local "big men," finely attuned to social hierarchies, would often approach Bill as the expedition's "big man," though he was not in fact the leader. When Albert Damon became incapacitated with his final illness during the 1972 trip, Bill did step in to assume command.

Yet remarkably, his most productive research period came during his long and active retirement at the Peabody Museum beginning in 1973. Bill noted its special pleasures (1992): "The discipline of teaching obliges you to try to present important matters in well-rounded, balanced fashion, even as you make your own views known. A nice ideal, but now I can lean back, read without having to revise lecture notes, and tell myself (in private) just what I think of things."

Howells realized, with characteristic clarity, that physical anthropology was in essence a descriptive endeavor and could not then be transformed into an experimental science, as some were attempting. His premier research accomplishment was to provide a comprehensive population-based description of human cranial variation. This meant an appropriate application of multivariate statistics to a large battery of measurements that he and his wife, Muriel, recorded, beginning in the late 1960s, on a well-defined and adequately sampled series of male and female crania. They initially took over 60 measurements on approximately 50 males and 50 females from 18 different skeletal populations from across the globe. The results were published in a series of Peabody Museum monographs, beginning with his authoritative *Cranial Variation in Man* (1973,1), followed by two subsequent expansions (1989, 1995) when he was 87. These data were made available online, augmented by subsequent sets that the Howellses accumulated from other skeletal series. The final total came to over 2100 skulls from 28 basic populations, and approximately 170,000 individual measurements. This dataset continues to be used as the basic global reference for craniometrics today.

Although Howells would never say it directly, since he always avoided personal attacks, this series of monographs should properly be viewed as a systematic debunking of Carleton Coon's controversial hypothesis on race that had appeared in 1962 in *The Races of Man* and in a companion volume (1965). Coon's thesis, which created a furor in anthropology at the time, was that there were five clearly identifiable geographic subspecies or races of humans: Caucasoid, Congoid, Capoid (Khoisan), Mongoloid, and Australoid. Furthermore, according to Coon, these had become mutually distinct at the level of *Homo erectus* hundreds of thousands of years ago, and all had evolved roughly in parallel, semi-independently up to the present. Coon relied heavily on the earlier work of Franz Weidenreich, but he also used a large amount of descriptive data, and both metric and nonmetric cranial observations.

Howells showed that notions of distinct races had no basis in craniometrics, contrary to the long tradition in biological anthropology before his time. His major conclusions were that modern humans are remarkably uniform as a species; that while some geographic patterning is detectable among human groups, the variation within populations substantially outweighs any among-group distinctions;

that this human uniformity appears to be very recent in origin (skulls earlier than roughly 15,000 to 20,000 years old, especially the Neanderthals, are well outside the range of modern human variation and cannot be related to it metrically); and that contrary to accounts from mitochondrial and Y-chromosomal DNA, African populations show no signs of any ancestral or distinctive status. He was skeptical of the Regional Continuity school of modern human origins and more supportive of the Replacement school, as these approaches developed in the 1980s and 1990s.

In Howells's view any distillation of a particular morphological feature as a definitive marker of population affinity, disease, or ancestry was suspect. He delighted in exhibiting to students his own shovel-shaped incisors as examples of supposedly "discrete diagnostic" traits (for North Asians and Native Americans). These and other such "discrete" traits are distributed more broadly in natural populations than is generally realized, and they are determined by poorly understood hereditary and environmental factors. He consequently distrusted Weidenreich's attempts (as well as those of his Regional Continuity followers) to trace the ancestry of particular modern human populations back to certain prehistoric fossils through a selection of such shared morphological characters. Instead he relied on size and shape relationships to establish population ties.

In his retirement he received even more honors. In addition to the Distinguished Service Award given by the American Anthropological Association in 1978, he received the Charles Darwin Lifetime Achievement Award of the American Association of Physical Anthropologists at its inception in 1992. In 1993 the William W. Howells Book Prize for general books in physical anthropology was created in his honor by the Biological Anthropology Section of the American Anthropological Association.

Almost until the end he was as mentally sharp and perceptive as ever. Barely two years ago Dan Lieberman and one of us (D.P.) visited him in Kittery Point to show him the unpublished reconstruction of the *Sahelanthropus* cranium, and Bill's comments showed that he was even then at the top of his game; he kept up with an eclectic literature practically until his death.

For his beloved Peabody Museum he and his wife endowed the Howells Directorship in 1998. In 2002 Muriel Howells died, after 73 years of marriage. A daughter, Gurdon Metz; a son, William Dean Howells; four grandchildren; and five great-grandchildren survive him.

He was truly a man of many excellent parts, and he will be long and fondly remembered.

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Photograph courtesy The Rockefeller University Archives.

HENRY G. KUNKEL

September 9, 1916–December 15, 1983

BY JACOB B. NATVIG AND J. DONALD CAPRA

HENRY G. KUNKEL WAS a true pioneer in immunology. During his lifetime, he led in an area of medicine and basic science that dates back to the turn of the twentieth century. His work placed him in the company of Emil von Behring, Ehrlich, Landsteiner, and other giants in the field. From the middle 1940s he was one of the world leaders in applying the fundamental scientific principles of immunology to clinical medicine, framing a field now termed clinical immunology. Early in his career he proposed that myeloma proteins could serve as models for normal immunoglobulins and antibodies. His intuition proved correct, and his work and the work of others that followed changed the course of immunology. He (and many of his trainees) used myeloma proteins to decipher the chain structure of immunoglobulins and antibodies. This chain structure allowed the definition of immunoglobulin classes, subclasses and genetic markers, which led to the first mapping of immunoglobulin genes to their respective chromosomes. His discoveries also reverberated through cellular immunology through his identification of major histocompatiblity complex (MHC) class II molecules as separate entities, and the genetic linkage of MHC classes I and II molecules with factors in the complement system. Thus, his work had enormous influence on the entire course of basic immunology. At the same time it established pathogenetic mechanisms that brought new diagnostic tools to the clinic.

Despite his intense interest in basic science, his first love was clinical medicine, which he looked upon as an avocation. Here he made major contributions to the diagnosis, and to understanding the pathogenesis of many diseases, and employed new therapeutic strategies for the treatment of many of these same diseases. His work substantially impacted our understanding and subsequent treatment of chronic liver disease, systemic lupus erythematosus, rheumatoid arthritis, primary immunodeficiency disorders, and lymphoproliferative diseases. In addition to his basic science and clinical contributions, he was one of the most sought after teachers and mentors for young scientists interested in the new field of immunology. His trainees included one Nobel Laureate, four members of the National Academy of Sciences and many distinguished scientists, including department chairs, institute presidents, deans, and others who are conducting both basic and clinical research throughout the world. His trainees are prevalent in the United States and Europe, and particularly in Scandinavia, where he had spent a happy and productive year as a visiting investigator with Nobel Laureate Dr. Arne Tiselius in Uppsala, Sweden.

Henry Kunkel's parents clearly helped focus his passion. He was born in Brooklyn on September 9, 1916, the son of the distinguished botanist Louis O. Kunkel and his wife, Johanna Kunkel. His father was a professor of plant pathology at the Rockefeller Institute (later university), who would later be elected to the National Academy of Sciences. His mother was an ardent horticulturist. His parents' passion for botany and biology kindled his interest at a very early age. He once told how he and his friends as children often had competitions to collect the widest variety of flowers as they played in the fields. In his later life, hybridizing irises became a major hobby. He grew up in Yonkers, New York, and in Princeton, New Jersey. He became an accomplished tennis player, and while at Princeton was elected captain of the varsity tennis team. His competitive ability served him well as he continued to hone his scientific skills. This spirit of competition was balanced by his passion for scientific understanding. It was a rite of passage for all his students to "take him on" on the tennis courts. Few won.

He graduated from Princeton University in 1938 and attended Johns Hopkins University Medical School, earning his M.D. in 1942. After spending two more years in training, as a house officer at Bellevue Hospital in New York City, he joined the U.S. Navy. He served in the European theatre as a physician and participated in the Allied invasion of Italy, during which several marines with hepatitis came under his care. This was a major turning point in his life. In 1945 he came to the Rockefeller Institute and Hospital in New York City (which later became the Rockefeller University) and because of his experience with hepatitis, was assigned to the navy's infectious hepatitis program. He maintained a lifelong interest in liver disease and, indeed, his first exposure to immunology was through his interest in hepatitis. He was appointed an assistant member at Rockefeller in 1947, associate member in 1949, and a full member in 1952. He became an adjunct professor of medicine at Cornell University Medical School in 1973. He was named the Abby Rockefeller Mauzè Professor in 1976. Except for the year 1950-1951 at the Biochemical Institute in Uppsala, Sweden, he remained at the Rockefeller University throughout his career.

Although he performed some research as a medical student, it was while he was a house officer that he became interested in clinical investigation. At Bellevue he was greatly influenced by Dr. William Tillett, then chief of the medical service. Tillet instilled in him the value of formal clinical investigation, and fired his enthusiasm for this work. Upon arriving at the Rockefeller Institute for Medical Research, he joined the laboratory of Charles L. Hoagland. The Hoagland Laboratory studied infectious hepatitis and, as noted above, was affiliated with the Naval Research Unit at Rockefeller. Kunkel rapidly developed an interest in both the clinical and biochemical events associated with various liver diseases. One year after he arrived at the Rockefeller Institute, Hoagland died unexpectedly at a very young age, and shortly afterward, Kunkel was appointed to head the laboratory. In the absence of a formal mentor his intellect and intuition were tested and forged at this critical juncture.

During this early period, he displayed his brilliance in clinical investigation. His studies on liver disease led to the description of two important clinical syndromes. One dealt with young females with liver disease and hypergammaglobulinemia. These patients often displayed active arthritis and positive LE cells. The second syndrome described in collaboration with Edward H. Ahrens Jr. was primary biliary cirrhosis. His analysis of these syndromes testified to his gift in identifying and linking important issues in clinical research by studying only a few patients in great depth.

In studying liver disease Henry Kunkel observed disturbances in the patients' serum proteins and named his service at the Rockefeller University Hospital the Protein Metabolism Unit. His method for measuring serum proteins, such as gamma globulins, by turbidimetric flocculation, using zinc sulphate, was widely used clinically in the 1950s and 1960s. He noted that a markedly elevated gamma globulin level in some patients with cirrhosis was associated with increased numbers of plasma cells in the bone marrow. This finding, in conjunction with the observation that marked increases in gamma globulin were also seen in patients with multiple myeloma, led him to postulate that myeloma proteins made by malignant plasma cells were reflective of normal gamma globulin. He used simple immunochemical techniques, primarily the generation of antisera and antigenic analysis by Ouchterlony immunodiffusion, to demonstrate antigenic similarities between myeloma proteins and normal immunoglobulins. The discovery was for many years rather controversial so that even the Nobel Laureate Rodney Porter and many others considered the myeloma proteins as paraproteins. Kunkel's seminal discovery of myeloma proteins as models for normal immunoglobulins markedly facilitated unraveling the genetics and structure of antibody molecules. It also marked the beginning of his lifelong study of immunoglobulins and B cells (immunoglobulin-producing lymphocytes), using B cell tumors as a model system.

His scientific career was greatly impacted by his year of study in the laboratory of Arne Tiselius in Uppsala. Here he solidified his concept that integrating basic sciences was crucial to forming a deeper understanding of clinical problems. In the Tiselius laboratory he learned physical chemistry and became an expert in free-boundary electrophoresis. His ingenuity in the laboratory was again displayed when he used pevikon, a commercial starch, as an inert solid support to separate large volumes of serum into focused electrophoretic bands. For many years pevikon block electrophoresis was used in his and later many other laboratories to isolate large amounts of homogeneous myeloma proteins for structural and antigenic analyses. In the 1950s Kunkel also made another seminal observation using pevikon block electrophoresis by identifying in normals a previously unknown hemoglobin (Hb) that he termed Hb A2. He also found Hb A2 very much increased in thalassemia minor. The finding of two hemoglobins in normals also influenced his thinking about immunoglobulin classes and subclasses. Henry Kunkel had a true knack when it came to recognizing the importance of identifying the right tools for specific scientific applications. His laboratory had the third Beckman Model E analytic ultracentrifuge comercially available and one of the earliest commercial preparative ultracentrifuges. Both analytic and preparative ultracentrifugation techniques served him well, and were used extensively in the Kunkel laboratory between 1950 and 1970.

During that period, his laboratory contributed significantly to our understanding of gamma globulin structure and genetics. An essential discovery was the finding that myeloma proteins and normal antibody molecules possessed individual antigenic specificities that were later termed idiotypic specificities. The interpretation of these individual specificities was at first perplexing. Ultimately, they were shown to be markers for the variable regions of antibodies, providing a major conceptual insight into the new field of antibody diversity. Later, cross-idiotypic specificity related to the antigen-binding site was described, and has since been used to define groups of antibodies with similar antigenic reactivity.

Using his keen perspective, he identified relationships among many myeloma proteins and normal immunoglobulins from thousands of Ouchterlony plates. He identified 19S IgM as a class of immunoglobulin distinct from 7S IgG. Four subclasses of human IgG were discovered. A second IgA subclass with no disulfide bond linking its heavy and light chain was described. His laboratory described the heavy and light chain structure of immunoglobulin as well as the two classes of light chains (kappa and lambda). The genetics of human immunoglobulins were largely worked out with homogeneous myeloma proteins and the heavy chain linkage groups were delineated. In addition, immunoglobulin deficiencies with absence of subclasses of IgG were described. His laboratory was instrumental in the initiation of the chemical characterization of the complement system. Clq was described as the first chemically defined component of the classical pathway. The scope and impact of the totality of these discoveries cannot be overstated.

It was during this period that we both came to his laboratory as fellows. It is safe to say that the training period in the Kunkel laboratory was the transforming event of our lives. Under his tutelage, we learned the skills that have kept both of us grounded as investigators. We both took from his laboratory the philosophy of studying the patient, then studying the disease, and then applying the principle back to normal physiology. The relationships established in his laboratory have not only been rewarding throughout the years, they have also influenced how we set up our laboratories and our interactions with our students. The impact of these training years in his laboratory was profound.

During this time, his laboratory also contributed significantly to clinical immunology, impacting two important autoimmune disorders: systemic lupus erythematosus (SLE) and rheumatoid arthritis (RA). His work in SLE was directly related to the liver disease syndromes associated with hypergammaglobulinemia and arthritis. He realized SLE as a distinct clinical and pathologic entity with no dominant liver manifestations. His laboratory demonstrated that SLE resulted from the mounting of an autoimmune response against nuclear constituents. Antibodies specific to DNA—ribonuclear proteins, including Sm, histones, mitochondria, and microsomes—were all described. Most importantly, the concept of immune complex diseases was proposed and proven by demonstrating the presence of specific autoantibodies in kidney eluates and showing the circulation of these complexes during disease flares. Today SLE is considered the prototypic autoimmune disease. In the case of rheumatoid arthritis, rheumatoid factors were shown to be 19S IgM antibodies. This IgM existed in the serum as a complex with 7S IgG. Other immune complexes were also described. In particular, IgG-IgG complexes involving IgG rheumatoid factor were detected in high concentrations in synovial fluids of these patients, and he realized that these might play a significant role in complement activation and inflammation.

In the 1970s he turned his investigative effort to study the cellular basis of the immune response. He continued his early strategy of studying a few patients well. He selected antinuclear antibodies (ANA) as an important focus for his laboratory, in addition to continuing his efforts to understand B cell maturation. With simplicity and elegance his laboratory showed that IgM and IgD were the primary membrane immunoglobulins. These two antibodies were shown to have identical V regions on the cellular level as demonstrated by anti-idiotypic antibodies. In addition, the idiotypic determinants were used as tumor markers for B cells, thus demonstrating for the first time that differentiation was not arrested in most cases of B cell leukemias. His laboratory also described a marker on B cells that was later shown to be CD40, a major costimulatory molecule for B cell activation and differentiation.

In the area of immunodeficiency, defective genes were identified in families with specific deficiencies in Ig subclasses. The cellular basis for immunoglobulin deficiency was explored in conjunction with demonstrating T cell helper factors for normal B cell activation and differentiation. Differentiation of T cells was demonstrated in some patients with B cell leukemias and in patients with common variable immunodeficiencies. And a class of T cells was identified that was capable of reversing some cases of immunodeficiency. The latter provided some of the first evidence for direct T-B interaction in B cell activation.

In his unique style, and with his scientific accomplishments, he established himself as a supreme model for what today we call a clinical scholar. He consistently uncovered basic immunological principles by studying patients. His peers sometimes felt that the issues he studied were mundane, only realizing later the full impact of his investigations. He was able to identify important issues that were suitable for fruitful scientific exploration, using the tools of his time. He often broke ground in a new field of investigation and then moved on to the next area, leaving other investigators to fill in the details. He had a unique talent for applying the tools and concepts learned in other fields to his own investigations. He expressed his excitement and enthusiasm for science clearly through a twinkling of his eyes when he encountered exciting ideas. His standard of scientific rigor was unsurpassed. He felt strongly that he could not publish any work that was not formed to the very best of his intellectual ability, putting great emphasis on reproducibility, accuracy, and critical interpretation of the data. Above all, he put a premium on originality. He advised his trainees upon leaving his laboratory to think big. Throughout his scientific career, he put that advice into his own practice.

Henry Kunkel had the foresight to identify and address the difficulties inherent in training clinical investigators. His thoughts on this topic first emerged formally in his 1962 presidential address for the American Society for Clinical Investigation, "The Training of Clinical Investigators," a topic that at the time was largely ignored. He made a deliberate decision early in his scientific career to focus his mentoring primarily on M.D. applicants. He understood that these M.D. applicants would require a considerable effort to train, and he consistently worked to turn them into outstanding clinical investigators. This is demonstrated by the large number of his trainees who assumed leadership positions in immunology, both in the United States and abroad. His philosophy of science had a profound influence on his trainees. Nearly all emerged from his tutelage with a strong Henry Kunkel imprint, which placed great emphasis on originality, accuracy, and the significance of one's investigative work. In his 1975 presidential address to the members of the American Association of Immunologists he gave a strong plea for enhanced ethics, which exemplified Kunkel's concerns for the integrity of the scientific enterprise (*Journal of Immunology*, vol. 115, no. 1, Jul. 1975).

Henry Kunkel was also a family man. His wife, Betty, was an accomplished figure skater and skating instructor. In addition, she was an important social partner in his professional career and was a gracious hostess for his many friends and students from all over the world. His children inherited a keen sense for matters of science. His younger son, Henry ("Hank"), acquired expertise in informatics and became a successful data management expert in banking and financial areas. In addition, he continued his father's interest in plant genetics. His eldest son, Louis, became an outstanding molecular biologist and geneticist. Louis's election to the National Academy of Sciences marked (to our knowledge) the first such three-generation NAS membership. His daughter, Ellen, was a promising neuroscientist. Despite her tragic early death, she made a substantial impact in the field.

Kunkel served on numerous editorial boards. Most importantly, he was an editor for the *Journal of Experimental Medicine* from 1960 until his death. He was also the co-founding editor for the major review series in immunology, *Advances of Immunology*. Through his editorship for these two important scientific journals, he had considerable influence in advancing the field of immunology during his lifetime and beyond. Through his contributions in science, training, and public service, he earned the right to be called an immunologist's immunologist.

Henry Kunkel received numerous awards and prizes, including the Lasker Award for Clinical Research. He was awarded honorary doctorate degrees from the University of Uppsala and from Harvard University during its 300th anniversary. He served on numerous committees and organizations, including on the council for the National Institute of Arthritis and Metabolic Diseases and as president of the American Society of Clinical Investigation and the American Association of Immunologists.

In closing, we believe Jonathan Uhr and Donald Seldin best captured the essence of the man with these two sentences (Journal of Immunology vol. 132, 2144-2145, 1984):

> His loyalty to and affection for his students and friends were unsurpassed. Nevertheless, his influence will continue to be felt as his former students carry on in their leadership roles and train a new generation of students with the same high standards that Henry represented.

WE ARE INDEBTED TO Drs. Alexander G. Bearn, Nicholas Chiorazzi, Shu Man Fu, Morten Harboe, Henry Metzger, and Robert Winchester for advice in writing this memoir. As helpful background material, we have used the special issue of the *Scandinavian Journal of Immunology* for Henry G. Kunkel's sixtieth birthday in 1976 (vol. 5, nos. 6-7, Sept. 1976); Henry G. Kunkel 1916-1983, An Appreciation of a

Man and His Scientific Contributions and a Bibliography of His Research Papers (*Journal of Experimental Medicine*, vol. 161, pp. 869-896, May 1985), and a biography of Henry G. Kunkel published by the Henry Kunkel Society in 2001.

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Photograph courtesy Harvard News Office

Hallam L. Movius Jr.

HALLAM LEONARD MOVIUS JR.

November 28, 1907–May 30, 1987

BY HARVEY M. BRICKER

HALLAM L. MOVIUS JR. WAS A Palaeolithic archaeologist, a specialist in the interpretation of human behavior and its environmental context during the latter part of the Old Stone Age, toward the end of the Pleistocene Epoch.[1] With broad training and varied field experience in Europe, the Near East, and Southeast Asia, he became in the years after World War II the preeminent spokesman for Palaeolithic archaeology in the United States. In his classes at Harvard and on his excavations in France, he was instrumental in training a generation of American and European archaeologists. His decades-long investigation of the Abri Pataud, a large Upper Palaeolithic rock shelter in southwestern France, formed the basis for what is today a French government museum and research center at the site.

Hallam Leonard Movius Jr. was born in Newton, Massachusetts, on November 28, 1907. He was the son of Alice Lee West Movius and Hallam Leonard Movius, an eminent landscape architect. Movius was educated at the Berkshire School in Sheffield, Massachusetts, and at Harvard College, which he entered in 1926, graduating with an S.B. degree in 1930. Immediately upon graduation, Movius started in on what would be his professional career by joining a six-month archaeological expedition to Central Europe sponsored by Harvard and the University of Pennsylvania. Upon his return to the United States, he began graduate work at Harvard in the Stone Age archaeology of the Old World, and during the years of his graduate training, he participated in fieldwork of very varied nature in both Europe and southwest Asia.

Movius's field experience in 1931 was an introduction to the archaeology and archaeologists of the Western European area that would be the locus of his latest and most important professional contributions. In the summer of that year he was one of several students in the summer field season of the American School of Prehistoric Research, an organization founded and directed by George Grant MacCurdy of Yale University. For much of the summer the group visited archaeological sites and museums in England, France, Germany, Switzerland, Austria, and Czechoslovakia, receiving private tours and lectures from many of the principal researchers in the field of European Stone Age prehistory. The Palaeolithic (Old Stone Age) sites in France to which Movius was introduced that summer included ones in Les Eyzies and elsewhere in the Vézère Valley of the Dordogne region; it was to Les Eyzies that he returned to start major field work following World War II. At the end of the American School's study tour in 1931, Movius stayed on in Czechoslovakia to excavate briefly with the Harvard-Penn expedition and then joined MacCurdy on a month-long archaeological reconnaissance trip through Yugoslavia.

In the spring of 1932 Movius joined the excavations at the site of Mugharet es-Skhul in the Mt. Carmel range of Israel that were being carried out by a joint expedition of the American School of Prehistoric Research and the British School of Archaeology in Jerusalem. The codirector representing the American School was Theodore McCown, and Hallam Movius was his assistant. The site was known to be of great importance because fossilized human skeletal material associated with Middle Palaeolithic artifacts had been found there the previous year. During the even more rewarding 1932 season, the remains of an additional eight to ten individuals were found, including the highly significant Skhul V, discovered by McCown and Movius on May 2, 1932. It was immediately obvious that this fossil was extremely important for understanding the relationship, evolutionary or otherwise, between Neanderthal and modern humans, but only at the end of the twentieth century did newly developed techniques of chronometric dating clarify the situation. It is now known that Skhul V dates to between 80,000 and 100,000 years ago and represents one of the earliest groups of modern humans to be found outside Africa, where such humans evolved.[2] Although Movius had a small part in the fascinating story of modern human origins in southwest Asia, he did not return to that area or topic during his professional career.

Having received his M.A. in anthropology from Harvard in 1932, Movius began the fieldwork that would lead to his Ph.D. dissertation during the summer of that same year, shortly after his return from southwest Asia. This new project was one part of a large interdisciplinary program, the Harvard Irish Survey, which included physical anthropologists (Earnest Hooton, C. Wesley Dupertius), sociologists (W. Lloyd Warner, Conrad Arensburg, Solon Kimball), and archaeologists. The archaeological research was supervised by Hugh Hencken, director of the Harvard Archaeological Expedition to Ireland, and Hallam Movius was the assistant director. Movius was given the responsibility for investigating the earliest sites, those of the Stone Age (primarily Mesolithic). He spent five summer seasons, 1932 through 1936, in Ireland (both Eire and Northern Ireland), excavating six different sites. This work provided the material for his doctoral dissertation, and he received his Ph.D. in anthropology from Harvard in 1937.

As important as the Irish work may have seemed on the professional plane, it was no less important to Movius in the personal realm. During the later years of the project, an Australian archaeology student at Cambridge University, Nancy Champion de Crespigny, from Adelaide, South Australia, was one of the field assistants. Hallam and Nancy were married in 1936. Thus began not only a *connubium* but also a very effective professional collaboration. In a publication on his Irish work Movius thanked Nancy for her help with the management of the expedition in 1935. Nancy continued to provide much of the essential logistical and managerial support for all of Movius's subsequent fieldwork.

Movius's first fieldwork after completion of his doctorate and his marriage was as archaeologist and assistant director of what was called the American Southeast Asiatic Expedition (or sometimes the Harvard-Carnegie Expedition). The main goal of the expedition was to survey the geology, paleontology, and Stone Age archaeology of the Irrawady Valley in Burma and to relate the anticipated new information to the somewhat better known sequences of India, China, and Java.[3] The expedition was directed by geologist Helmut de Terra (Academy of Natural Sciences of Philadelphia and Carnegie Institution of Washington). The eminent paleontologist and Jesuit theologian Pierre Teilhard de Chardin took part in the expedition as a consultant on the fossil faunas of the region and comparisons with China. Movius was in charge of the archaeological research. He and Nancy made the long ocean voyage, first to Calcutta and then to Rangoon, continuing on by train and river steamer to Mandalay, and finally by whatever land transport was available, further northeast into the southern Shan states.

In later years Movius referred often to the difficult and adventurous aspects of this expedition. Beyond the archaeology, which was his primary concern, he was impressed by the linguistic complexity in this region of Upper Burma. A story that I heard him recount several times involved their getting lost somewhere in the hills and the inability of their Burmese driver to ask directions of the local inhabitants. The difficulty was that the driver's dialect had only three tones (as I remember), whereas the closely related but crucially different local dialect had five. After three months of fieldwork in Burma, the members of the expedition traveled to Java to study the geological context of the famous *Homo erectus* localities (Trinil, Modjokerto, and others) and to examine existing archaeological and paleontological collections from various Pleistocene localities on the island. The expedition ended in May 1938.

Movius's archaeological investigations in southeast Asia, the results of which were published during and just after World War II (1943; 1944; 1949,1-2), were pioneering contributions to knowledge of an area of the world whose Stone Age prehistory was virtually unknown. A general conclusion based on the wealth of specific data gathered was that during much of the Early Stone Age the archaeological materials in eastern and southeastern Asia were fundamentally different from those in western Asia, Europe, and Africa. In this eastern province the early hominid inhabitants manufactured rather simple core tools (choppers and chopping tools) and flake tools. In the western and southern province, however, core tools of the same period were more completely patterned (Acheulian handaxes and cleavers), and flakes for tool manufacture were often struck from specially prepared Levallois cores. This model, described in oversimplified form as the absence of Acheulian handaxes in eastern Asia, was codified by others in terms of a Movius

Line, running northwest to southeast across the Eurasian continent, separating the realm of the west from the very different eastern realm. There is no question that Hallam Movius is better known for the Movius Line than for anything else. It is, however, the case that this model, in its original form, is now known to be incorrect. Discoveries of handaxes and other Acheulian materials at sites in China at the end of the twentieth century showed that east and west were not completely isolated realms at this time despite what are in most cases quite different archaeological sequences.[4]

Movius's return to the United States from the Asian expedition in 1938 marked the end of the exciting first stage of his professional career. In addition to preparing the monographic reports on the Palaeolithic archaeology of Southeast Asia, he was revising his dissertation for publication by the Cambridge University Press. The outbreak of World War II in Europe complicated communications with the press. In his preface to *The Irish Stone Age*, which was published in 1942, Movius expressed his grateful admiration of the editorial staff, which "carried on with the job" despite the loss of proofs and other materials "due to enemy action."

As of 1939 Movius held the title of assistant curator of Palaeolithic archaeology at Harvard's Peabody Museum, but it was not obvious where his career would go from there. After Hallam's death, Nancy told me that there was a period when he thought that a career in Old World prehistory might not be possible (with many of the areas of interest to him occupied by invading armies) and that he might have to switch his specialty to the archaeology of the United States. This is apparently the explanation for an otherwise puzzling entry in his publication list—a brief report published in 1941 on excavations at a prehistoric site in Massachusetts. The entry of the United States into World War II in December 1941 removed all doubt about what Movius would do next.

Movius entered military service in 1942 as a first lieutenant in the 12th U.S. Army Air Force. He served in the Mediterranean theater, primarily in southern Italy, for over three years. He was an intelligence officer attached to a unit whose duties included assessing bombing damage inflicted on Axis industrial plants, tracking the extent to which damage from previous raids was being repaired, and recommending the scheduling of future bombing raids such that the fruitless expenditure of enemy resources would be maximized. Movius's wartime work was recognized by the award of the Legion of Merit, given "for exceptionally meritorious conduct in the performance of outstanding services and achievements."[5] He left the Air Force in early 1946 with the rank of lieutenant colonel.

Upon returning from the war, Movius began his career as a member of the Harvard faculty. He was appointed lecturer in the Department of Anthropology in 1948, promoted to associate professor with tenure in 1950, and promoted again to professor in 1957. He and Nancy added to their family during this period. A son, Geoffrey, had been born before the war in 1940, and now a daughter, Alice, was born in 1947. Movius's professional activities in these early postwar years were, in addition to teaching, of two kinds. He very quickly got started with new field research (discussed below), and through an ambitious program of publication, he set about establishing himself as one of this nation's leading experts on Palaeolithic archaeology.

During the late 1940s and early 1950s, Movius served as an interpreter or broker for American anthropologists of important new information about the Old Stone Age that was being published in Europe and Asia in languages other than English. Such publications included reports on human fossil finds in France (1948) and Uzbekistan (1953,1), a very ancient Lower Palaeolithic wooden spear from Germany (1950,2), and various sites in northern China (1956,1). As radiocarbon dating was first being developed, Movius wrote several articles (e.g., 1950,1) exploring its potential applications in Palaeolithic archaeology, a topic he followed up in the early 1960s with major critical reviews of radiocarbon dates on Upper Palaeolithic sites in Central and Western Europe (1960, 1963). Collaborating with a project of the Geological Society of America, he published a detailed review of Villafranchian stratigraphy in Western Europe (1949,3). With Henri Vallois, a French physical anthropologist, he coedited the *Catalogue des hommes fossiles* (1953,3), the result of an international project to inventory all known fossils of early humans and their ancestors. In 1952 the Wenner-Gren Foundation for Anthropological Research held a two-week symposium to assess the state of anthropological knowledge and practice in the middle of the twentieth century. Movius was chosen to prepare the inventory paper on Palaeolithic archaeology (1953,2), a measure of the stature he had achieved in his discipline. This leadership position was recognized by national and international honors throughout his career. He received the Viking Fund Medal for Archaeology in 1949, and he was elected to membership in the National Academy of Sciences in 1957. He was, in addition, a member of the American Academy of Arts and Sciences, and in 1970 he was named a Chevalier des Arts et Lettres of the Republic of France.

The first archaeological fieldwork done by Movius after the war was the excavation of a large rockshelter, La Colombière, in the foothills of the Jura Mountains in east-central France. The rockshelter was occupied by Upper Palaeolithic groups at several different times near the end of the Pleistocene Epoch. In the summer of 1948 an interdisciplinary research program codirected by Movius and Kirk Bryan, a Harvard geologist, began the field investigation of the human occupations of the site and of their geological and climatological contexts. Bryan died in 1950, and the geological research was completed by his colleague, Sheldon Judson, who coauthored with Movius the monographic site report (1956,2). The age and cultural affiliation of the principal archaeological level at La Colombière have been controversial, and radiocarbon dating done at the time was inconclusive. On the basis of more recent research,[6] the age of the occupation is now believed to be some 10 millennia younger than Movius thought (Magdalenian rather than Gravettian). Earlier excavations of a portion of this level by French prehistorians had discovered a series of eight water-smoothed river cobbles covered with engravings of Ice Age animals, and a ninth such object was discovered by Movius in 1948. These excellent examples of Palaeolithic mobiliary art make La Colombière one of the important sites of this age in eastern France.

In the summer of 1949, with the archaeological part of the fieldwork at La Colombière having been completed, Movius spent several months in France, mostly in the Dordogne region of the southwest, talking with local prehistorians and looking for a good Upper Palaeolithic site at which to start a major new excavation. The site he chose was a large collapsed rockshelter overlooking the Vézère Valley in the town of Les Eyzies. It was this site, the Abri Pataud, that would be Movius's primary professional concern from then until the end of his career. He did a test excavation in 1953 on the part of the site then accessible to him. The property was, however, part of a working farm, and a barn stood on the main portion of the site. In 1957 Harvard purchased the property and immediately transferred ownership to the French government, which in turn granted what became known as the Harvard Dordogne Project the excavation rights for a 20-year period. Six seasons of excavation were conducted at the Abri Pataud between 1958 and 1964. The old farmhouse and its ancillary structures (located, in fact, in a second walled-up rockshelter) were converted into laboratory and storage areas so that on-site analysis of the excavated materials could continue throughout the year. Early in the project, Hallam and Nancy acquired a property, Roque Veyral, just a few kilometers distant from the Abri Pataud and renovated it into a combination residence and laboratory. At the site or at Roque Veyral, or both, research and writing about the Abri Pataud continued for at least part of every year for nearly two decades.

Hallam Movius's Abri Pataud project made several kinds of contributions to Palaeolithic archaeology and European prehistory. First, and most obviously, it answered substantive technical questions about the sequence and radiocarbon dating of Upper Palaeolithic archaeological cultures in southwestern France, a classic region for the understanding of human behavior at the end of the Ice Age. The site was, in fact, occupied repeatedly between about 34,000 and 20,000 radiocarbon years ago, by people representing the Aurignacian, Gravettian, Noaillian, and Solutrean archaeological cultures.

Second, it provided for U.S. archaeologists a model of the sort of broadly interdisciplinary approach to an archaeological site that was becoming standard operating procedure for Old World prehistory after World War II. The breadth of Movius's research plan can be seen from the contributors to the introductory volume of the multivolume site report (1975): these included two archaeologists, two geologists, a vertebrate paleontologist, a malacologist, two human paleontologists, a palynologist, and two ecological biologists.

Third, the Abri Pataud project invented and tested new techniques for excavating large rockshelters, especially the simultaneous control of vertical (stratigraphic) and lateral variation in the archaeological deposits.

Fourth, the operation directed by Movius served as a training ground for a generation of aspiring Palaeolithic archaeologists, primarily from North America and Europe. During much of the 1950s and 1960s, the Abri Pataud and the several sites excavated by François Bordes of the University of Bordeaux, who was also digging in the Dordogne, were the principal training academies in this field. During the course of his operation, Movius had 78 field and laboratory assistants coming from 11 different nations. An important part of the training occurred on the Sunday excursions led by Hallam and Nancy for the benefit of the crew. The students visited sites and museums and met many of the prehistorians active in southwestern France at the time. Movius had benefited from such opportunities in the 1930s as a student with the American School of Prehistoric Research, and he considered them valuable for his own students.

Fifth, Movius and several of his graduate students working at the Abri Pataud developed new techniques of subtypological "attribute analysis" for the study of Upper Palaeolithic chipped lithic tools (1969, 1970, 1971). This work extended previous work on French material by James Sackett,[7] another of Movius's graduate students, and paralleled similar techniques being advocated in the United States by Albert Spaulding[8] and being applied in Hungary by Lászlo Vértes.[9] The full potential of this approach, which could later be realized because of developments in personal computing, was not achieved by the Pataud group or by others working in the 1960s and 1970s. It did, however, move the analysis of the Abri Pataud materials completely away from what had been the traditional typological or index fossil approach.

In 1970, still in his early sixties and at the height of his career, Movius suffered a stroke while working at the Abri Pataud. He recovered almost fully, with only a lingering weakness on one side that required him to walk with a cane. For several years he continued to teach and to spend part of every year in Dordogne pursuing his research and writing. The site report on the Abri Pataud was planned as a multivolume monograph series to be published by Harvard's Peabody Museum as bulletins of the American School of Prehistoric Research. Movius saw the first two volumes through to publication, in 1975 and 1977, but a series of increasingly debilitating health problems made it more and more difficult for him to take an active part in the publication program. He retired from teaching in 1974 and from his curatorship at the Peabody Museum in 1976. Two more site report monographs[10,11] were published in 1984 and 1985, but the Peabody Museum had already confirmed its inability to proceed with the final three volumes planned for the series. In view of the great importance of the site to the profession, the director of antiquities for southwestern France proposed that a one-volume, French-language summary report on the entire Abri Pataud operation be compiled and published at French government expense. Movius enthusiastically endorsed this plan, and the volume in question[12] was published in Paris in 1995, some years after Movius's death. Movius's *Avant propos* to this volume, dated December 1985, was the last thing he wrote about the great site that was the capstone of his career. Hallam Movius died in Cambridge, Massachusetts, on May 30, 1987.

The Abri Pataud continues to provide important information about Upper Palaeolithic prehistory to both specialists and the general public. The French government has built a building over the excavated part of the rockshelter, and Movius's laboratory areas have been turned into a research center and a museum, le Musée de l'abri Pataud, with outreach programs that enrich the cultural life of the region.[13] The contributions of Hallam Movius to French prehistory are prominently chronicled in this museum.

I was closely associated with Hallam Movius during the last quarter-century of his life. He was my teacher, dissertation director, and valued colleague, and he and Nancy became warm personal friends. Two aspects of his legacy to me I regard as most important and most indicative of what kind of man there was behind the publication list and the professional honors. The first was his insistence that his students gain the fullest possible knowledge of the history of their discipline and of the particular problems on which they were working. In his teaching and in his own research, he fully implemented the proposition that we, as scholars, must stand on the shoulders of our predecessors, endeavoring to give to our successors a platform just a bit higher than the one on which we ourselves first stood. We can build on the platform of previous research only if we have taken the trouble to learn about it. The second aspect of his legacy, taught by example on numerous occasions, was the precept that one must not deal with ideas or positions in terms of where they originated. He did not believe in scholarly guilt or merit by association, and he did not discount or ignore the work of a scholar who belonged to the "wrong" school or one that was out of fashion. Vital contributions to knowledge can and often do have the most unlikely origins, quite unrelated to the prominence or professional affiliations of their proponents. In these ways and others, Hallam Movius fulfilled the expectations one has of an eminent university professor, excelling in both scholarly research and teaching.

IN PREPARING THIS MEMOIR I was aided by information contained in an obituary[14] written by William Howells and Nancy Movius given to me by its authors, a memorial minute[15] prepared by Gordon Willey and others for Harvard's Faculty of Arts and Sciences, a copy of a curriculum vitae prepared by Movius early in his career and supplied to me by Willey, Web-based information on the Movius papers in the Peabody Museum Archives,[16] and by conversations with Nancy Movius. I am very grateful for this invaluable assistance.

NOTES

1. The Palaeolithic or Old Stone Age is a cultural stage recognized by archaeologists. It began about 2.5 million years ago and ended about 10,000 years ago. With this temporal span, the Palaeolithic extended from the end of the Pliocene Epoch to the end of the Pleistocene Epoch of the geologic time scale. The Palaeolithic was followed in some parts of the eastern hemisphere or Old World by a Mesolithic stage, which began at the end of the Pleistocene or Ice Age and ended a few millennia later when farming, the defining characteristic of the Neolithic stage, began in the area in question.

2. Skhul V: background. No date. Available at http://www.peabody.harvard.edu/skhul-bak/background.html. Accessed December 3, 2005.

3. H. de Terra. Preliminary report on recent geological and archaeological discoveries relating to early man in Southeast Asia. *Proc. Natl. Acad. Sci. U. S. A.* 24(1938):407-413.

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5. Legion of merit. No date. Available at http://www.tioh.hqda.pentagon.mil/Awards/LOM1.html. Accessed December 18, 2005.

6. For example, R. Desbrosse. Les civilisations du Paléolithique supérieur dans le Jura méridional et dans les Alpes du Nord. In *La Préhistoire Française*, vol. 1, ed. H. de Lumley, pp. 1196-1213. Paris: Centre National de la Recherche Scientifique, 1976.

7. J. Sackett. Quantitative analysis of Upper Paleolithic stone tools. *Am. Anthropol.* 68(no. 2, pt. 2)(1966):356-394.

8. A. Spaulding. Statistical techniques for the discovery of artifact types. *Am. Antiquity* 18(1953):305-313.

9. L. Vértes. *Tata: Eine Mittelpaläolithische Travertin-Siedlung in Ungarn.* Budapest: Akadémiai Kiadó, 1964.

10. H. Bricker and N. David. *Exacavation of the Abri Pataud, Les Eyzies (Dordogne). The Périgordian VI (Level 3) Assemblage.* Harvard University, Peabody Museum, American School of Prehistoric Research, Bulletin 34, 1984.

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13. B. Delluc and G. Delluc. *Visiter l'abri Pataud.* Luçon: Editions Sud-Ouest, 1998.

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15. G. Willey, J. Brew, H. Bricker, K. Chang, W. Howells, and C. Lamberg-Karlovsky. Hallam Leonard Movius. Memorial Minute adopted by the Faculty of Arts and Sciences, Harvard University, March 8, 1988. *Harvard Univ. Gaz.* 83(May 20, 1988).

16. Peabody Museum Archives. 2001. Movius, Hallam L., Jr. (1907-1987), Papers c. 1931-1969: A finding aid. Available online with a search at http://oasis.harvard.edu. Accessed December 3, 2005.

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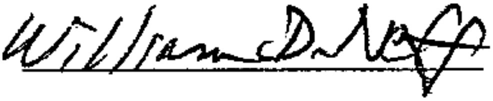

WILLIAM DUWAYNE NEFF

October 27, 1912–May 10, 2002

BY JAY M. GOLDBERG AND NELSON Y.-S. KIANG

William DuWayne ("Dewey") Neff was a plainspoken Midwesterner, who became committed to the study of hearing as an undergraduate and maintained that commitment throughout his career. Best known for his use of the ablation method to study the functions of the various levels of the auditory pathways, he combined behavioral, electrophysiological, and neuroanatomical techniques to define deficits in auditory function more elaborate than simple audiometry. Rather than concocting intricate theories, he was content to follow experimental facts wherever they led. Because of his sound judgment and no-nonsense style, he was asked to provide advice to numerous professional and governmental agencies. His straightforward, nonpedantic approach to science appealed to graduate students. It may very well be that the large number of graduate students that he trained is one of his most enduring legacies. Many of these graduate students went on to distinguished careers and, following in his footsteps, trained many successful scientists.

Dewey Neff was born on October 27, 1912, in Lomax, Illinois, to Lyman Neff and Emma Jacobson. A sister named Jenona had been born a year earlier. Dewey's father, like his own father and brothers, was a skilled carpenter, but for much of his adult life he worked as a lumberyard manager. The family spent the first 10 years of Dewey's life in Lomax, a small town on the Illinois side of the Mississippi River between Burlington, Illinois, and Fort Madison, Iowa. After short stays in Yates City and Brereton, Illinois, and a trip to South Dakota, the family settled in Freeport, Illinois, where the children graduated from the local high school (Jenona in 1929 and Dewey in 1930). In a privately published memoir (L. M. Neff, 1969) their father commented on the fact that the children attended schools in many different towns: "I always thought [this] was good for them. They learned to meet strangers and adapt themselves to changing conditions."

Dewey entered the University of Illinois in 1930 with the intention of becoming an architect. In those depression years he had to work his way through college, including spending one year working full time. As a result, it took him six years to complete his undergraduate education.

THE GRADUATE YEARS

During his undergraduate years, Dewey came under the influence of Elmer A. Culler, a professor of psychology, who persuaded him to continue as a graduate student in experimental psychology. Culler was a well-known physiological psychologist, whose research combined studies of animal learning and hearing. When Culler moved from Illinois to the University of Rochester, he persuaded Dewey to move with him. During his stay at Rochester, Dewey made lasting friendships with fellow graduate students Karl Kryter and J. C. R. Licklider, both of whom went on to distinguished careers of their own.

When Dewey began his graduate work, the dominant question facing hearing researchers was how tonal frequencies were represented in the cochlea and in central auditory pathways. Dewey was intrigued by a report from the Johns Hopkins University neurosurgeon, Walter E. Dandy (Dandy, 1934) on the hearing losses resulting from inadvertent lesions to the auditory nerve when the vestibular nerve was cut to relieve the symptomss of Ménière's disease. Hearing could be impaired at high frequencies, but never at low frequencies.

Dewey realized the potential import of Dandy's findings for the place theory of hearing, in which near-threshold tones of any particular frequency were supposed to activate only a restricted and specific set of auditory nerve fibers. Therefore, the question arose whether, in fact, Dandy had ever sectioned the hypothesized low-frequency fibers. The lack of histological controls prevented the question from being resolved in humans. For his doctoral dissertation, Dewey partially cut the auditory nerve in several cats and, compared their hearing preoperatively and posoperatively. The behavioral testing was based on a method developed by Brogden and Culler (Brogden and Culler, 1936). In addition, cochlear electrophysiology was performed at the time of sacrifice and postmortem histology was examined in collaboration with Prof. E. G. Wever of Princeton University. Dewey's results in cats were similar to those of Dandy's in humans. Partial section of the auditory nerve could produce a selective high-frequency hearing loss, but never one confined to low frequencies. The histology provided a rationale for the behavioral findings. In all animals, nerve fiber degeneration started at the cochlear base, where high frequencies are represented, and extended for varying distances toward the apex, the region most sensitive to low frequencies. There was no instance of restricted apical degeneration. From electrophysiology Dewey concluded that cochlear microphonics, which are now recognized to arise from hair cells, could be spared even in the virtual absence of innervating nerve fibers. World War II delayed full publication of the research until 1947. (Neff, 1947; Wever and Neff, 1947).

Dewey's results left two questions. (1) What would be the consequence of cochlear lesions restricted to the apex? (2) Why did partial sections of the auditory nerve in his experiments never produce selective apical nerve degeneration? In an attempt to answer the first question, Schuknecht and Neff (Schuknecht and Neff, 1952) found that cochlear lesions confined to the apex resulted in modest hearing losses limited to low frequencies. A potential answer to the second question was suggested by Kiang (Kiang, 1979) at a conference organized by Dewey's graduate students and postdoctoral trainees to honor him. Kiang speculated that Dewey's lesions were too medial. As such, they passed through the cochlear nucleus, rather than the auditory nerve and were located so as to sever the high-frequency innervation of the nucleus, but not the more laterally situated low-frequency innervation.

WARTIME

Following completion of his graduate work, Dewey was appointed as a research associate at Swarthmore College, where he collaborated with the famous Gestalt psychologist Wolfgang Köhler. The attack on Pearl Harbor occurred shortly after the start of Dewey's second year at Swarthmore. Within the month Dewey resigned his position and joined the Underwater Sound Laboratories in New London, Connecticut, where he worked on the selection and training of submarine crews (Cronbach and Neff, 1949) and auditory discrimination in sonar detection (Neff and Thurlow, 1949).

THE UNIVERSITY OF CHICAGO

In 1946 Dewey joined the faculty of the Department of Psychology at the University of Chicago. This was an exciting time at the university. A team led by Enrico Fermi had made important contributions to the Manhattan Project, including the first demonstration of a sustained nuclear reaction. Immediately after the war, Edward Teller returned to the University from Los Alamos and Harold Urey, a Nobel laureate in chemistry, came from Columbia University. Paul Weiss, a professor of zoology, was doing pioneering studies in developmental neurobiology, including the first demonstration of axoplasmic flow. Roger Sperry, having already done his limb re-innervation and optic tectum studies in Weiss's laboratory, was returning to the university after a postdoctoral fellowship with Karl Lashley at Orange Park. Ralph W. Gerard was a professor of physiology in whose laboratory pioneering work with microelectrodes was taking place. Kenneth S. Cole, having made fundamental observations on action potentials in the giant squid axon, was appointed a professor of biophysics and was perfecting electronic methods needed to control the voltage and current across the nerve membrane. Heinrich Klüver and Stephen Polyak were located on the top floor of Culver Hall, a musty old building. Klüver was continuing his studies of the behavioral effects of temporal lobe lesions in monkeys—the Klüver-Bucy syndrome—while Polyak, having completed his monumental monograph on the vertebrate retina, was now working on an even more ambitious monograph dealing with the entire visual system.

At the same time, a different breed of graduate student was arriving on campus: war veterans financed by the GI Bill. Compared with graduate students before or since, they were older and more mature, sobered by their war experiences. Many were married and already raising families. They had sublimated their personal agendas to the war effort and now wished to finish graduate school and begin their professional careers.

On arriving at the University of Chicago, Dewey taught a summer course in physiological psychology attended by some of these students. Here Dewey could expound his view that a multidisciplinary approach, then practiced in only a few laboratories, was required to make progress in hearing research and, more generally, in brain research. His own doctoral research had exemplified this perspective, combining behavioral testing, electrophysiology, and neuroanatomy. In addition, Dewey was of the opinion that to understand the functions of higher auditory centers, one had to expand the behavioral testing of animals beyond the measurement of hearing thresholds. Here he was inspired by work in the visual system, especially that done by Lashley and Klüver.

Dewey and the other psychologists doing animal research were assigned laboratory space in a cockroach-infested, two-story, prefabricated wooden structure, officially known as the Laboratory of Physiological Psychology, but commonly referred to as the “Prefabs.” Dewey set up soundproofed experimental chambers to test animal hearing and histological facilities to prepare tissue for microscopic evaluation of lesions. He established an electrophysiological laboratory in space belonging to the Section of Otolaryngology in Billings Hospital. This was part of a continuing collaboration with otolaryngologists in the Surgery Department, including Chief of Otolaryngology John Lindsay, Henry Perlman and Harold Schuknecht (Lindsay et al., 1953; Schuknecht et al., 1951). Dewey’s work with Schuknecht on selective cochlear lesions has already been mentioned and was the start of a lifelong friendship.

As was the case for his mentor, Elmer Culler, much of Dewey's otolaryngology-related research was partly funded through various otological societies. More importantly, Dewey was able to use contacts he had made during his wartime service to secure funding from the Office of Naval Research (ONR). Nowadays most funding for neuroscience research comes from the National Institutes of Health (NIH) or from the National Science Foundation (NSF). The NSF was founded in 1950, and the Extramural Program of the NIH, although started in 1946, became an important source of funding only in the late 1950s. In the early postwar years the Defense Department became a major source of support for university research, and the ONR was a major source of funding for Dewey's work.

Many of the war veterans who were graduate students in psychology chose to do their doctoral research in Dewey's laboratory because he exuded a quiet confidence and displayed a generosity of spirit. He seemed to be a person who knew where he was going and would be happy for others to accompany him. In addition, his multidisciplinary approach to science had an intellectual appeal. With a coherent and, for its time, a well-funded research program, Dewey's laboratory appealed to prospective doctoral candidates as a place where they could successfully learn their craft. As one of his graduate students, Irving Diamond, said at a reunion some 45 years later: "You knew when you came to work in Dewey's lab that you were joining a winning team." As the last of the war veterans finished their graduate studies, Dewey was able to recruit younger, more traditional graduate students. By the time Dewey left the University of Chicago in 1961, he had successfully supervised the doctoral research of over 20 students.

There were usually several graduate students working with Dewey at any one time. Concentrating on ablation studies, Dewey and each student would settle on a thesis topic. It was then the student's responsibility to do the necessary behavioral testing, usually in cats, but in some cases in rhesus monkeys. Audiometric testing was done in a rotating cage, the device devised by Brogden and Culler. For more sophisticated auditory discriminations (e.g., tonal frequency or tonal pattern discriminations) animals were trained in a double-grill box, which required that the animals run between two compartments to avoid shock. Sound localization was studied in a semicircular arena in which the animal was trained, when released from a restraining box, to go to one of two food wells behind which a buzzer had sounded before release. Once an animal had reached a high level of performance, an ablation of some selected part of the auditory system was done under sterile surgical conditions. Dewey usually did the surgery with the student assisting. After a postoperative recovery, typically a few weeks to a month, the animals were retested and, if necessary, retrained to see if they could once again do the discriminative task. Following postoperative testing, an attempt was made to assess the extent of the lesion with evoked-potential methods, after which the animal was sacrificed and the brain was fixed by transcardiac perfusion as the first step in histological processing.

The main topic of interest for Neff and his students was the function of the auditory cortex, although work was also done on lower levels of the auditory pathways. The extent of cortical lesions was based on electrophysiological mapping done by Clinton Woolsey and his colleagues (Woolsey, 1960), among others. A major conclusion of the research was that animals were still able to make simple discriminations after removal of all known auditory cortical areas. This had already been suggested by Harlow Ades and his colleagues, who showed that hearing thresholds (Kryter and Ades, 1943) and intensity differential limens (Raab and Ades, 1946) were unaltered in animals after cortical ablations. Work in Dewey's group showed that frequency discriminations could be relearned after very large cortical removals (Butler et al., 1957), but the ability to perform more complex tasks, including tonal pattern discriminations (Diamond and Neff, 1957; Diamond et al., 1962) and sound localization (Neff et al., 1956) were permanently compromised. Even the simpler tasks, which could presumably be accomplished by lower auditory centers, had to be relearned after the cortical lesions.

The results demonstrated that in the absence of cortical receiving areas, the auditory system still has impressive capabilities. That trained discriminations were lost after cortical removals might imply that cortical processing has a ubiquitous role when the entire system is intact. The definition of that precise role still remains elusive. In addition, even permanent deficits may be related to the behavioral tasks used to demonstrate discriminative ability, rather than to the sensory discrimination per se. For example, later work showed that animals, after cortical removals, are capable of indicating the location of a sound by unlearned orientation head movements (Thompson and Welker, 1963; Poon, 1979) or simple learned responses (Heffner and Masterton, 1975) even though they are unable to walk toward the position of a previously presented sound (Neff et al., 1956; Heffner and Masterton, 1975). It remains unresolved as to whether this last deficit reflects a defect in sensorimotor integration, short-term memory, or attentiveness/distractibility.

Consistent with his belief in a multidisciplinary approach to the neurosciences, Dewey was instrumental in the founding in 1955 of the degree-granting Section of Biopsychology in the Biological Sciences Division. To be enrolled in the Biopsychology Section, graduate students had first to be admitted by the Psychology Department, which was in the Social Sciences Division. Once enrolled, the student's curriculum and research had a large biological sciences component and the Ph.D. was awarded in the Division of Biological Sciences. The philosophy of the program as stated in the university announcements was as follows:

> It is expected that the research [person] trained in biopsychology may . . . devote . . . attention to problems falling between the discipline of psychology and the traditional disciplines, such as physiology, anatomy and zoology. The student is expected, therefore, to become substantially grounded in the physical and biological sciences, in addition to the general field of psychology. Emphasis is placed . . . upon understanding the role of neural and extra-neural mechanisms of the internal environment in the regulation of behavior.

This nascent neurosciences program served as the focus for the training of his graduate students while Dewey remained at Chicago and continued well into the 1990s.

BOSTON

After spending 15 years in Chicago, Dewey was persuaded to move in 1961 to Bolt, Beranek and Newman (BBN), a high-technology company founded in 1948 by two Massachusetts Institute of Technology (MIT) professors in acoustics, Richard Bolt and Leo Beranek, and a former student of theirs, Robert Newman. Originally an acoustical consulting company, BBN obtained many prestigious commissions in architectural acoustics, including the innovative Kresge Auditorium at MIT, the Shed at Tanglewood, and Avery Fisher Hall at Lincoln Center. By the late 1950s the company was looking to diversify. Dewey's graduate student friends Karl Kryter and J. C. R. Licklider had been recruited to the company. Both had changed their interests from their original training in auditory physiology and psychophysics. Kryter had become an expert in the effects of noise on human performance (Kryter, 1970) while Licklider grew interested in the relation between humans and computers and is credited with helping to develop computer networking, which eventually led to the Internet (Waldrop, 2001).

In recruiting Dewey the company had hoped to expand into the field of auditory physiology. Dewey brought with him two of his recent graduate students, Norman Strominger and Philip Nieder. They set up an experimental suite with a surgery, animal facilities, and testing rooms, and began several promising experiments. Unfortunately, at this time the federal government decided not to support basic research at commercial companies. Since Dewey's research in a corporate environment depended on federal funding, it was clear to him that he had to move on. In 1963 he welcomed the opportunity to join the faculty of Indiana University at Bloomington. The next year he was made director of the newly formed Center for Neural Sciences.

INDIANA UNIVERSITY

In his new position Dewey faced two challenges: (1) to build the Center for Neural Sciences and (2) to reestablish his research program. Dewey attracted several people to the center: Jorgen Fex, an auditory physiologist who pioneered the study of auditory efferents, came from the Karolinska Institute after stints in Australia and the National Institutes of Health; Conrad Mueller, a visual scientist best known for his work on Limulus eye, came from Columbia University; Willem van Bergeijk, a physicist interested in the evolution of hearing, arrived from Bell Laboratories; Ilsa Schwartz, an auditory neuroanatomist, came from a postdoctoral fellowship at Albert Einstein Medical School; and Boyd Campbell, a comparative neuroanatomist, moved from Walter Reed Army Medical Center. Through no fault of Dewey's, the center did not flourish. Van Bergeijk met an untimely death. Fex left to become director of the Laboratory of Neuro-otology at the National Institutes of Health. Schwartz left to take an offer at the University of California, Los Angeles. Campbell left to resume clinical training. Of the people Dewey recruited, only Mueller remained at Indiana University.

Dewey did resume his own research. With his first Indiana graduate student, J. I. ("Pete") Casseday, he studied the role of acoustic commissures in sound localization. Building on the graduate research of Colston Nauman Moore, done at Chicago, they were able to show that sound localization was unperturbed by section of the corpus callosum or the commissure of the inferior colliculus, but was impaired by section of the trapezoid body, the main fiber bundle allowing for the integration of binaural inputs in the superior olivary complex (Moore et al., 1974; Casseday and Neff, 1975). Two other graduate students worked with Dewey on studies of relatively simple auditory behaviors. Paul Poon confirmed that animals could indicate the location of a sound by unlearned orientation head movements (Poon, 1979), while C.-K. ("Joseph") Chan defined some of the brain stem pathways involved in various acoustic reflexes (Chan, 1983).

Dewey summarized his own studies on auditory cortex in a brief review (Neff, 1977) and wrote a comprehensive survey of behavioral studies in the auditory system (Neff et al., 1975).

NATIONAL SERVICE AND AWARDS

In addition to his wartime service Dewey served for a year (1953-1954) in London as a scientific liaison officer for the Office of Naval Research. He was a long-standing member of the Committee on Hearing and Bioacoustics of the National Research Council, as well as a member of the Executive Council of the Acoustical Society of America and of the Psychonomics Society. He served on the Editorial Boards of the *Journal of Neurophysiology* and the *Annual Review of Psychology*. Along with Wolf Keidel, he edited volume V ("Auditory System") of the *Handbook of Sensory Physiology*. He was the sole editor of a series, *Contributions to Sensory Physiology*, which published reviews on sensory processing; eight volumes were published between its founding in 1965 and 1983. In recognition of his contributions, Dewey was elected to the National Academy of Sciences in 1964 and the American Academy of Arts and Sciences. He received the Annual Award of the Beltone Institute for Hearing Research and the Award of Merit of the Association for Research in Otolaryngology.

IN SUMMARY

During Dewey's professional career, much of experimental psychology moved from emphasizing speculative doctrines, as exemplified by various learning theories, to obtaining more empirical descriptions of the brain, behavior, and the relation between the two. He and his students never established a school of thought wedded to specific dogmas but concentrated on obtaining rigorous experimental results with proper controls and making limited generalizations. A list of his graduate students at Chicago and Indiana and the titles of their Ph.D. dissertations are included at the end of this memoir, and it will be evident that they moved in many directions, contributing in diverse ways to the neurosciences.

On balance, one of Dewey's most lasting contributions may be his students. He created an environment where young people could find themselves and develop their individual strengths. He was patient, encouraging, and available to members of his laboratory including students, technical staff, and visitors. The comforting atmosphere universally felt by the people who worked with him was directly related to his social skill in providing education under the guise of friendly chats. He was able to attract some of the best students available and they always benefited from his relaxed yet rigorous instructional style. These personal qualities also made him a highly regarded, informed advisor to his peers and to a variety of scientific organizations.

Dewey had a daughter, Carol, and a son, Peter, by his first wife, Ernestine, a professor of English, and a total of five grandchildren. His second wife, Palmer, contributed to auditory research by studying the effects of muscular dystrophy on the middle-ear reflexes. His professional family was large and his influence remains strong on all who were fortunate enough to know him.

DOCTORAL DISSERTATIONS OF STUDENTS TRAINED BY W. D. NEFF

University of Chicago

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Butler, R. A. The role of the auditory cortex in frequency discrimination, 1951.

Diamond, I. T. The function of the auditory cortex: The effect of ablation of the auditory cortex on the discrimination of temporal frequency patterns, 1953.

Gerken, G. M. The evoked electrocortical response to acoustic stimulation and its relation to behavioral conditioning, 1959.

Goldberg, J. M. Frequency discrimination after central nervous system lesions, 1960.

Hahn, J. F. Effects of partial ablations of visual areas I and II on visual form discrimination in the cat, 1952.

Hind, J. E. An electrophysiological study of tonotopic organization in the auditory cortex of the cat, 1952.

Jerison, H. J. Effect of auditory cortex ablation on tone pattern discrimination in the rhesus monkey, 1954.

Kennedy, T. T. K. An electrophysiological study of the auditory projection areas of the cortex in monkey (Macaca mulatta), 1955.

Kiang, N. Y.-S. An electrophysiological study of cat auditory cortex, 1955.

Lindquist, S. E. Stimulation deafness: A study of temporary and permanent hearing losses resulting from exposure to noise and to blast impulses, 1949.

Nauman, C. Sound localization: The role of the commissural pathways of the auditory system of the cat, 1958.

Nieder, P. C. Electrophysiological recording and stimulation studies of the auditory system of unanesthetized cats, 1960.

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DONALD OSCAR PEDERSON

September 30, 1925–December 25, 2004

BY DAVID A. HODGES AND A. RICHARD NEWTON

DONALD O. PEDERSON IS BEST known in the field of electronic design automation for leading the development of a groundbreaking program for integrated-circuit computer simulation called SPICE (Simulation Program with Integrated Circuit Emphasis). Beginning in the 1960s, he carried on this work with colleagues and students at the University of California, Berkeley. SPICE allows engineers to analyze and design complex electronic circuitry with speed and accuracy. Pederson's colleagues point out that virtually every electronic microchip developed anywhere in the world today uses SPICE or one of its derivatives at critical stages during its design.

Donald O. Pederson was born in Hallock, Minnesota (just south of the Canadian border), son of Oscar Jorgan and Beda Emilia Pederson, in 1925. He had one sister, Beatrice, two years older. Don never talked much about his youth, but when he won the Medal of Honor of the Institute of Electrical and Electronics Engineers in 1998, he gave a long interview that was the basis for an article published in IEEE's *Spectrum* magazine (Perry, 1998). Most of the following account of his youth and early career is adapted from that article, with thanks to and by permission of Tekla S. Perry and IEEE.

While still in elementary school in Fergus Falls, Minnesota, Don built his first crystal radio, using parts given to him by an uncle and a cousin as well as junkyard finds. Soon after, he got a paper route and saved his money to buy his first soldering iron and his first vacuum tube. His enthusiasm for electronics was apparent in his high school physics class, in Fargo, North Dakota, where his family had moved. From that class he was recruited for a weekend job repairing electric motors at the Fargo Electric Motor Co.

When Don graduated from high school at age 17 in 1943, in the thick of World War II, he had three months before the U.S. Army would pounce on him for service. Having spent his life so far in Minnesota and North Dakota, he decided to see the West Coast, and went to Seattle. His first stop was a shipyard, where he asked for a job working with electricity. He was sent to the union hall, and officials there offered him a post as an apprentice electrician.

"No," the teenager told the union representative, "I want to be a journeyman. I've been working for two years in electric motor repair, I must have learned something." The union officials gave him an oral test. After he answered the last question, which he recalled had concerned safety precautions in working with hot electrical lines, a listening electrician laughed, muttering, "Well, the kid is wrong on that one." The shop steward corrected him. "No, the kid is right, you're wrong," he said, and gave Pederson his journeyman assignment. He was put in charge of providing temporary electric power, when needed, for lights and tools on a destroyer that was being built.

His knowledge of electrical safety was to be tested further. Often he had to work with live wires, so as not to cast workers in various sections of the destroyer into the dark. "I would get a couple of very dry pieces of wood," he said, "put them on the metal deck, make the break, hold the two ends, then remake the connection. But the guys from a welding crew would sneak up on me and, just when I had broken the line, pour salt water on my feet. The shock would knock me down, and they'd laugh." Pederson said he retaliated by cutting off power to the coworkers when they were in the farthest reaches of the ship; eventually a truce was called.

Just before turning 18, Pederson joined an army training program in engineering. He completed one term at Iowa State University, in Ames, but then the program was terminated and the would-be engineers ended up in the infantry. After combat in Germany, France, and Austria, Pederson was sent to the Philippines. The war ended shortly after he arrived, but he remained there for about a year, taking charge of the regimental power station. "I had two primary missions," he remembered. "The colonel had to have power for his shaver and the troops had to have power for the movie at night."

Pederson returned to the United States and to college in 1946, when he enrolled at North Dakota State University in Fargo. After a day of aptitude tests, a counselor told him that if he wanted to make money, he should forget about college and go to work for a local electric shop, buy into the business, and have a nice life as an electrician. But Pederson hung on to his childhood ambition to be a radio engineer.

After half a semester of college, though, the freshman found himself a C student. That, he concluded, was not going to get him anywhere; clearly, he couldn't have fun and be an engineering student at the same time. Therefore, he worked with a study partner for six days each week, and took an overload of courses to speed through college ahead of other returning veterans. In 1948 he finished his bachelor's degree after two years and one term and, at the urging of a professor, applied to several graduate schools, eventually choosing Stanford University in California.

The late 1940s were an exciting time at Stanford. William Hewlett of soon-to-be Hewlett-Packard fame had just discovered the distributed amplifier, a broadband amplifier used in high-frequency systems. An analysis and redesign of that device was to become Don's thesis project, under the guidance of Professor Joseph Pettit. After earning his Ph.D. in 1952, he stayed on for two years as a postdoctoral researcher at Stanford, designing high-performance electronic amplifiers. The work led to his first professional publications.

Don joined Bell Laboratories, in Murray Hill, New Jersey, in 1953. "Bell Labs had a superb recruiting effort," he recalled. "They would spot the young students who were coming along, then nurture that relationship. It seemed that every time I turned around, there was somebody from Bell Labs in the hallway, so when it was time for me to leave Stanford, it was natural that I would consider Bell." At Bell, he continued working on electronic circuits, switching from tubes to transistors. He doubts he made any major technical breakthroughs during this period, but achieved solid day-to-day development. "I earned my keep," he said.

Soon after he started at Bell Labs, he was contacted by a former North Dakota State professor, Harry Dixon, who had become head of the electrical engineering department at the Newark College of Engineering (now the New Jersey Institute of Technology). Dixon asked Pederson to teach a course on electrical network theory, saying he had committed the school to offering this course in the fall and did not have the knowledge to teach it himself. Despite having decided years earlier that he would never pursue an academic career, and despite the objections of his supervisors at Bell

Labs, Pederson agreed. His former mentor was asking for help, and he felt that he owed it to him.

Preparing for the course and teaching it filled up most of Pederson's nights and weekends, but when the year was over, he concluded that he had enjoyed teaching better even than his work at Bell Labs. He taught another class the next fall, and the following year, 1955, contacted acquaintances back in California and obtained a position as an assistant professor at the University of California, Berkeley.

This was the turning point in Pederson's career, and out of all his accomplishments, he is most proud of his efforts working, he says, with so many "bright, eager students." "He certainly didn't do it for money," commented John Whinnery, one of those who helped to recruit Pederson and now a Berkeley emeritus professor. "He took a big [salary] cut to come to the university. The idea of teaching was what motivated him." "He always could excite students," recalled Ernie Kuh, a Berkeley professor emeritus, who followed Pederson from Bell Labs to California.

In 1959 the integrated circuit (IC) was developed, and the world of electronics changed. According to Don, some engineers thought the IC was merely another way to make amplifiers and switching circuits, but others, including himself, realized that ICs opened up a new world, one in which dramatic reductions in size and cost of electronics would become possible.

Don decided that to undertake research in ICs and to teach students to design them, the university needed its own semiconductor fabrication facility. When he voiced this idea, he met a host of objections: Building a fab was too complicated; his group was made up of engineers, not chemists; the university had no money for expensive fabrication equipment; and the project simply couldn't be done. Ignoring the objections, Pederson, with professors Tom Everhart, Paul Morton, Bob Pepper, and a group of graduate students, started designing the facility. One of us (D.A.H.) was among those students. "Never wait for approval, don't tell anyone you are doing something, just do it," Pederson said later. "That's my motto."

Funds were very limited, but industry was willing to donate used processing equipment as the technology sped ahead to new IC generations. Some key equipment was built locally, with appropriate compromises, recognizing that no mass production was planned. A sympathetic department chair reassigned offices to free up space. A few university grants to young faculty and graduate students, along with some money from the Army Research Office and the U.S. Air Force, provided about $300,000 in cash. In 1962 Pederson announced at a conference of the Institute of Radio Engineers (IRE), the predecessor of the IEEE, that his group had produced its first working circuit. "We stole [the fab] fair and square," he said.

Before the university could consider whether to give the project formal approval, notable engineers from industry were visiting and praising the facility, the first IC fab at a university. "His vision, which gave Berkeley an IC fab way ahead of any other universities, proved to be a key move for the university, for we educated a large number of outstanding students," Berkeley's Kuh said.

"Other universities were arguing at the time that a university can't possibly keep up in the microfabrication field, because you can't afford the most modern facilities," Whinnery, the Berkeley professor who helped to recruit Pederson, said later. "This is true, but Don saw that if you didn't have reasonable facilities, you wouldn't be able to contribute to the field at all. That was one of the farsighted things he did that really paid off. Students that came out of that program became leaders in the semiconductor industry."

Microfabrication capabilities at Berkeley have advanced and grown steadily ever since. Currently, in 2006, several hundred students and faculty members from a wide range of academic fields make use of an extremely flexible research facility.

In the middle 1960s Don became interested in the application of computer programs to the analysis of integrated circuits. He and his students had used a Bendix G15 machine (the very one now displayed in the National Museum of American History at the Smithsonian Institution in Washington, D.C.) with only typewriter and paper tape input and output, to try to gain a deeper understanding of the behavior of certain linear circuit designs. In his circuit design work he found that many of the first-order approaches to the analysis of circuits did not predict the correct behavior of real circuits. In fact, depending on the assumptions made, first-order theory could be used to predict a variety of outcomes for the same circuit.

One of Don's graduates, William Howard, had been working at the U.S. Army's Harry Diamond Laboratory to better understand a particularly tricky problem involving the thermal behavior of the input characteristics of a linear IC design. It was 1967 when Bill Howard first implemented a computer program at Berkeley for the analysis of the nonlinear dc operating point of an IC—he called the program BIAS—on a 16-bit IBM 1130 machine. Don was now convinced that the computer was to play a central role in the design and analysis of integrated electronics. Around 1968 Bill Howard left Berkeley for Motorola, where he eventually retired as a senior vice-president. He was a senior fellow at the National Academy of Engineering from 1987 to 1990.

Many other computer-aided design projects followed at Berkeley, based on techniques developed in Don's laboratory as well as elsewhere (most notably at IBM's San Jose laboratories and North American Rockwell), and included the work of William McCalla and Frank Jenkins—Don identified Frank's potential and brought him into the group as a freshman—leading to programs like SLIC, Frank, and SINC. Bill McCalla, now deceased, made many significant contributions to the CAD industry over the years, including the reworking of the original BIAS program and the integration of nonlinear dc and ac analyses into a single code; Frank Jenkins went on to develop the commercial circuit simulator ASPEC in the late 1970s and some early commercial logic and switch-level simulators, including LOGIS and ILOGS.

In the fall of 1969 the young professor Ron Rohrer returned from a leave at Fairchild and began teaching a course designed to apply modern system and circuit-theoretic concepts and advanced numerical methods to circuit analysis and design. He set about to build the best and most comprehensive circuit simulator he could, assigning various aspects of the task to different students in the class. At the end of the class he told them their grade would be based on how well they had convinced Don Pederson that their contribution was the best that could be done. The outcome of that course was a program called CANCER (Computer Analysis of Circuits, Excluding Radiation), a program that later became the starting point for SPICE 1 (Simulation Program, Integrated Circuit Emphasis) development. A postdoctoral student from Belgium, Hugo De Man, was visiting Berkeley at the time and made his own contributions to the CANCER effort. Professor De Man, of Katholieke Universiteit Leuven and IMEC, both in Belgium, is well known for the many significant contributions he and his group have made to electronic design automation.

In the fall of 1970 Don selected CANCER for his classroom instructional programs, rejecting the other Berkeley competitors of that time, SLIC and SINC. Unfortunately, Don's ability to distribute the CANCER code to his friends and colleagues in industry was hampered by the fact that the program had been declared proprietary. Because much of the IC design and development work was going on in industry, Don felt that this was an unacceptable barrier. A young graduate student named Larry Nagel had been closely involved in the CANCER project and when his adviser Ron Rohrer left Berkeley, Don took him on as a graduate student on the condition that he could use the CANCER source as a starting point for a truly public-domain, general-purpose circuit simulator. In May 1972 the first version of that new program, SPICE 1, was released from Berkeley. Larry Nagel continued his work with SPICE, releasing SPICE 2A.0 before joining AT&T Bell Laboratories, where he led the AT&T in-house circuit simulation development efforts for many years.

A freshman that Don identified and recruited from one of his classes, Ellis Cohen, picked up the SPICE baton from Larry and carried SPICE 2 forward, making significant contributions even as an undergraduate. Ellis was quickly recognized as a superbly talented computer scientist. Much of what became the version of SPICE 2 that formed the basis of the many commercial versions should be attributed to Ellis.

A.R.Newton (one of the authors) joined Don's group at Berkeley in early 1975. Don recruited him from the University of Melbourne after he had already been effectively working for Pederson there for two years, in both undergraduate and M.S. programs. Newton contributed to SPICE as well. Newton remembers one day when Ellis Cohen had just added dynamic memory management to SPICE 2. (Note that SPICE 2 was a Fortran program in punched-card form and ran on a CDC 6400 computer, whose only output device was a 132-column line printer). Adding dynamic memory management was no small feat. But it slowed the program down by almost a factor of two. This was not acceptable to Don and, in his own way, he challenged Ellis to fix the problem. Within 24 hours Ellis had designed and repunched the cards needed to add automatic machine-code generation to the program—where SPICE itself generated the native object code needed to solve the sparse-matrix circuit equations for that particular circuit, rather than using the more general code produced by the Fortran compiler. He had debugged and installed the program and, since he managed to get back the factor of two as a result, he was able to keep his dynamic memory management. Ellis Cohen now is with Mentor Graphics Corp. in Oregon.

The Army Research Office (ARO) also played a critical role in the development of SPICE. In these days of tight budgets and congressional pressures, many agencies that fund university research are being asked to show increasingly shorter-term payoffs, force technology transfer to industry, and reduce their long-term commitments to research programs. It is clear that the long-term research funding commitment ARO made to Don's effort was instrumental in giving him the flexibility to continue his work to completion over many years. There were a number of occasions when the ultimate pay-off of the work was questioned, when the path was not perhaps as clear to those outside the SPICE group, but the ARO support continued. The SPICE 2 work continued until the early 1980s, when the program was converted to the C language, new models added, and some analysis techniques generalized, resulting in SPICE 3.

Don and his students have made contributions to electronic design automation (EDA) in many other areas including device modeling, mixed-mode simulation, rule-based circuit diagnosis, and macromodels. In fact, his analog macromodel for the operational amplifier, developed in conjunction with Jim Solomon and Graeme Boyle in the 1970s, is still a standard today. Jim Solomon, formerly of National Semiconductor Corp. and founder of SDA Systems (now Cadence Design Systems) received his M.S. degree under Don's supervision at Berkeley; Graeme Boyle received his Ph.D. with Don before joining Tektronix.

Many other leaders in our field worked as graduate students with Don Pederson. They include Gary Baldwin, the former laboratory director at Hewlett-Packard Labs; Professor Mohammed Ghausi, former dean of engineering at the University of California, Davis; Professor Gary Hachtel, University of Colorado; Robert Pepper, founder and CEO of Level 1 Communications (acquired by Intel); and Professor Bruce Wooley, Stanford. Almost every major design technology company of today has been influenced significantly by at least one of Don Pederson's former graduate students.

The industrial impact of Pederson's early work in electronic design automation is best measured by the use of the technology he and his colleagues developed over a quarter century ago. As mentioned in the introduction, virtually every semiconductor company and the vast majority of electronic system design companies throughout the world use a version of SPICE, or a program derived directly from it. In addition to companies who have obtained SPICE from Berkeley and who have adapted it for their own in-house use, over the past 20 years more than a dozen companies have been formed based around SPICE versions. In almost every case the basic architecture of the program, as originally designed by Don Pederson and his students, is still used and in most cases, much of the original code is still present. Not only has Don's pioneering work been of vital importance to the semiconductor industry, it also has contributed significantly to the formation of a significant, new industrial base—integrated circuit CAD companies.

Beyond the clear commercial impact of the SPICE effort, one cannot overlook the impact this contribution has had on engineering education. In fact, the first versions of SPICE, in use at Berkeley in May 1972, were intended to augment instructional laboratories. Brian Preas noted that he had counted 12 textbooks and monographs at the Stanford University student bookstore that contained the word "SPICE" in the title, such as "Integrated Circuit Design Using SPICE." We concur when he says that he cannot think of a significant undergraduate electrical engineering instructional program anywhere in the world today that does not use the SPICE program as an integral part of its curriculum. No other electronic design automation tool or technology has had such a broad educational impact.

While there were numerous important scientific contributions in the original SPICE 1 and SPICE 2 programs, the success of this technology cannot be attributed to science alone. In fact, even the original developers of SPICE admit that there exist "better" algorithms than those used in SPICE, when considered separately. It is the *engineering* contribution—the way the algorithms and ideas were combined—and the unique, open relationship Don Pederson and his students established with industry that have resulted in a family of programs that have lasted almost intact and without significant competition for over a third of a century. Many programs have been developed for specific technologies, or which produce approximate results (using simpler models and algorithms) and so can run faster than SPICE on larger circuits, but for general purpose, robust circuit simulation, the user community seems always to fall back to the trusted and well-tested SPICE-based solution.

At the time the SPICE program was being developed, there were similar programs under development in major electronics companies worldwide. These programs used precursors to many of the techniques present in SPICE. A number of the early innovations in circuit simulation were developed as company proprietary. However, the original vision of Pederson's SPICE program development team was to put together the best combination of algorithms, to code them in as flexible and portable a style as possible, and to make them freely available. The only restriction placed on users was that they should never charge any third party for the SPICE program itself; Pederson considered it a public-domain resource. As a result, new ideas and contributions to SPICE flowed from many sources, both in universities and in industry.

Don always saw the circuit simulator as a means to an end, not as an end in itself. His attention was always directed to the solution of circuit design problems, both through heavy classroom use and in research; the simulator was developed as the best way to achieve that goal. It was the process used to develop the technology, as much as the technology itself, that represented the great insight Don Pederson contributed to the steps needed to develop and transfer the knowledge embodied in engineering research. In fact, Berkeley has continued to use this model, pioneered by Pederson, for its ongoing research in other electronic design automation areas. Other universities (for example, MIT and Carnegie Mellon University) follow similar models for their interaction with industry, based in part on the early Berkeley success.

Don Pederson died on December 25, 2004, aged 79, of complications from Parkinson's disease. He is survived by

Karen, his wife of 27 years; four children from his first marriage (to Claire Nunan): son John, daughters Katharine Rookard, Margaret Stanfield, and Emily Sanders; and four grandsons, all in California.

He was elected to membership in the National Academy of Engineering in 1974 and the National Academy of Sciences in 1982. He garnered numerous other honors and awards, including a Guggenheim Fellowship in 1968, an American Association for the Advancement of Science Fellowship in 1988, the Berkeley Citation in 1991, the Phil Kaufman Award from the Electronic Design Automation Consortium in 1995, and the Medal of Honor from the Institute of Electrical and Electronics Engineers in 1998. He also received an honorary doctorate from Katholieke Universiteit Leuven in Belgium.

The narrative above fails to do full justice to the Don Pederson so greatly loved by generations of students and close colleagues. Bill Howard, a former student already mentioned above, eloquently articulated an image of the man we knew so well in comments prepared for Don's memorial service (Howard, 2005), presented below in mildly edited form.

Don Pederson (or DOP, as he is affectionately known by students) was the guiding influence in my professional career and in many other dimensions of my life. Knowing many of his other students, I know the feeling to be universal. On the most elementary level, Don embodied the ideal of an engineer as one who uses science to achieve useful results. He practiced our profession with a spirit and enthusiasm that coupled all his and his students' efforts with his dedication. His face was a mirror reflecting all his zest, surmounted by a pair of expressive, brushy eyebrows that punctuated every thought.

He was an inspired teacher. All who studied circuits with Don knew that he held the Guinness World's Record for drawing a single transistor amplifier stage on the blackboard. His diagrammatic artistry was faster than the circuit itself could switch. After taking electronics from Professor Pederson you knew you had triumphed over the most challenging intellectual marathon in the field. His students were infected with his exuberance for whatever he was teaching. That, alone, makes Don an inspired educator, but he was much, much more.

Don was one of the most innovative people it has been my joy to know. When he and Bob Pepper established the Berkeley Integrated Circuits Lab, they led the world. Many said ICs couldn't be done in university environments. Many said it was too expensive. And many said it was too advanced for university work. Many were proven wrong. Berkeley has been the leader in this area for nearly forty years, based on Don and his team's foundation.

Don's imagination did not prevent him from having strongly held views. This applied to his technical work as well as to wine. When confronted with a circuit thermal drift problem I could not solve, I was compelled to consult Don for help. I informed him it was necessary to use computer simulation to find out what was going on, and was told, in his own inimitable way and in no uncertain terms, that "any problem worth solving can be solved on the back on an envelope." Don was the master at practical circuit engineering. We each agreed to take our own approach to see who could get to the solution first. When, after spending many nights writing a program that revealed the source of the drift, I went back to Don. I found him in his office, with his board covered with calculations and diagrams (and a lot of filled envelope backs) and no answer. Many at that point would have become defensive. Don looked at the computer results, went "humpf," and returned the next morning full of ideas on how to use circuit simulation in his electronics teaching and research. As we all know, he soon became the godfather of IC CAD and changed the way we all work. Don's and his students' creation, SPICE, is now the gold standard in IC design.

Don's innovativeness applied to every aspect of his work and life. Once he decided he was tired of trudging through the same subject material every semester and devised the idea of having his sophomore students read up on class material beforehand. He would come to class prepared to answer questions; if there were none, he would tell jokes. The first few weeks of the term were hilarious! After the first exam, things rapidly became very serious; his students really learned to be prepared and many developed the skills of independent research early.

Don's vitality was ubiquitous. Anyone who accompanied him on his lunch-time "safaris" (many of which entailed walking three or four miles), could not help but be affected by his enthusiasm, spirit and curiosity. When the two of us went to a Japanese woodworking shop, he became so intrigued with the novel tools that we almost did not get back home for the dinner party he and Karen were having (thereby earning a stern rebuke).

Although he professed to hate it, Don's talent for management was immense. His stints as Electronics Research Laboratory Director and Department Chair left Berkeley in superb shape. His skill at assembling a team of outstanding colleagues is evidence by their subsequent roles in the University.

It was Don's personal touch, however, that is my most treasured memory. He was always there to listen to his students' problems. His advice was always sound, well grounded, and definitive. As a young faculty member, he provided support when times seemed tough and when the road ahead seemed lost in a fog of uncertainty—the ideal mentor. He (together with Karen) was the best man at Kathy's and my wedding. He provided the most memorable moment of the ceremony: as Kathy started down the aisle, Don leaned over and whispered in my ear, "Look at her in all her beauty—and remember this moment all your married years.

Thank you, Don Pederson, for everything you mean to all of us. We will treasure having known you, been guided and taught by you, being shaped by you and inspired by you. And I can't suppress a grin at the image of Don at the Pearly Gates, correcting Saint Peter's great book by labeling each dangling participle with a red "dp," as he used to do for all of us.

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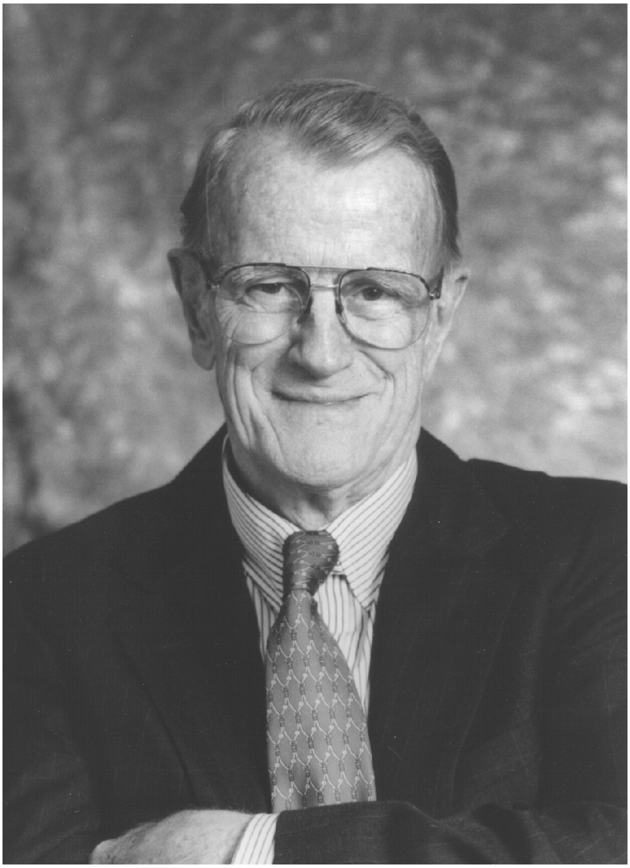

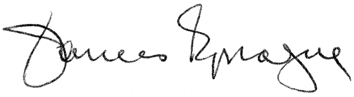

JAMES MATHER SPRAGUE

August 31, 1916–December 22, 2002

BY ALAN C. ROSENQUIST AND S. MURRAY SHERMAN

JAMES MATHER ("JIM") SPRAGUE, THE Joseph Leidy Emeritus Professor of Cell and Developmental Biology at the University of Pennsylvania died from leukemia on December 22, 2002, at the age of 86. Jim is survived by his wife of 43 years, Dolores, and a son, also Jim, who is a pediatric ophthalmologist. Jim Sprague was one of the pioneers in the study of the anatomy, physiology, and functions of the brain, and he was a member of the Founding Council of the Society for Neuroscience in 1970. He was elected to membership in the National Academy of Sciences in 1984.

Jim began his scientific career very early in life, largely because of his privileged upbringing. He was born into an old and wealthy New England family translocated to Kansas. His family owned a summer cottage on Mackinac Island, and this afforded Jim a wonderful base for exploring nature. He fell in love with the prospect of becoming a naturalist, and this interest helped him to develop the sort of wide-ranging, questioning mind that matured into the successful neuroscientist that Jim became. The Great Depression hit Jim's family hard, ending the bucolic, idyllic summer stays on Mackinac Island, but Jim continued his study of nature through the Boy Scouts and other public opportunities. He "tramped" the Missouri River bottoms, the Missouri Ozarks, and the Colorado Rockies seeking the habitats of birds and mammals. This experience undoubtedly sharpened his observation skills, which served him well throughout his career.

Because of his family's struggles during the Great Depression, Jim had to get a job when jobs were extremely difficult to find. He found work as an elevator operator and janitor in an office building, earning about 30¢ per hour. But he already had ambitions to become an academic zoologist, which required an extended education. Jim finished high school with a record that he described as mediocre. Entering Kansas City Junior College in 1934, he then had to solve the problem of how to manage his education and his job simultaneously. His employer was sympathetic, allowing him to change his hours to half-time, and he secured a loan from an uncle to make up for the lost wages. His schedule was daunting. He attended classes in the mornings, worked in the afternoons, and studied in the evenings. Jim described those days as very fortunate for his continued education, because his teachers at Kansas City Junior College, while perhaps not qualified for university positions, turned out to be wonderful educators, and provided a necessary bridge in his education toward an academic career.

At this time a new building was being constructed in the center of town and during the excavation, fossils were found in large numbers. Jim petitioned the contractor and received permission to go into the pit and carry out a backpack of fossils. He identified these fossils of pelecypods, brachypods, crinoids, and ferns using library books, and he placed them carefully on shelves in his room next to his collection of American Indian artifacts, bird nests, animal skulls, and minerals.

Upon completion of his two years in junior college, good luck struck Jim in the form of two wealthy, powerful, and generous family friends who secured a job for him at the Natural History Museum in Kansas City and provided tuition for him to attend the University of Kansas. Again he faced a grueling schedule, having to find time both to be a productive university student and to do his work at the museum. His job entailed field work to obtain new fossils for the museum collection and teaching various courses in comparative anatomy, evolution, and ecology. Some of Jim's colleagues at the museum, like Jim, went on to distinguished careers in comparative zoology and paleontology.

After four years Jim had earned baccalaureate and master's degrees in zoology, the latter under the supervision of Edward Taylor. Jim's thesis was a study of the rodent hyoid bone that attaches to the base of the skull and supports the tongue and pharynx. His description of his methodology for this work is quite revealing: Starting with rodents trapped during his various field trips, he skinned them, treated them chemically, and placed the carcasses in boxes with dermestid beetles, insects that devoured the soft parts of the carcasses, leaving complete, articulated skeletons, including the delicate hyoid bone complex. This work acquainted Jim with the writings of some of the great European comparative anatomists and led to his desire to pursue the doctorate.

Jim then turned his attention to further his education with a Ph.D., and one of his chief targets was Harvard University, largely because of the presence there of the noted vertebrate paleontologist Alfred Romer. Again, his family stepped in and supported a visit to Harvard, which marked the beginning of Jim's transformation from a Midwesterner to an Easterner. Jim interviewed with Professor Romer at Harvard in 1940, and this interview reflected the remarkably good fortune that characterized Jim's career and allowed him to overcome so many obstacles. Professor Romer greeted Jim "by chanting with full body participation" the football cry of the University of Kansas. It seems that Jim and his future mentor were both proud Kansas alumni, and this happy coincidence cemented a relationship that had much to do with Professor Romer's offer of a position at Harvard and an opportunity for Jim's Ph.D.

Jim initially intended to train himself as a future museum curator, and Professor Romer was an early model for him. As it happened, Harvard's Museum of Comparative Zoology had an extensive collection of "pickled" bats, to which Jim was quickly introduced. Not surprisingly, he chose for his Ph.D. dissertation to study their hyoid structure, culminating in a scholarly thesis in which this was carefully and thoroughly described for 39 species of 32 genera of bats. Jim received his Ph.D. in 1942: his thesis was published in the *American Journal of Anatomy* in 1943.

His plans were to continue on the track as a museum curator, and Professor Romer arranged for Jim to get a position at the Field Museum in Chicago. At this point the actual career trajectory that led Jim to become such an important figure in experimental neuroscience seemed far fetched, but fate intervened. World War II imposed itself on Jim's career plans. His sought-after post in Chicago was never realized, and Jim was instead drawn toward the general field of medicine because of the perceived national need for more physicians during wartime.

Jim decided to be trained to teach medical students and after graduation took a course at Harvard involving dissection of the human body. This gave him the bare rudiments required to teach human anatomy to first-year medical students; with this rather limited training he was able to secure a medical teaching post at Johns Hopkins University.

As it happened, Jim's home at Johns Hopkins was the Department of Anatomy, a place where the study of neuroanatomy was heavily emphasized. This afforded Jim his first real exposure to neuroscience; as they say, the rest is history.

Jim's colleagues at Johns Hopkins included some of the great neuroscientists of the day: Bill Strauss, Marion Hines, Louis Flexner, Vernon Mountcastle, Jerzy Rose, Reginald Bromiley, and Clinton Woolsey. Soon after arriving at Johns Hopkins, Jim developed his deep fascination with the brain that was to endure for the remainder of his days. Experimental approaches were new to Jim, however, and he suffered several unproductive forays into experimental problems of the brain. Then he found a practical problem worthy of his talents: He successfully mapped the locations in the primate spinal gray matter of the motor neurons that innervated the myotonic or lateral plate muscles. This arduous task was completed by cutting the dorsal or ventral rami and noting the locations of chromatolytic neurons. These were the days before the advent of the sensitive retrograde tracers that are in use today.

Jim then developed a series of collaborative arrangements that furthered his breadth and competence in neuroscience. Many of these were with distinguished neuroscientists at other institutions, which showed Jim's ability to network; for instance, Jim worked with Professor Donald Barron of Yale on a project to describe the development of the sheep spinal cord. He then arranged a collaboration with Professor Horace Magoun of Northwestern University, which led to spending much of the spring of 1948 in Chicago working with Magoun on the neurophysiology of the reticulospinal control of stretch reflexes.

Later in 1948, with a Guggenheim Fellowship in hand, Jim boarded the *Mauritania* for a journey that led him to the United Kingdom and to both Oxford and Cambridge Universities. At Oxford under the direction of Sir W. E. Le Gros Clark, he learned the Glees silver technique for staining degenerating axoplasm and applied it to the study of hippocampal connections in the rabbit. At Cambridge in the physiology laboratory of Bryan Matthews, he was surprised to learn that investigators were expected to do everything for themselves and that very few general facilities were available. Jim began, along with Michael Fourtes, by building an amplifier that he described as a "pile of junk" but one that worked! While in the U.K., Jim also had the pleasure of visiting with Lord Adrian and Sir Charles Sherrington.

Jim returned to Johns Hopkins University for only one year, a year that was in many ways frustrating for him. His home department had new leadership that Jim found less than supportive, and he discovered that the very promising Glees technique that worked so well in Oxford failed to work at all in Baltimore. Undaunted, Jim reverted to old standby techniques of retrograde chromatolysis and Marchi degeneration to tackle his next problem, the anatomical location of the cells of origin and axonal course of the ventral spinocerebellar tract.

In 1950 he eagerly accepted a position at the University of Pennsylvania. Here under the leadership of Dr. William Windle and in collaboration with Bill Chambers and John Liu, Jim continued his studies of the spinocerebellar tracts and the structure and function of the cerebellum. Using the newly devised silver degeneration techniques of Walle Nauta and his collaborators, Jim, Chambers, and Liu expanded on the earlier studies of Jan Jansen and Alf Brodal on the efferent projections of the cerebellar cortex and deep nuclei. This work showed that there were three different systems of cerebellar output, organized in mediolateral "zones," and this naturally resulted in Jim pondering the question of function: What is the functional significance of these three systems? The pursuit of this question led to an approach that marked much of the remainder of Jim's career: testing structure and function by evaluating the behavioral deficits associated with specific brain lesions.

To address the functional questions concerning the cerebellum, Jim and Chambers placed cerebellar lesions or stimulating electrodes into each of the three mediolateral cerebellar zones of the cat, and showed that the vermis and fastigal nucleus are involved with gross postural tone, equilibrium, and locomotion of the entire body. They further showed that the intermediate zone is involved with skilled movements and tone of the ipsilateral limbs, and that the lateral zone (lateral cerebellar cortex and dentate nucleus) is involved in skilled movements of the ipsilateral limbs but without effects upon posture and tone. These were seminal studies of the functional organization of the cerebellum that have largely stood the test of time.

In collaboration with John Liu, Bill Chambers, Eliot Stellar, and postdoctoral fellows Tom Meikle, Mel Levitt, and Ken Robson in the 1950s, Jim undertook to amplify the work of Moruzzi and Magoun on the functions of the brainstem reticular activating system (RAS). Earlier work was limited to acute descriptions of lesion effects, and Jim and his collaborators extended these studies by studying the long-term effects of brainstem lesions, employing a large battery of behavioral tests. These studies contributed to a much better understanding of the roles of RAS and direct sensory pathways to attentive, adaptive, and affective behaviors than the short-term studies alone. In the course of these studies Jim noted that lesions placed below the superior colliculus that interrupted collicular afferents and efferents had caused unexpected visual deficits that included visual neglect. He hypothesized that these attentional and other deficits involved the superior colliculus. It is for this work and much subsequent work on the roles of cortical and collicular pathways in visual functions that Jim is most remembered.

Nonetheless, before committing to studying visual pathways, Jim was involved in one last, important study of spinal circuitry. The background to this was a controversy as to whether the 1a dorsal root afferents made monosynaptic, inhibitory connections onto ipsilateral antagonist muscle motoneurons or whether they affected their inhibition on these motoneurons via local, inhibitory interneurons. The importance of this question is linked to a key hypothesis that still endures: a single neuron must produce the same transmitter(s) at all of its presynaptic terminals. That is, there was already strong evidence that 1a afferents monosynaptically excited ipsilateral motoneurons, and for the same axons to inhibit contralateral motoneurons would seem a violation of this hypothesis. (We now know that a single axon can inhibit some target neurons and excite others, but this is via different postsynaptic receptors activated by the same neurotransmitter.)

Jim's first attempt to determine the projections of these 1a afferents was the result of yet another collaboration that took Jim to the Rockefeller Institute for Medical Research (now Rockefeller University) in New York City to work with David Lloyd. This occurred during a sabbatical in 1955. This project was purely anatomical and produced ambiguous results regarding the main question. Undaunted, Jim then teamed up with Karl Frank a few years later to reinvestigate the problem using physiological techniques of intracellular recording of motoneurons and latency analysis of EPSPs elicited by 1a afferent stimulation. They found that the contralateral pathway had a longer latency consistent with an extra synaptic delay; they thus concluded that the 1a inhibition of contralateral motoneurons was disynaptic and involved an inhibitory interneuron.

At the beginning of the 1960s Jim, in reanalyzing his lesion studies of the brainstem, began to recognize a relationship between lesions involving the superior colliculus and vision disorders. He decided to follow this up. It is relevant to note that at the time, when vision research was coming under the domination of David Hubel and Torsten Wiesel at Harvard University, the field had a decided cortical bias; this led to the prevailing view that any important visual capacity must be cortical in nature and not, for instance, involve subcortical structures, such as the superior colliculus, for any but the most mundane reflex-like functions.

Jim began by making various lesions of the superior colliculus, with the general thread that these interfered with detecting and orienting to objects, and again this view challenged the cortical chauvinism of the day (that persists still!). Then, in 1966 Jim published a seminal paper in *Science* that described a remarkable visual recovery phenomenon in the cat that has since been called the “Sprague effect.” Jim had shown that a large unilateral visual cortical lesion produces an enduring hemianopia (i.e., blindness in half the visual field) in the side opposite to the lesion. This by itself was an old story and part of the lore that elevated cortex to a prominent, unique role in vision. However, when the superior colliculus contralateral to the cortical lesion was ablated or when the commissure between the two colliculi was transected, there followed a dramatic recovery of the cat’s visual orienting ability to visual stimuli presented in the previously blind hemifield. Later studies showed that this restored visual capability was subserved by the remaining superior colliculus, ipsilateral to the original cortical lesion. This remarkable observation should serve as a red flag to the interpretation of all lesion studies, since in this case a second lesion partly ameliorated the effects of a first lesion, perhaps because any lesion, in addition to directly removing neuronal circuitry, may have widespread secondary effects on other, apparently intact neuronal structures. It is the depression of these secondary structures that leads to the lesion-evoked impairment. Thus the structure/function relationship from lesion studies can be misinterpreted.

Indeed, Jim's interpretation of the Sprague effect is as follows. There is a large ipsilateral projection from the visual cortex to the superior colliculus, and the result of the first cortical lesion removes this input, leaving a depressed colliculus; this depression is largely subserved by the remaining fibers coursing through the collicular commissure, and the second lesion of the other colliculus or transaction of the commissure removes this depressing input, releasing the untouched colliculus for action. It should be noted that the visual function subserved by the remaining colliculus is done so by a wounded colliculus, since many of its normal inputs are removed, suggesting that in the normal animal the colliculus may subserve even more visual functions that are much more than vestigial reflex functions.

Jim continued his involvement in the Sprague effect into the 1990s in collaboration with Alan Rosenquist and Steve Wallace at the University of Pennsylvania. Together they showed that the crossed inhibitory connections to the colliculus arose from the substantia nigra, pars reticulata. The mechanism underlying the Sprague effect has since been further elaborated by Rosenquist and his collaborators. Our current understanding is best summarized by Jim in his autobiography published by the Society for Neuroscience:

The mechanism appears to work as follows. Visual input from the retina reaches extrastriate cortex, which projects to the striatum and there activates a striatonigral path (using glutamate), which terminates in the substantia nigra, pars reticulata. This system (using GABA) exerts a controlling influence on nigral neurons which project to the superior colliculus by way of a nigrotectal tract. The nigrotectal path is a tonically active GABAergic tract that suppresses firing of the orienting neurons in the colliculus; these nigral neurons are phasically inhibited by GABAergic activity in the striatonigral path, thus releasing the colliculus to trigger contralateral orienting responses.

In 1966 Jim took a sabbatical to work at the Institute of Physiology in Pisa with Giovanni Berlucchi, a young protégé of the director, Giuseppe Moruzzi, who had worked as a young man with Magoun at Northwestern. At Pisa Sprague and Berlucchi began a warm and lasting friendship and a decades-long collaboration aimed at understanding the roles of cortical and midbrain visual areas in visual form and pattern discrimination and interhemispheric transfer. They used a split-brain approach, making a combination of cortical and midbrain lesions differing on each side, to maximize information from each cat. These experiments, which led to a string of research publications, established an unexpected role for the midbrain in pattern vision.

While in Italy, Jim also collaborated with Giacomo Rizzolatti and Lorenzo Marchiafava in conducting some of the earliest single-cell recordings of the feline superior colliculus.

Jim's longstanding interest in cat visual psychophysics stemmed from his collaborations with Mark Berkley at Florida State, which began in 1972. From 1984 to 1995 this interest took the form of a rich and fulfilling collaboration with Guy Orban, Erik Vandenbusshe, and others at the University of Leuven, Belgium. Jim loved to visit Leuven and did so twice annually for many years. He and Dolores especially liked living in the beautiful facility (the Begijnof) owned by the University of Leuven.

Jim will long be remembered for his important contributions to a wide range of biological and neuroscience areas. His work on the cerebellum, spinal cord, brainstem reticular formation, superior colliculus, and the multiple visual cortical areas and pathways will remain his legacy and seminal contribution to the field of neuroscience. Jim will also be remembered for his contributions to the University of Pennsylvania as one of the founders and as director of the Institute of Neurological Sciences (1973-1980). He also served as chair of the Department of Anatomy (now the Department of Cell and Molecular Biology) from 1968 to 1975.

Both authors of this memoir were students and later colleagues of Jim Sprague and both of us greatly lament his loss of a role model, mentor, and close personal friend. He will be missed but never forgotten by us, or by the hundreds of younger neuroscientists who will continue to amplify and extend the discoveries that are his legacy.

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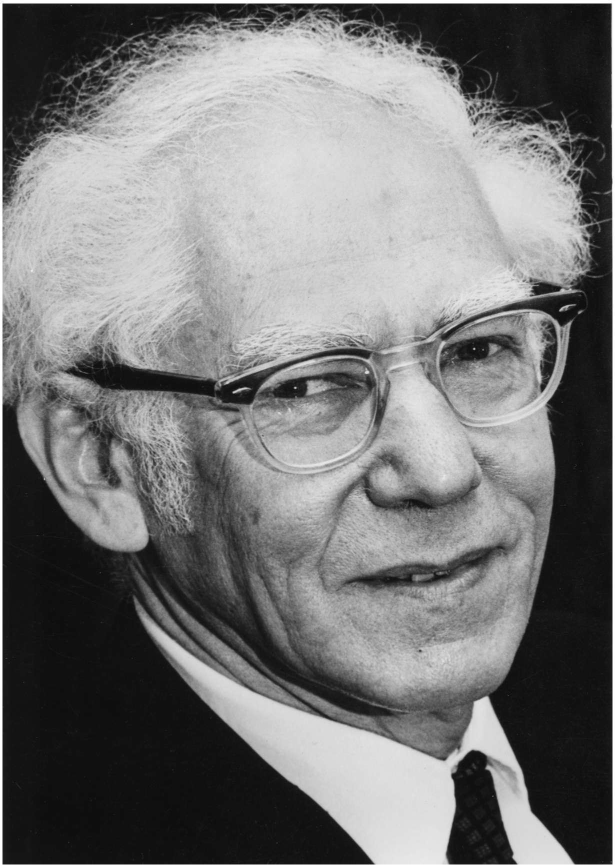

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Owsei Temkin

OWSEI TEMKIN

October 6, 1902–July 18, 2002

BY SAMUEL H. GREENBLATT

OWSEI TEMKIN WAS NOT A scientist in the ordinary sense of one who works at the benchtop, in the field, or with theoretical models. Rather, as a physician-historian he spent a long and productive lifetime studying how medicine and science develop and interact with the cultures that harbor them. His election to the National Academy of Sciences in 1978, 10 years after his formal retirement, was indicative of the recognition that he had achieved for this effort. It also signified the immense respect that he commanded in the scholarly world by virtue of his extraordinary knowledge of languages and historical cultures, the depth of his analyses, and his gentle but firm modesty. Indeed, gentleness and modesty were his personal hallmarks, but his modesty was not false in any way. He understood his own intellectual powers and the place they gave him in society, but he abhorred self-promotion, mainly because it was inconsistent with dispassionate scholarship.

The timing of Temkin's election to the National Academy of Sciences was paradigmatic of the way he achieved recognition for his work—late but in nearly full measure. Since he worked prodigiously but quietly, the size and quality of his contribution became apparent rather slowly to the world outside of his immediate circle. In the end, however, the work spoke for itself and for its author.

Temkin was born in Minsk, Belarus (then part of Russia), on October 6, 1902, the son of Samuel and Anna (Raskin) Temkin. In 1905 his Jewish family moved to Leipzig, Germany, to avoid pogroms. There he had his elementary schooling and attended the *Real-Gymnasium*. After the Russian revolution of 1917, his family lost its Russian citizenship. The young scholar felt the sting of being an alien in German society, but he also benefited from the residual rigor of the German educational system, which still remained partially intact after the disaster of World War I. In 1922 he enrolled at the University of Leipzig:

> I was asked to state my field of study. "Medicine and philosophy," I said. My reply was not acceptable; only one school (*Facultät*) could be chosen, and so I declared for medicine. It would satisfy my interest in science, particularly human biology, while eventually enabling me to make a living in a useful manner. . . As an alien, I could not count on becoming a teacher at a Gymnasium or university, and there was no other possibility of supporting myself in philosophy or history, which had also attracted me since boyhood. (1977, p. 3)

Although its shape evolved, Temkin's interest in philosophy endured. This interest informed his approach to history for the rest of his life.

Fortunately, Leipzig had the preeminent Institute for the History of Medicine. In the fall of 1925 Temkin attended the survey course offered by the newly arrived director, Henry E. Sigerist (1891-1957). When Sigerist talked about the Hippocratic concept of disease, Temkin's philosophical predilections were aroused. He became Sigerist's pupil and wrote his thesis for the M.D. (an advanced research degree) on the subject. After he received his M.D.

in 1927, Temkin spent a year as an intern at St. Jacob Hospital in Leipzig. He then returned to the institute as an *Assistent* from 1928 to 1932. By returning he took his place in a historiographic tradition that continues to this day, albeit in Baltimore rather than Leipzig. The tradition began with the founding of the Leipzig institute in 1905, when Karl Sudhoff became its first director. After Sudhoff's retirement, "the first four years of [Sigerist's] directorship . . . coincided with the few good years of the Weimar republic" (1977, p. 6). Temkin thus rejoined a small but lively group of like-minded scholars in a place where the new director engendered a heady milieu that was even tinged with a little philosophic romanticism.

Speaking of romanticism, the timing of Temkin's return to the institute was also most fortunate in the personal sense. At a costume ball that Sigerist organized "for the very staid association of the professors of the Leipzig University" (1977, p. 8), Temkin met a gracious young English woman, Clarice Lilian Shelley (1906-1992), who was working on her M.A. thesis in German at the University of Wales. They were married on July 15, 1932. Their daughters, Ann and Judith, were born in the United States. Mrs. Temkin was her husband's adviser, editor, and scholarly colleague until her incapacitation from Parkinson's disease. She died in 1992.

In 1931 Temkin became *Privatdozent* for the history of medicine at Leipzig. In 1932 he followed Sigerist to the recently founded Institute of the History of Medicine at Johns Hopkins University. Both young men—Sigerist and Temkin—were recruited by the legendary William H. Welch, who in his retirement was the first director of the Johns Hopkins institute. Welch had been the acknowledged founder of the Johns Hopkins Medical School, so Temkin in his old age was the last living link to the founder.[1] At Johns Hopkins, Temkin was originally appointed associate to the institute. From 1935 to 1957 he was associate professor. He was appointed professor of the history of medicine in 1957, and in 1958 he became William H. Welch Professor and director of the Institute of the History of Medicine, following the retirement of Richard Shryock. (Shryock had been appointed in 1949, two years after Sigerist's retirement.) In 1964 Temkin also became professor of the history of medicine in the Johns Hopkins Department of the History of Science. He took emeritus status in 1968.

Returning to Temkin's earlier chronology, the newlywed Temkins made their first real home in Baltimore. With his usual insight and good humor, Temkin later recalled Mrs. Temkin's critical role in his acculturation:

> As a professional linguist, my wife . . . soon told me that to write acceptable English I had to think in English . . . The lack of strict definitions of words and the ease with which new definitions of words can be formed make German an ideal philosophical language. These qualities, however, easily protect vagueness and lack of clarity hiding behind an array of words that give a false impression of depth. My assimilation to English became concomitantly a critical review of much German writing, including some of my own. (1977, p. 23)

In 1934, following the rise of the Nazis, Temkin lost his German citizenship. He became a naturalized American citizen in 1938. By 1943 he had finished the manuscripts of three monographs, including his magisterial history of epilepsy, *The Falling Sickness*, which was published in 1945. In 1943-1944 Temkin served as a civilian with the Division of Medical Sciences of the National Research Council. His assignment (with colleague Elizabeth Ramsey) was "the preparation of reports on current research concerning the therapy of certain diseases important in the war effort," especially malaria (1977, p. 27; Stevenson and Multhauf, 1968, p. 303).

The two and a half decades from the end of World War II until just after his formal retirement in 1968 were the period of Temkin's most intense productivity and the foundation of his increasing reputation. During this time one of his most important colleagues was the peripatetic classicist-historian Ludwig Edelstein (1902-1965). Edelstein worked at the Johns Hopkins institute from 1934 to 1947 and again from 1952 to 1960. In addition to being director of the institute from 1958 to1968, Temkin was the editor of the *Bulletin of the History of Medicine* from 1948 to 1968. Mrs. Temkin was assistant editor from 1957 to 1971. The bulletin was—and remains—the most important journal in its field. In 1958-1960 Temkin served as president of the American Association for the History of Medicine.

One of the most taxing and rewarding obligations of many academics is the molding of advanced students into full-fledged scholars. Temkin directed the doctoral studies of his two immediate successors at the institute, Lloyd G. Stevenson (director 1968-1983) and Gert H. Brieger (director 1984-2001). A partial list of other prominent historians of medicine who took their Ph.D.s with Temkin includes Donald G. Bates, Chester R. Burns, and Toby Gelfand. In addition, several established scholars spent long periods at the institute during Temkin's tenure, including Henry Guerlac, Edwin Clarke, Jerry Stannard, and Charles Rosenberg. Temkin also had several M.A. students who have contributed much to scholarship and teaching in the history of medicine (Stevenson, 1982, pp. 223-225). His lectures for medical students in the required courses on medical history were models of organization and clarity. I have known some Hopkins medical graduates of that era who later regretted that they had not listened more carefully.

For many years after his retirement Temkin remained a valued presence at the institute and in the university. This was also the time when honors began to flow in. Some had come earlier: the Welch Medal of the American Association for the History of Medicine (1952), the Sarton Medal of the History of Science Society (1960), and the Prize for Distinguished Scholarship in the Humanities of the American Council of Learned Societies (1962). In 1969 Temkin delivered the Hideyo Noguchi Lectures at Johns Hopkins, and in 1970 he gave the Messenger Lectures at Cornell University. In 1973 Johns Hopkins conferred an honorary LL.D., and in 1975 he received an honorary Sc.D. at the Medical College of Ohio in Toledo. He was a member of many prestigious academic societies in the United States and abroad, including the American Philosophical Society and the American Academy of Arts and Sciences.

Temkin's election to the National Academy of Sciences was the last of the major honors that came to him, and he was very pleased by it. After his election, Temkin was asked to select a section for his membership. There was no historical section, but there was a section on social and political sciences. Temkin chose the neurobiology section, because, as he explained in a letter to Kac,[2] it was "closest to my scientific interests and because quite a few of its members are known to me personally."[3] His daughter Judith Temkin Irvine recalls that he made his choice because of his identification with *The Falling Sickness*. At that time he was continuing to keep abreast of the scientific literature on epilepsy.[4] In 1981 Temkin supported an effort to establish a permanent section of history and philosophy of science in the Academy,[5] but the effort came to naught and apparently was never revived. In 1982 Temkin requested and received emeritus status in the Academy, because of his "age and advanced deafness."[6] The progressive hearing loss had started in middle age. In his later decades his mobility was increasingly restricted by severe arthritis.

Owsei Temkin remained clearheaded and sharp-witted to the end, which came with appropriate quietude when he was three months short of his one-hundredth birthday. His last book, *On Second Thought,* was published, with the help of his daughter Judith, before Temkin died in 2002. Judith was also his coauthor on his last historical paper, which appeared posthumously in the *Bulletin of the History of Medicine* in 2003.

THE FALLING SICKNESS

Temkin's first historical book was *The Falling Sickness,* which appeared in 1945. He had originally decided to undertake the work in 1931, partly in the hope that "historical clarification might be of some help to neurologists" (1977, p. 20). Its approach became the paradigm for his later monographs, and it set the standard for scholarship in the history of medicine for several decades. The book's contents are well defined by its subtitle, *A History of Epilepsy from the Greeks to the Beginnings of Modern Neurology* (i.e., from Hippocrates to John Hughlings Jackson and Jean Martin Charcot). An underlying theme is Temkin's lifelong interest in the concept and meaning of disease, including its scientific and cultural aspects. Given epilepsy's long association with religion, evil, magic, and scientific theorizing, it could be the perfect vehicle for such an investigation; as usual, the scholarly devil is in the myriad detail.

For each historical period (antiquity, Middle Ages, Renaissance, the Enlightenment, nineteenth century), Temkin guides the reader through the complexity of theories, beliefs, and practices that constituted the totality of epilepsy. He was able to do this effectively because of his thorough knowledge of the relevant languages and cultures and his command of how epilepsy was understood in his own time. In essence, until the nineteenth century, scientific and extrascientific concepts of epilepsy coexisted or even cohabited, usually with reasonable compatibility. By the decade of the 1870s, the modern scientific study and understanding of the disease (really, diseases) had begun in earnest, and that is where Temkin leaves off.

When the first edition went out of print and became scarce, Temkin decided against reprinting it, because he felt that it was out of date in two ways. First, there had been significant historical work that needed to be incorporated into a new edition. Second, in the period after World War II, scientific and clinical concepts of epilepsy had changed dramatically. Electroencephalography had become central to the diagnosis and understanding of seizures, and the concepts of psychomotor/temporal lobe epilepsy had emerged. The revised second edition appeared in 1971. In both editions Temkin explicitly acknowledged that he had limited himself to "a history of epilepsy in Western civilization" (1971, p. vii, x), including classic Arabic cultures, because he did not want to be "confusing history and anthropology" (1945, p. viii). That is, the relationship of epilepsy to prehistoric trepanning was (and remains) speculative, and he felt that folk practices in East and South Asian civilizations had little effect in the West.

In his preface to the second edition Temkin wrestled with a familiar historical conundrum: whether "to let the past speak for itself and [or] to bring it near to the understanding of the modern reader" (1971, p. vii). He concluded that the past must speak for itself as much as possible, but in the end the reader can see the past only through his own eyes. Most of the substantive changes in the revised edition of *The Falling Sickness* deal with the more recent history of epilepsy. Temkin extended his historical cutoff date by a decade to approximately 1890, "except in the case of Hughlings Jackson, where it seemed advisable to avoid any arbitrary boundary" (1971, p. vii). Jackson's work on (what was later called) psychomotor/temporal lobe epilepsy extended into the twentieth century.

GALENISM

This small volume is the published version of the Messenger Lectures on the Evolution of Civilization, which Temkin delivered at Cornell in October 1970. It appeared in 1973. Again, the subtitle defines the nature and scope of the work: *Rise and Decline of a Medical Philosophy*. This book belies more of Temkin's philosophical interests than his concern with the meaning of disease. Galen of Pergamun (ca. 130-200) codified and greatly expanded the entire corpus of Greek/Western medical knowledge up to and including his own time. His authoritative legacy was carried into the Renaissance, and parts of it persisted into the nineteenth century.

Temkin analyzed the philosophical underpinnings of this legacy, starting with the Platonic background of Galen's medical and scientific ideals. Galenic medicine—and some of the philosophy that went with it—was authoritative through the Christian Middle Ages. Temkin was particularly interested in the challenges that Galenism encountered during the Renaissance. Even in the seventeenth century "the mechanistic orientation . . . was not strong enough to replace Galenism as a unifying medical philosophy" (1973, p. 178). On the other hand, "By 1870 medicine was firmly launched on its new scientific course, which gave it the intellectual unity it had lost after the downfall of Galenism as a medical philosophy" (1973, p. 191).

THE DOUBLE FACE OF JANUS

Janus was the Roman god of beginnings, with two bearded faces on one head, looking in opposite directions.

Temkin agreed with Sigerist that this pagan deity is an appropriate "allegory" for the history of medicine, which looks simultaneously toward medicine as it advances and toward its history (1977, p. 9). The idea for the book was "planted" by Shryock and doggedly pursued by Jack Goellner, director of the Johns Hopkins University Press, until it was published in 1977. The first essay ("The Double Face of Janus") in this large volume is an intellectual autobiography, followed by reprintings of 36 of Temkin's previously published papers. A few are translated from their original German. Much of the factual substance in my present memoir about Temkin is based on this title essay. His good-humored but penetrating sense of irony is displayed on the first page when he says, "A publication of collected essays by their author is intrinsically an immodest undertaking." (1977, p. ix).

Fortunately, Temkin's scruples were overcome by the opportunity to comment on the republished essays. Indeed, the title essay is much more than a commentary on his previous work. It is also a participant's account of how the entire historiography of medicine evolved from the 1920s to the 1970s—a treasure for later historiographers. One of Temkin's lifelong concerns was the place and usefulness of medical history within medicine as a whole, because he always felt a strong obligation to the whole. Writing about his sense of commitment ca. 1930, he said, "As a historian I felt committed to scholarship rather than to a profession. My professional commitment was to medicine, for which I had been trained, and the feeling of obligation to medicine never left me throughout my career as an active member of a medical faculty" (1977, p. 20). In retirement 45 years later he had thought about moving outside medical history to do a study of the great historians of antiquity, but his mind's inclination stayed closer to home:

Health and disease have been subjects of religious and philosophical meditation, and as metaphors they are to be found in politics, science, and literature. . . Man has speculated over the meaning of his disease for himself and for his community. Medicine is not only a science and an art; it is also a mode of looking at man with compassionate objectivity. Why turn elsewhere to contemplate man's moral nature? (1977, p. 37)

HIPPOCRATES IN A WORLD OF PAGANS AND CHRISTIANS

True to the above conclusion, Temkin's last historical monograph, published in 1991, went back to his original interest in Hippocrates. It also followed the methodological example of *The Falling Sickness*, because it took a complex subject and traced it through many centuries of encounter between the subject and its environment. To a significant degree, this was a different way of looking at Hippocrates (i.e., a different way of asking questions about the Hippocratic corpus and its legacy). Again, the breadth and depth of intellectual sweep is astonishing and essential.

The first sentence of Temkin's preface poses the question, "How did the fame of the Greek physician Hippocrates fare during the first six centuries of our era, during which a pagan culture was transformed into a Christian one?" (1991, p. ix). In the first third of the book he explored the place of Hippocrates in Greek and Roman medical practice and culture, including its relationship to pagan religion. Christianity eventually absorbed much of Hippocratic practice and philosophy, especially through the mediation of Galen. However, there was always some tension in the relationship, mainly because ancient medicine and philosophy contained an element of scientific materialism that was inimical to Christian monotheism. Throughout the work Temkin deliberately avoided theological problems, but he took moral issues to be a legitimate part of his historical investigation.

"ON SECOND THOUGHT" AND OTHER ESSAYS IN THE HISTORY OF MEDICINE AND SCIENCE

In some respects this book is a followup volume to *The Double Face of Janus*, 25 years later. Fourteen of its 16 chapters are reprints of Temkin's earlier papers, none of which had been included in *Janus*. What had been left out but was now deemed worthy of reprinting is interesting in itself. "Gall and the Phrenological Movement" (1947; 2002, pp. 87-130) is a strikingly clear exposition of the cultural and philosophical milieu in which Gall and Spurzheim developed their ideas. In commenting on his reprinting of the essay in 2002, Temkin says only that it is "an early, mid-nineteenth century example of the conflict between the objective and the subjective sides of human beings"[7] (2002, p. 9). It is also interesting to note that serious historical interest in phrenology developed widely only in the late twentieth century.

The most remarkable aspect of "On Second Thought" is not the fact of its appearance from the pen of a centenarian. Rather, it is the fact that this centenarian was still rethinking and reworking his previous positions on scholarly issues. The most important example of this reconsideration is Temkin's changed opinion about Edelstein's analysis of the Hippocratic oath (Edelstein, 1943). Edelstein concluded that the oath is largely of Pythagorean origin, and this idea was widely accepted. The second essay in "On Second Thought" is not a reprint. It is an original essay that takes issue with Edelstein's position by asking, "What Does the Hippocratic Oath Say?" and then offering "Translation and Interpretation" (2002, pp. 21-28). Temkin pointed out that the evidence for the existence of a group of Pythagorean practitioners is very weak and not supported by the oath itself, which remains "a puzzling document" (2002, p. 27).

Perhaps because Edelstein was no longer present to defend himself, Temkin seems to have had some pangs of remorse about the matter, so he offered a *mea culpa*: "To atone for my heresy, I have included in this volume the obituary of my friend Edelstein, a statement written before I developed second thoughts about his approach to ancient medicine" (2002, p. 4).

EPILOGUE

Even in a life as long and productive as Temkin's, there are projects left unfinished. Early in his career Temkin took an interest in the history of the biological concept of irritability (1936). By the late 1940s he had resolved to write an extensive analysis of the subject. The Noguchi Lectures of 1969 at Johns Hopkins were titled "On the History of Anger, Irritation, and Irritability." According to his account in *Janus*, they remained unpublished at that time because he still hoped "to expand them in a much more comprehensive book" (1977, p. 31). It was not to be, and we can only contemplate the whole from the fragments. Perhaps this is also true of the man.

The manuscript was critiqued by Gert Brieger, Judith Temkin Irvine, and Nancy McCall, whose assistance was much appreciated. I am also indebted to the assistance provided by Andrew Harrison at the Chesney Archives at Johns Hopkins (see note 2 below).

NOTES

1.Owsei Temkin: The man who knew Welch. *Hopkins Med. News,* spring/summer, 2001.
2.Typescript letter from M. Kac to Temkin, May 10, 1978, in Temkin papers at the Alan Mason Chesney Medical Archives of the Johns Hopkins Medical Institutions (henceforth, Temkin/JHMI Archives).
3.MS draft of letter from Temkin to Mark Kac, May 22, 1978, in Temkin/JHMI Archives.
4.Personal communications: emails from Judith Temkin Irvine to Samuel Greenblatt, January 31 and February 3, 2006.
5.Copies of typescript letters from Joseph S. Fruton to members of the National Academy of Sciences and to Bryce Crawford Jr., Home Secretary of the NAS, both dated September 28, 1981; MS of Temkin's reply of October 10, 1981; in Temkin/JHMI Archives.
6.Copy of typescript letter from Temkin to Crawford, May 4, 1982, and letter from Crawford to Temkin, May 17, 1982; in Temkin/JHMI Archives.
7.He had previously stated that the essay on Gall was omitted from that volume because of its excessive length (Temkin, 1977, p. 31, footnote 67).

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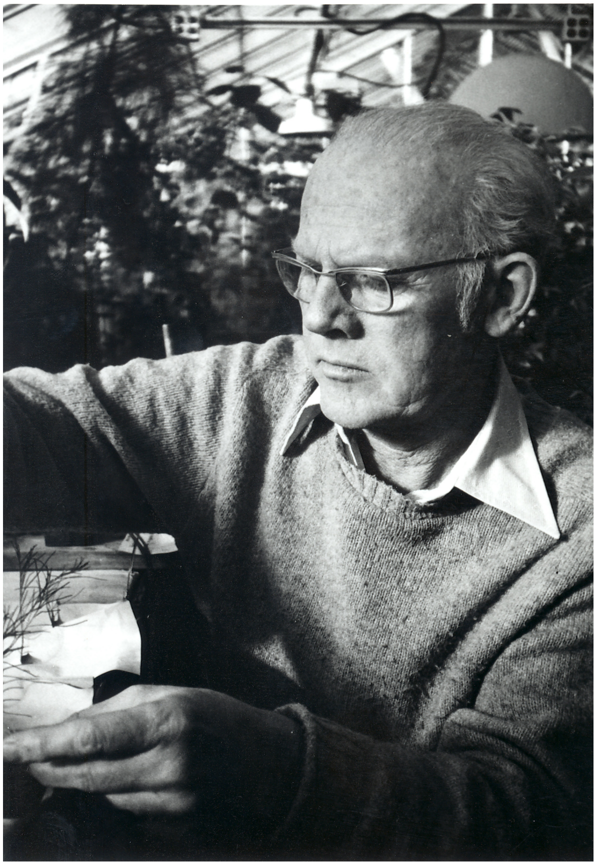

John G. Torrey

JOHN GORDON TORREY

February 22, 1921–January 7, 1993

BY LEWIS FELDMAN AND ALISON BERRY

John gordon torrey was a forthright, honest, highly principled man, and a groundbreaking plant scientist. All who associated with John, or "JGT" as he was called by his graduate students, valued greatly his opinions and wise counsel.

He was born in Philadelphia on February 22, 1921, the third of four children, and the second son of Edward and Elsie (Gordon) Torrey. He died January 7, 1993, in Greenfield, Massachusetts. Torrey graduated from Williams College (Williamstown, Massachusetts) in 1942 and shortly thereafter enlisted as an officer in the U.S. Army, serving for the duration of World War II in the Medical Administrative Corps in both the United States and Europe. As noted by Kenneth Thimann, John Torrey's Ph.D. dissertation supervisor at Harvard, Torrey "came to Harvard while still in uniform and was the first graduate student to come into the Biology Department after World War II." In 1947 while still a graduate student, he was awarded a traveling fellowship allowing him to spend a year (1948-1949) at Cambridge University in the Botany School. In 1949 in England he married Noreen Lea-Wilson whom he had met during his earlier military service in the United Kingdom. Also in 1949 when he returned to Harvard, Torrey submitted a thesis titled "Studies on the Physiology of Lateral Root Formation and Root Growth," for which he was awarded a Ph.D. in biology in 1950. Thereafter roots would be at the center of his long and highly influential research career.

Torrey received assistant professor offers from Berkeley, Santa Barbara, and McGill. Thimann lobbied for McGill, but Torrey instead chose Berkeley and joined the Department of Botany in late 1949. There he became part of a young cadre of new faculty, including Leonard Machlis and Johannes Proskauer, who together rejuvenated the department and expanded its role from that of service to agriculture to include the broader discipline of plant science.

While at Berkeley, Torrey began to elaborate his research on roots, focusing mainly on their growth and development. His dissertation adviser, Professor Thimann, had played a central role in characterizing the structure and function of the plant growth regulator, auxin (indole-3-acetic acid), and Torrey built on this knowledge to provide definitive evidence for the involvement of auxin in lateral root initiation and outgrowth. Based on his previous tissue culture experience at Harvard with Professor Ralph Wetmore, Torrey incorporated sterile culture techniques into his studies of roots, allowing him to manipulate root growth and development. From these efforts came seminal papers that characterized the controls of patterning in roots, with special emphasis on vascular patterning. Torrey was able to show that patterning was regulated by the root apical meristem. His research papers and the reviews arising from this work serve as the foundation for much of contemporary experimental root biology.

While at Berkeley, Torrey taught, with Leonard Machlis, a plant physiology course that included a laboratory. As no suitable laboratory manual was then available, he and Machlis wrote *Plants in Action; a Laboratory Manual of Plant Physiology*, which not only became the standard text for plant physiology laboratory classes throughout the United States but also served as a valuable laboratory reference.

In 1960 and now an associate professor at Berkeley, Torrey accepted an invitation to return to Harvard as a professor in the Department of Biology. There he expanded his studies of roots and began to focus on an aspect of root development that had earlier attracted his attention, namely, the fixation of atmospheric nitrogen (nitrogen fixation) in rootborne structures known as nodules. Torrey believed that his earlier work on lateral root development could provide a context for further discoveries of this scientifically interesting and economically important process. His initial studies of this phenomenon were with members of the pea family (legumes), in which nitrogen fixation occurs as a consequence of the association between the root and a bacterium belonging to the genus *Rhizobium*. Torrey was interested in the beginning stages of nodule initiation, and focused much of his early research on understanding the reprogramming of the root cortical cells allowing them to develop into nodules. During the early years following his return to Harvard, Torrey authored (in 1967) *Development in Flowering Plants*, in which he pointed the way to the challenges ahead for plant developmental biologists.

In the early 1970s Torrey moved his research activities to the Harvard Forest, in Petersham, Massachusetts, about 60 miles west of Cambridge. Coincident with this move was a redirection and refocusing of his research to include nitrogen fixation in root nodules of perennial, nonlegume plants, with initial emphasis on the genus *Comptonia* (Sweet Fern). This plant grew abundantly in and around the Harvard Forest, inhabiting open woodlands and clearings. Torrey and his group showed that the organism causing nodule formation in *Comptonia* and responsible for nitrogen fixation was neither a fungus, as was once believed, nor a member of the bacterial group called Rhizobia, which carries out nitrogen fixation in root nodules of the pea family. Instead, the microsymbiont in *Comptonia* root nodules and those of related plant hosts, belonged to the genus *Frankia*, bacteria in the Actinomycete group, which are evolutionarily distant from the Rhizobia. A significant accomplishment stemming from these efforts was the discovery of how to grow *Frankia* in culture, outside the root nodule environment. These landmark discoveries formed the basis of the most productive period in Torrey's research career, in which more than 70 papers and one coedited volume (*Applications of Continuous and Steady-State Methods to Root Biology*) were published between 1978 and 1991, detailing various aspects of the development of the association between *omptonia* roots and *Frankia*.

In 1965 while still in Cambridge, Torrey assumed a major administrative role as the fifth director of the Maria Moors Cabot Foundation for Botanical Research. Using fair, balanced judgment he was instrumental in directing foundation funds to support a much needed updating of botanical facilities at Harvard, including the expansion of the University Herbaria and the establishment of the Controlled Environment Facility at the Harvard Forest. He also ensured that foundation funds were made available to junior faculty, and that the granting of these funds would involve a minimum of paperwork.

Later, in 1984, he continued his administrative duties through his appointment as the Charles Bullard Professor of Forestry and director of the Harvard Forest. He maintained these positions until 1990. As director he had great impact on activities at the forest, from initiating freshmen seminars (in those days a novelty at universities) to spearheading a consortium of scientists from several institutions that eventually led to the awarding of a large National Science Foundation grant supporting the establishment of a Long Term Ecological Research site at the forest. The legacy of his efforts and his perspectives are today reflected in the expanded and varied ecological research activities at the forest.

Although located in Petersham, Torrey continued to teach classes, supervise graduate students, and meet with visiting scholars in Cambridge. He also established an association with the University of Massachusetts in nearby Amherst, and there offered his mentoring skills and served on doctoral dissertation theses.

He was a strong supporter of women in science and encouraged his female associates, both graduate students and visitors, to aim high. The legacy of this mentorship is today evident in the many successful academic careers of women who worked with John Torrey. “In a tough academic world,” one former female graduate advisee noted, “he was truly a hero.”

John Torrey was a recipient of many awards and appointments, including a Guggenheim Fellowship (1965-1966) and a Fulbright Senior Research Scholar Fellowship (1984). He was a member of the American Academy of Arts and Sciences, the Botanical Society of America, and the Society for Developmental Biology (president, 1963), among other organizations. He was elected to the National Academy of Sciences in 1981.

Torrey was known to his many associates worldwide for his interests in etchings, particularly those from Scotland, England, and New England, from the late 19th and early 20th centuries. In his retirement he had intended to explore “the interaction, interplay and influence of the group of British etchers on the Americans and vice versa.” Whenever he traveled he would reserve some time to visit local antique and art establishments. In these adventures he was usually, but not always, successful in acquiring a new etching or watercolor.

John Torrey was occasionally confused with the noted 19th-century American botanist of the same name, founder of the Torrey Botanical Club. Although there was no relation, he was mildly bemused by the confusion occasioned by the coincidence of both their names and occupations. His wry sense of humor was not often seen but could be noted occasionally, such as when Torrey drove past a graveyard and chuckled at the sign reading “One Way.”

Reflecting on John Torrey, his Harvard colleagues recollected that he was “outspoken and, in his controlled way, passionate about what he thought was right and what he thought was wrong.” On one occasion when a Harvard colleague off-handedly informed Torrey in his role as director of the Cabot Foundation that he (the colleague) would be spending funds differently from what was originally budgeted, Torrey noted firmly, “No, I don’t think you should spend that money in a way that differed from your original intentions.”

John and his wife, Noreen (Norah), were married for 43 years and had five daughters: Jennifer, Joanna, Susan, Sarah, and Carolyn.

John Torrey was a man of high ethics both in his science and in the way he lived his life. He treated everyone equally, with respect, dignity, and honesty, and he expected no less from others. At his passing his daughter Joanna penned, “We lay him to full rest, sorry that he is not here with us, hat in hand, to point up into the trees, or down to the earth, sharing his faith in that which grows.”

Some information used in preparing this remembrance was obtained from the Memorial Minute of the Harvard University Faculty of Arts and Sciences, appearing in the November 7, 1996, edition of the *Harvard Gazette*.

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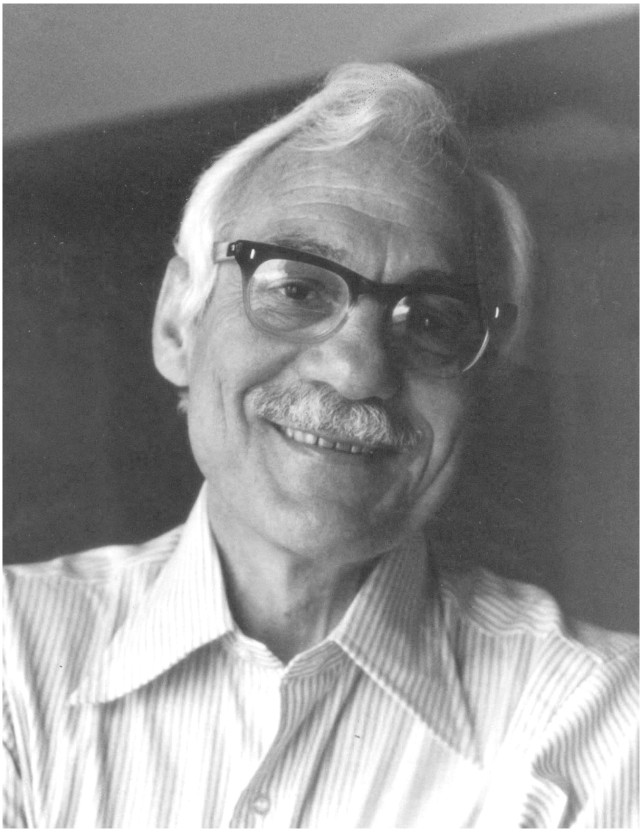

Courtesy of the Archives, California Institute of Technology

Jerome Vinograd

JEROME VINOGRAD

February 9, 1913–July 7, 1976

BY ROBERT L. SINSHEIMER

JEROME VINOGRAD WAS BORN ON February 9, 1913, in Milwaukee, Wisconsin. His academic career began with a series of two-year programs at the University of Minnesota (1929-1931), University of Berlin (1931-1933), and the University College, London (1933-1935). He then studied at the University of California, Los Angeles, receiving an M.S. in 1937 and then at Stanford, receiving a Ph.D. in 1940 in physical and colloid chemistry. His work at Stanford resulted in five publications concerning the use of detergents to solubilize otherwise insoluble dyes, among other topics.

In 1937 he married Sherna Shalett. They had two daughters, Julie and Deborah. He was subsequently divorced and in 1975 married Dorothy Colodny.

During 1941-1951, Vinograd worked for the Shell Development Company on the use of emulsion polymerization to produce synthetic rubber and on problems of catalysis to manufacture aviation gasoline. In 1951 he moved to the Department of Chemistry at the California Institute of Technology, first as a senior research fellow, then in 1956 as a research associate, and in 1966 as a professor of chemistry and biology.

Professor Vinograd was elected to the National Academy of Sciences in 1968. He received the Kendall Award from the American Chemical Society in 1970 and the Helen Hay Whitney Foundation Duckett Jones Award in 1972.

Active with a variety of problems, Professor Vinograd was best known for two major areas of scientific accomplishment: the theory and application of density gradient ultracentrifugation and the study of the properties of closed circular DNA rings.

DENSITY GRADIENT ULTRACENTRIFUGATION

Vinograd's initial major contribution was the development of density gradient ultracentrifugation. This was stimulated by Matthew Meselson and Frank Stahl, who were seeking a means to implement their bold experiment to verify the hypothesis that DNA replication involved the separation of the two parental strands, one going into each of the two daughter DNA molecules.

Meselson and Stahl initially wanted to make the parental strands heavier than normal by incorporation of 5-bromouracil and sought Vinograd's advice as to whether 5-bromouracil-containing DNA could be separated from normal DNA by velocity ultracentrifugation. Vinograd indicated this seemed unlikely unless the velocity difference could be magnified by approximately matching the density of the DNA of the strands with a salt solution. From this germinated the concept of equilibrium sedimentation of macromolecules in density gradients and subsequently the famous experiment of Meselson and Stahl using the isotope of N^{15} instead of bromouracil.

In the equilibrium sedimentation method the macromolecule (DNA) is dissolved in a salt solution (CsCl) of the appropriate density and centrifuged to equilibrium (approximately 24 hours). At equilibrium, driven by sedimentation and diffusion, the CsCl will form a stable gradient of concentration, increasing in density with the radius. The larger

DNA molecules will form a narrow Gaussian band centered about the solution density equivalent to their own density. At equilibrium the DNA molecules are then distributed with respect to concentration in a band of width inversely related to their molecular weight. If the DNA molecules are of more than one discrete density (as in the Meselson and Stahl experiment) more than one discrete Gaussian band will appear. If the DNA varies broadly with respect to composition a skewed non-Gaussian distribution will appear. From the width of a Gaussian band and the gradient of CsCl density the molecular weight of the DNA can be calculated.

Several later papers refined the theory and extended the density gradient technique in its application to macromolecules. A variation that employed a lamella of the macromolecules layered on a self-generating density gradient permitted more rapid determination of their sedimentation and diffusion coefficients. This was applied to hemoglobin, MS2 RNA, T7 DNA, φX174 virus, and southern bean mosaic virus.

Further papers considered the solvation of DNA in CsCl as a function of the CsCl density and the solution temperature, the viscosity of CsCl as a function of its density, and the banding of RNA in a CsCl gradient. The addition of dimethylsulfoxide was shown to enhance the ability to band RNA in CsCl without aggregation.

STUDIES OF CIRCULAR DNA

Jerome Vinograd opened a new chapter in his research with his studies on closed circular DNA from several sources. It is interesting to follow the progression of this research project. The first paper of this series concerned the doubled-stranded DNA of polyoma virus, which was shown to sediment monomolecularly and not to lose infectivity after heating to 100 degrees for 10 to 20 minutes. Sedimentation analysis under varied conditions and electron microscopy confirmed the presence of cyclic DNA (form I) and what were believed to be linear DNA molecules (form II).

Subsequent research demonstrated that form II was also circular but with one strand of the DNA helix cut. By investigating why the circular form I (an intact duplex) sedimented 20 percent faster than form II (also a duplex with one or more strand scissions) Vinograd demonstrated that form I was a twisted circular structure that could be converted to form II by enzymatic scission of one strand. This twisted circular form appeared twisted in electron micrographs. Its sedimentation behavior implied the presence of secondary left-handed turns. The binding of intercalative dyes, such as ethidium bromide, was shown in Vinograd's and other laboratories to cause a partial unwinding of the duplex DNA structure. In closed circular DNA such unwinding is accompanied by a change in the number of superhelical turns so that the total number of turns in the molecule remains constant. At a critical amount of dye binding the number of superhelical turns is zero. More dye binding results in superhelical turns of the opposite sign.

All of this can be followed in the ultracentrifuge. As the maximum amount of dye that can be bound by the closed circular molecule is less than can be bound by the linear or nicked circular molecule and as the buoyant density of the DNA-dye complex is inversely related to the amount of dye bound, the buoyant density of the closed circular DNA-dye complex is greater than that of the linear DNA or nicked circular DNA-dye complexes. At saturating amounts of ethidium bromide the buoyant density difference is approximately 0.04 gm/ml in CsCl. This difference provided a means to isolate closed circular DNA from the mitochondria of HeLa cells. Electron microscopy of this preparation demonstrated not only the presence of closed circular DNA but also of small amounts of duplex or larger multiples of the mitochondrial DNA.

All of this work resulted in the formulation of the topological winding number,α, an invariant number, which characterizes the molecule. α is smaller than the secondary winding number, β, the expected number of turns for a DNA double helix of the size involved. γ is the number of superhelical secondary turns needed to achieve the maximum chemical stability, and $\gamma=\beta-\alpha$. Alkaline titration revealed an early titration of 3 percent to 4 percent of the base pairs of polyoma virus DNA, which suggested that the superhelix density is 0.03-0.04 yielding 15-20 superhelical turns in a DNA of 3 million molecular weight.

Further research demonstrated that intercalation of ethidium bromide causes an unwinding of the superhelical turns of 12 degrees per bound dye molecule. The binding of 30 dye molecules results in the removal of one superhelical turn. Thus, the native superhelical density can be determined by measurement through the region in which the superhelix changes sign by dye titration of the buoyant density.

Alternatively, at high dye concentration the buoyant density difference between open and closed forms of SV40 DNA was shown to be approximately constant because of the free energy required to form positive superhelixes. This buoyant energy difference is quantitatively related to the native superhelix density. Vinograd demonstrated that the initial superhelix density σ_c is related to v_c, the molar binding ratio at the dye concentration at which all superhelical turns are removed by $\sigma_c = 0.62\, v_c$.

The superhelix density of a closed circular DNA thus can be determined by measurement of its buoyant density in CsCl-ethidium bromide relative to that of a nicked version of the same DNA or to that of a reference DNA (with appropriate correction for base composition).

The superhelix densities, determined by these methods, differ for DNA from various sources. The density for mitochondrial DNA from SV40-transformed mouse cells in culture is ~ 2×10^{-2}, while for DNA from the bacterial virus PM2 it is ~ 5×10^{-2}. Mitochondrial DNA from cells grown in media containing the intercalating dye ethidium bromide can have a superhelix density as high as 12×10^{-2}.

Subsequent studies were undertaken with "nicking and closing" enzymes, now called topoisomerases, which converted closed circular DNAs into a product set with a zero mean degree of supercoiling. The individual species of the sets differed by one, two, three, etc., of supercoils with the relative masses of each type fitting a Boltzmann distribution, with the energy of supercoiling proportional to the square of the degree of supercoiling. The enzymes can relax both positive and negative superhelical turns. The same set of products could be obtained by action of the enzymes upon separated rings of varied supercoiling.

MITOCHONDRIAL DNA

Further studies of mitochondrial DNA from varied sources found the common presence of circular oligomers and of catenated oligomers. In a CsCl-ethidium bromide density gradient the closed oligomer bands at the same density as the monomer, although its sedimentation velocity is significantly greater.

Catenated molecules can (for dimers) have both rings closed, one closed and one relaxed (nicked), or both relaxed. If both are closed, their sedimentation rate is the same as the circular dimer. If one ring is closed and one relaxed, the sedimentation rate is intermediate between a closed circular dimer and a relaxed circular dimer, demonstrating that the two rings are connected. If, however, both rings are relaxed the sedimentation value is surprisingly less than that of a circular monomer (i.e., the two rings behave as though they are more or less independent).

Continuing studies of mitochondrial DNA molecules revealed the presence of replicating circles. These involved an initial D loop of some 450 nucleotides (the "heavy" strand), which is subsequently continued in the presence of a nicking process. Later replication of the "light" strand commences at about 60 percent from the "origin," proceeding counterclockwise.

The discovery that closed circular mitochondrial DNAs of about 10 million molecular weight invariably suffered chain scissions at high pH led to the suggestion of covalently incorporated ribonucleotides. This was confirmed by the quantitative conversion of closed mitochondrial DNA to nicked DNA by ribonuclease H. From the biphasic activity of the enzyme, two populations are present, one containing about 10 ribonucleotides, the other some 30 ribonucleotides.

SCIENTIFIC ACTIVITIES

The 1950s and 1960s were an era of great advances in molecular biology, and Caltech, with George Beadle and Max Delbruck and Linus Pauling, was a major center for this progress. Professor Vinograd thrived in this environment and, as described, made many contributions to this vibrant field. During this period of intensive research and discovery, Professor Vinograd was highly active in the scientific community. He gave nearly a dozen lectures each year at various colleges and universities, including the Burroughs Wellcome Lecture at Harvard in 1970, the Jesse W. Beams Lecture at the University of Virginia in 1972, and the Falk-Plaut Lecture at Columbia University in 1972, and participated in many scientific meetings. He mentored a continuous stream of graduate students and postdoctoral fellows, including William Bauer, Robert Bruner, David Clayton, Lawrence Grossman, John Hearst, Bruce Hudson, James Ifft, Harumi Kasamatsu, Roger Radloff, Robert Watson, William Upholt, and Hans-Peter Vosburg.

Jerry, as he was known to his friends and colleagues, had an open personality, always ready to discuss scientific questions of common interest and quick to provide analytical insight.

Professor Vinograd died suddenly at the age of 63.

FOR THEIR ASSISTANCE, I am indebted to the staff of the archives of the California Institute of Technology.

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Aaron C Waters

AARON CLEMENT WATERS

May 6, 1905–May 18, 1991

BY CLIFFORD A. HOPSON

AARON C. WATERS, ONE OF the leading volcanologists of the twentieth century, passed away on May 18, 1991, at age 86 in Tacoma, Washington. Professor Waters, best known for his pioneering work on the Columbia River Basalt, led the way also in other studies of basaltic volcanism in the Pacific Northwest, the mechanics of basaltic lava flows, the development of lava-tube cave systems, and violently explosive basaltic volcanism recorded by maar-type volcanoes. This expertise led directly to his participation in studies of lunar geology as the U.S. space program got underway, including composition and origin of the lunar surface, the assessment of Apollo landing sites, and geologic training of the Apollo astronauts. But it was the breadth of Aaron Waters's geologic accomplishments, rather than specialization, that marks his distinguished career. He made important contributions to our understanding of calc-alkaline volcanism, to granitoid batholiths and their hypabyssal intrusive complexes, to deep-seated metamorphism, and to the geomorphic evolution of landscapes as well as facets of structural and economic geology. The Pacific Northwest region was the focus of his diversified studies, and he was regarded for many years as the leading geologic authority on that region. Yet, his studies extended also to other parts of the United States, the world, and the Moon. Professor Waters was known, too, as an outstanding mentor of graduate students, the coauthor of a leading textbook on the principles of geology, the builder of distinguished geology departments in leading universities, and, late in his career, a valued geologic consultant to federal research organizations.

BEGINNINGS AND EARLY DIRECTIONS

Aaron Waters was born in Waterville, Washington, on May 6, 1905, the son of pioneer parents and the youngest of seven children. His early years were spent on the family homestead and wheat ranch near Waterville, on the western edge of the Columbia River Plateau in the shadow of the Cascade Mountains. He worked on the ranch during his youth and helped to run it at age 12 and 13 during the last years of World War I, when his older brothers went off to war. Following graduation from high school, Aaron entered the University of Washington, supporting himself by work in a gas station and other jobs. He began prelaw studies but later changed to geology, partly from his love of the out-of-doors but influenced especially by his college friend Richard E. Fuller, who later became both a distinguished geologist and a long-time director of the Seattle Museum of Art. Waters earned a B.Sc. in geology (cum laude) in 1927 and an M.Sc. in 1928, both from the University of Washington.

Waters continued his studies at Yale University, known for its program in geology and development of leaders in that field. He earned his Ph.D. in 1930 under the mentorship of Professor Adolph Knopf. His dissertation, "Geology of the Southern Half of the Chelan [30'] Quadrangle, Washington," provided the basis for several outstanding papers, and had a lasting influence on the direction of his career. The eastern part of his map area overlapped the Columbia

River Plateau, one of the world's great outpourings of tholeiitic plateau basalt. His doctoral study of those lavas was a first step toward later preeminence in the study of plateau basalts and leadership in establishing the stratigraphy of the Columbia River Basalt Group. The western part of the Ph.D. map area exposed the pre-Tertiary crystalline basement of the North Cascades. Here his work encompassed both the bedrock geology and the geomorphology, also leading to important publications.

THE STANFORD YEARS: PART 1, 1930-1941

The first taste of university teaching came during his doctoral studies at Yale, where Waters served as instructor in the Geology Department (1928-1930). Teaching proved congenial and complimentary to his research interests, and it was here that his ability to stimulate and motivate students first blossomed. Here, too, the choice of a career was determined and with his scholarly reputation growing, Waters accepted appointment as assistant professor of geology at Stanford University. He soon rose to professor and remained at Stanford for 21 years (1930-1951).

Waters's early research was not yet focused on volcanic rocks but reflected broad interests in Pacific Northwest regional geology, interests that spanned igneous and metamorphic petrology, geomorphology, and tectonics. His stature and reputation grew as the excellence of his published research during the prewar period (1930-1941) became widely recognized and appreciated. Two influential papers soon emerged from continued work on the bedrock geology of his Washington field area: "A Petrologic and Structural Study of the Swakane Gneiss" (1932) and "Petrology of the Contact Breccias of the Chelan Batholith" (1938). The latter was the first of several perceptive studies of granitoid pluton emplacement and crystallization.

Aaron's interest in geomorphology vied with that in petrology during his early years at Stanford. He deciphered the complex record of Pleistocene glaciation, where the continental ice sheet—advancing across and retreating from the Columbia River Plateau—had dueled with the alpine glaciers flowing down from the Cascade Mountains. Their interaction is brilliantly documented in "Terraces and Coulees along the Columbia River near Lake Chelan, Washington" (1933). His "Resurrected Erosion Surface in Central Washington" (1939) was followed later by other papers (e.g., 1955) that describe the geomorphic evidence for Neogene deformation of the southwestern margin of the Columbia River Plateau and uplift of the Cascade Mountains. These and other aspects of Aaron's early studies in north-central Washington and his coeval projects in Oregon (e.g., 1927, 1929, 1935), along with his exceptional breadth spanning volcanism, plutonism, metamorphism, geomorphology, and tectonics contributed to Aaron's growing reputation as an expert on regional geology of the Pacific Northwest.

Aaron Waters's arrival at Stanford also ignited a new interest (cataclasites and mylonites) launched by his discovery of those distinctive rock types along the San Andreas Fault zone near Crystal Springs Lake. The San Andreas was an enigma back then: It was thought to be a "big" fault from the contrasting rocks on each side, but its huge strike-slip displacement was not yet recognized. Mylonites had first been described along faults in Scotland, so their occurrence here within the San Andreas Fault zone seemed consistent with a fault-related origin. With doctoral student Charles Campbell he studied the rocks petrographically, reviewed the occurrence of similar rocks elsewhere, and devised a descriptive and genetic classification for mylonitic rocks (1934) that remained authoritative for many years.

His papers led to a Guggenheim Fellowship for studies in Scotland and Scandinavia in 1938-1939, where other mylonites reinforced his awareness of the role of penetrative mechanical deformation in developing fine-grained, banded crystalline rocks. It was therefore no accident that his next major field study, with doctoral student and later distinguished professor Konrad Krauskopf, addressed the unique banded granodioritic rocks that bordered the eastern side of Washington's Okanogan Valley. Their resulting paper, "The Protoclastic Border of the Colville Batholith, Washington" (1941), attracted wide interest. Known now as the Okanogan gneiss dome and recognized as one of a semicontinuous belt of metamorphic core complexes that extend from southern Arizona to British Columbia, it records the penetrative deformation of midcrustal crystalline rocks and hypersolidus mushes, later brought to the surface during crustal extension. But the fundamental role of protoclastic deformation—the penetrative crushing, granulation, and neo-recrystallization of rock in the presence of an interstitial melt phase—first established by Waters and Krauskopf, remains valid today.

Another aspect of Professor Waters's Guggenheim studies in Scotland, however, had greater influence on the direction of his future work. His examination of the Tertiary volcanic centers and ring-dike complexes on the Inner Hebrides islands of Mull, Ardnamurchan, and Skye returned his attention to basaltic volcanism and its shallow plutonic connections, This became a focus of his research.

THE WAR YEARS (1942-1945) AND AFTERMATH

World War II intervened, and Waters took leave from Stanford to join the U.S. Geological Survey's expanded exploration program for strategic minerals, deemed vital to the war effort. Mercury was among those essential metals whose known reserves were limited, and Waters teamed with other volcanologists—mercury ores being associated with volcanic and subvolcanic rocks—to improve the inventory. His introduction to mercury (quicksilver) deposits had begun in the prewar years in southwestern Oregon (1935), and these were now the focus of his war-related fieldwork in Arkansas, Oregon, and elsewhere in the western United States. Resulting U.S. Geological Survey (USGS) publications appeared in 1951. Beyond the primary purpose of strategic mineral assessment, this work extended Waters's grasp of regional volcanism in Oregon, and the volcanogenic processes that concentrated trace elements.

A brief return to ore deposits research came with Waters's participation in the USGS's uranium exploration program on the Colorado Plateau (1951-1952), where volcanogenic processes once again proved important (1953). Here the search for magmatic sources of the strata-bound uranium ores led to his collaboration with Charles B. Hunt on the petrology and petrogenesis of subvolcanic rocks of the North La Sal stock and its laccoliths (1958).

THE STANFORD YEARS: PART 2, 1945-1951

Professor Waters's return to academic geology at Stanford (1945-1951) marked the beginning of his main concentration on volcanism and volcanic rocks, especially basalts of the Pacific Northwest, soon to flower after his subsequent move to Johns Hopkins. This work began during the summers of the late 1940s with his geological mapping in north-central Oregon, as part of the USGS's regional geologic mapping program that had evolved from the strategic minerals program of the war years. This reconnaissance mapping extended Waters's knowledge of the Columbia River basalts and other Tertiary volcanic rocks.

But a more immediate task came first. Waters and close friend James Gilluly, then a professor at the University of California, Los Angeles, and Professor A. O. Woodford of Pomona College, had become disenchanted with the existing textbooks on introductory physical geology, and began to write one of their own. The first edition of *Principles of Geology* (1951), superbly illustrated by the skillful drawings of Stanford colleague Robert R. Compton, was an immediate success. This book, substantially improved in its second edition in 1959 and its third edition in 1968, remained the leading physical geology textbook for more than two decades.

THE JOHNS HOPKINS YEARS, 1952-1963

A uniquely productive and happy period of Aaron Waters's career came with his appointment in 1952 as professor of geology at Johns Hopkins University in Baltimore, a position he would hold for the next 11 years. His appointment was part of a profound resurgence of the university's geology department, engineered by its new chair, Ernst Cloos. Cloos hired both Waters and famed sedimentologist Francis Pettijohn, followed by experimental petrologist Hans P. Eugster plus several younger men. Cloos, Pettijohn, Waters, and Eugster were all elected to the National Academy of Sciences within the next dozen years; they, together with veteran mineralogist and X-ray crystallographer J. D. H. Donnay (later elected president of the Mineralogical Society of America), formed the nucleus of an exceptionally strong graduate program and center of research excellence. It was a close and congenial group that interacted and worked well together. Good friends Waters and Cloos were an especially effective team: Chairman Cloos was a kindly but strong and progressive department executive who ran a harmonious ship, and Waters worked effectively behind the scenes to help strengthen the department and to attract bright students to its graduate program. The highly successful cooperative program between the Johns Hopkins geology department and the Geophysical Laboratory (Carnegie Institution of Washington) in nearby Washington, D. C., whereby Hopkins graduate students undertook doctoral research projects in experimental petrology at the "Geewhiz Lab," was one of the fruits of Aaron Waters's progressive thinking and influence.

It was during his Hopkins era that Waters wrote and published several of his most important volcanological papers: "Volcanic Rocks and the Tectonic Cycle" (1955), "Stratigraphic and Lithologic Variations of the Columbia River Basalt" (1961), and "Basaltic Magma Types and Their Tectonic Associations: Pacific Northwest of the United States" (1962). Waters also spearheaded a meaty revision of the Gilluly-Waters-Woodford textbook (2nd edition). He also researched and wrote at that time (with younger colleagues Richard Fiske and Clifford Hopson) the monograph "Geology of Mount Rainier National Park, Washington" (1963). This project was organized and led by Waters, who generously gave both younger men their choice of topics (Fiske took the Tertiary volcanogenic formations; Hopson, Mount Rainier volcano) and relegated himself to last position in the authorship. Yet, the section of the Mount Rainier monograph written by Waters on the "Miocene Tatoosh Pluton and Its Hypabyssal Intrusive Complexes" is perhaps the most insightful part of that study.

The Johns Hopkins years were marked also by his mentoring of an exceptionally able group of graduate students (J. G. Moore, R. S. Fiske, D. L. Lindsley, R. Shepard, D. Swanson, H.-U. Schmincke, W. S. Wise, and others), who later rose to prominence in volcanology. When Mount St. Helens awoke from a long quiescence in 1980, two of the three volcanologists who helicoptered daily to monitor the volcano before and after its cataclysmic eruption of May 18, 1980, were Waters's former Hopkins doctoral students (Moore and Swanson). His Johns Hopkins years were truly a uniquely active and creative period in Aaron Waters's distinguished career.

THE UNIVERSITY OF CALIFORNIA YEARS: 1963-1972

Always receptive to new challenges, Professor Waters returned to his West Coast roots in 1963, accepting the chair of the budding Geology Department at the University of California, Santa Barbara (UCSB), slated to grow into a major University of California campus. He soon improved the department's standards and brought aboard new, research-oriented faculty members who enlarged the department and enhanced its stature. Having launched the department at Santa Barbara into productive growth, he turned an attentive ear to the offered opportunity to build a geological program from the ground up at the University of California's newest campus at Santa Cruz (UCSC). The challenge proved irresistible and Waters transferred to UCSC in 1967, plunging once again into the demanding task of department building. Outstanding faculty appointments were soon made, and—despite the still small size of the group—a Ph.D. program in earth sciences was established under Waters's leadership. By the time of his formal retirement in 1972, the graduate and undergraduate programs in earth sciences at UCSC were flourishing.

Professor Waters's expertise in volcanology opened the way for a new, exciting field of research and teaching during the Santa Barbara and Santa Cruz years: lunar geology and exploration of the Moon's surface. With NASA's Gemini Project to orbit the Moon, and the follow-up Apollo Project to land on and sample its surface, an urgent need arose to learn more about the geological processes that had shaped the lunar surface and operated within its interior. Such knowledge was needed to explore and sample effectively and to geologically train the astronauts who would do the sampling. Waters's preeminent qualifications as a volcanologist soon involved him in several important aspects of this exotic program, including deciphering the record of volcanism and meteor impacts on the lunar surface, the assessment of Apollo landing sites, and the geologic training of the astronauts. His monograph on "Moon Craters and Oregon Volcanoes" stemmed from the Condon Lectureship held by Waters in 1967.

Some lunar craters bore a marked resemblance to terrestrial maar-type volcanic craters, formed by highly explosive basaltic eruptions, but not enough was yet known to compare them effectively. Waters therefore teamed with UCSB volcanologist Richard V. Fisher in a dedicated study of terrestrial maar volcanism at localities ranging from the western United States to the Philippines (Luzon), the Azores (Capelinhos) in the Atlantic, to Germany (Lacher See district).

Thus, his UCSB and UCSC years (1963-1972) involved department building and teaching, the guidance of graduate research, a heavy involvement in lunar programs and astronaut training, the research and publication of terrestrial maar-type volcanism, and the extensive revision and expansion of the *Principles of Geology* textbook, leading to its third edition in 1968.

THE CONSUMMATE PROFESSOR

Although Aaron's election to the National Academy of Sciences (in 1964) and other honors largely reflected his prowess as a researcher, it is his towering reputation as a teacher, particularly of graduate students, for which he is perhaps best remembered. He was rigorous but made learning an adventure and fun, whether in the classroom, the lab, or out in the field.

His classes were rewarding, and his outstanding graduate course on petrogenesis became a rite of passage for many future researchers. But it was his ability to motivate and guide graduate students that earned his reputation as an outstanding professor. He could be tough, yet his genuine warmth and acts of kindness are legendary. Though much of his teaching career predated the years when large research grants tended to tie graduate students to sources of financial support, limiting their choice of mentors and dissertation topics, Waters always had more than his share of good students. Many gravitated to him for guidance, partly because of his specialty fields, partly because of his sterling reputation, and perhaps because of a mystique that drew students to him. One could expect his expert guidance of dissertation projects but with a loose rein that encouraged individual initiative and alternative interpretations, even those that ran counter to his own published work. In summing up his qualities, it is hard to improve on an earlier description by three of his close former colleagues:

> Those who knew and worked with Aaron remember him as forthright and vigorous, a person of enormous vitality and a pointed sense of humor, the latter often marked by a characteristic arched eyebrow. Behind what to some seemed a gruff persona, he was a kind individual, enormously supportive and helpful to students and colleagues. In turn, he expected the best from them in terms of effort and accomplishment. Woe to the individual who turned in slipshod work! Such an event could make Aaron erupt in frustration, leading some students to think that his choice of volcanology as a specialty was entirely appropriate. But students always knew that Aaron had faith in them, often more faith than they had in themselves. Generations of his students have come to recognize that both Aaron and Elizabeth were as devoted to their futures as to the classwork at hand" (Krauskopf et al., 1992).

Another part of Aaron's mystique was an indefinable sense that graduate research was almost a family affair. The research itself was hard-driving, no-nonsense work, yet Aaron was a genial host to his students on frequent social occasions. That one came to feel almost like one of the family at such times was due to the hospitality and warmth of hostess Elizabeth Waters. Aaron and Elizabeth functioned marvelously as a team: He provided the leadership and critical guidance to students while she was indispensable in social backup.

Elizabeth von Hoene Waters was also a native of Washington State, but Aaron and Elizabeth met and courted in California while Aaron was a young prof at Stanford. Elizabeth, an art history major at prestigious Mills College, graduated with honors in the morning of June 10, 1940, and married Aaron that afternoon. They honeymooned at Mount St. Helens, where he was doing fieldwork, and they remained a devoted couple through 51 years of marriage.

The Waters hospitality to his, or rather to *their* students, seemed like part of the deal. And Elizabeth could provide good advice along with her tasty cooking. As one of his grad students, I recall bringing a new girlfriend to one of the Waters' evening socials. Elizabeth took me aside later on: "Don't let her get away," she solemnly advised. There were other considerations of course, but now, after 50 years of marriage to this same lady, I still recall and appreciate the sagacity of Elizabeth's advice and her approval. An endowed fund for graduate research at UCSC is fittingly called the Aaron and Elizabeth Waters Graduate Award.

RECOLLECTIONS

Some humorous—in retrospect—incidents are part of the Waters legacy. Aaron used to enjoy recalling a conversation between two graduate students that he overheard while still a young assistant professor at Stanford. Austin Flint Rogers, the famed mineralogist, had just retired from teaching, and Waters was slated to take over his petrology course. "Well," one student was overheard to say, "maybe Waters can fill Rogers's pants but he can't fill his shoes!" Now, one smiles when recalling that the petrogenesis course that Waters soon developed at Stanford later became famous at four universities.

There was also the time, still in the middle 1930s, when two of the giants of American geology—Bailey Willis of Stanford and Andrew Lawson of the University of California, Berkeley—were seminar guests at the Waters home. Both men, known for their strong convictions and fiery dispositions, were friends but also professional rivals, especially on siting the foundations for the Golden Gate Bridge, for which both were consultants. A hot argument over the south abutment developed between them that evening, culminating in a fistfight in the Waters parlor! The outcome of the fight is not recorded but the controversial south tower of the bridge, resting on the foundation endorsed by Lawson, remains stable 70 years later.

Waters loved to generate controversy and spirited discussions over the outcrops on field trips that he led. The trap was set when he led participants to an outcrop or roadcut where geologic relationships at first seemed confusing. Observations were followed by differing interpretations, some quite vigorous and egged on by Waters's provocative comments. Finally, the group was invited to step farther back, where the broader relationships in surrounding terrain were also visible and revealed the true explanation, sometimes surprisingly simple. He took much satisfaction in the discussions and their outcome, but especially in this pedagogic technique. The field trip was enlivened, and participants learned not to reach conclusions too hastily while their noses were glued to the outcrop.

There were also times when even the most challenging geology did not induce discussions, to Waters's huge disappointment. One such time was a frigid November field trip along the Blue Ridge Mountains in northern Virginia: a strong wind drove sleet horizontally over the ridge crest and long icicles decorated the roadcuts. The Johns Hopkins grad students dutifully emerged from the warmth of the vehicles at each stop but huddled silent and shivering around the outcrops as Waters attempted to get discussions going. Exasperated, he finally exclaimed, "Trying to get any discussion out of you guys is like trying to pull teeth." Later, at the geology department Christmas party, where humorous gifts were exchanged, Aaron unwrapped a small but weighty package, gaily decorated, to find a shiny new pair of pliers. Attached was a gift card that read: "Trying to get any discussion out of you guys is like trying to pull teeth!" It remained a prized memento for years.

THE RETIREMENT YEARS, 1972-1991

Retirement from UCSC came in 1972 but with no letup in the pace of Professor Waters's professional activities. His university teaching continued, as did his geologic research. Most notable was the near-completion of his beloved long-term project on the geology of the Columbia River Gorge, separating Oregon and Washington (1973). He also lent his expertise to federal research organizations as a consultant in volcanology. Only in the last few years of his long and productive life did he finally slow down and go into true retirement.

Aaron Waters loved teaching; it was among the things he was born to do well, and the official act of passing into emeritus status did not change this part of his nature. It did change his opportunities, allowing him to be more selective and to answer new challenges. He taught for a while longer at UCSC but then took teaching appointments at the California State University, Los Angeles, and at the University of Texas at El Paso, each appealing for different reasons. The geology department at California State, in a large metropolitan setting, scheduled many of its classes at night to enable working adults to pursue baccalaureate and masters degrees. The commitment of those students and their extra effort harmonized with his own philosophy. The setting of the University of Texas amid the wide-open spaces of West Texas, its congenial faculty and their commitment to improvement, were also appealing to Waters. Fortunately, he was able to teach his beloved petrogenesis course and lab at both places.

His final major research project, involving several summers of fieldwork during the 1970s, was instigated by the National Park Service. Lava Beds National Monument in northeastern California was a public attraction for both scientific and historical reasons. The park contains voluminous basaltic lava fields, cinder cones, and extensive lava-tube canyons on the northern flank of the Medicine Lake Highlands shield volcano. Part of this rugged terrain encompassed Captain Jacks Stronghold (1981), famous as the core of Indian resistance to U.S. troops during the Modoc War of 1872-1873. The extensive, highly complex system of lava-tube caves within the basaltic flows was obviously a feature of remarkable geologic and public interest, but had never been studied and the Park Service knew little about it. Aaron Waters rectified this deficiency. Still vigorous in his seventies and aided by field assistants Dave Kimbrough and Jamie Gardner, he produced a detailed map and cross-sections of much of the cave system—within ~ 20 square miles of flows—in several summers of fieldwork. This monumental study, combined with surface mapping of the lava flows by Julie Donnelly-Nolan, finally appeared in 1990 as a *U.S. Geological Survey Bulletin*. As the most detailed study of a lava-tube cavern system ever published, it is a fitting capstone to Aaron Waters's long, productive, and distinguished research career.

Waters served also as a consultant in volcanology after retirement from UCSC. As an authority on the Columbia River Basalt, he consulted during the late 1970s for the Rockwell Hanford Corporation, which managed the Hanford, Washington, plutonium-producing facility (situated on the basalt) for the U. S. Department of Energy. He then joined the Geologic Division of Los Alamos National Laboratory as a consultant in late 1979. He was brought to LANL initially to add expertise on basalt studies on the Yucca Mountain nuclear repository project, but also collaborated with Fraser Goff on a lengthy report written to muster scientific and financial support for drilling five magma-hydrothermal systems under the emerging Continental Scientific Drilling Program (1980). The later successful drilling and borehole geophysical measurements at the Inyo Domes (Coso), Valles Caldera, Salton Sea, Geysers-Clear Lake, and Long Valley hydrothermal areas owed much to Waters's early efforts to get those projects launched. He served also as adviser to three successive Geologic Division leaders at LANL, and critically reviewed all outgoing reports and manuscripts.

Waters left LANL in 1983 and moved to Tacoma, Washington. There Aaron and Elizabeth Waters lived in retirement in full view of Mount Rainier and his beloved Cascade Mountains, until his death eight years later.

EPILOGUE

In retrospect, one looks back over Aaron Waters's career with profound admiration. He worked hard all his life, took satisfaction in what he did, and blazed new trails within the realm of geology. He was instinctively a leader, pursuing his goals with conviction and the spirit of adventure, never too concerned by the adverse views of others. His finest hour was as a professor, motivating his students by example, nudging them along with "tough love," and opening new vistas before them. His memory is revered by those who had the good luck to know him and to work alongside.

Many honors came his way, including a Guggenheim Fellowship (1937-1938), election to the National Academy of Sciences (1964) and the American Academy of Arts and Sciences (1966), and the Penrose Medal of the Geological Society of America (1982), its highest award. Those who knew Aaron well will perhaps agree that the honor he treasured most was his role in encouraging and nurturing so many fine young minds, and launching those young scientists upon their own successful geologic careers.

Aaron and Elizabeth's daughter, Susan Waters, provided important facts and recollections of his early years. Fraser Goff, Aaron's research colleague in the Geologic Division of Los Alamos National Laboratory as well as his grandnephew, provided a wealth of information concerning the Waters' "retirement" years (i.e., the post-1972 period not covered by his biobibliography). Their invaluable help, along with that of Don Swanson, is gratefully acknowledged. An earlier memorial by Konrad Krauskopf, Robert Garrison, and George Thompson provides insightful glimpses of Aaron in his roles as professor and as research geologist, including the passage quoted in this account (Krauskopf et al., 1992). Academicians John Crowell and William Dickinson asked me to write this memorial, provided later encouragement, and helped greatly in suggesting revisions.

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Fred L. Whipple

FRED LAWRENCE WHIPPLE

November 5, 1906–August 30, 2004

BY GEORGE FIELD

THE PERSON

When Halley's comet approached the Sun in 1910, Fred Whipple was three years old. When it approached again in 1986, he was 79. By then Fred had a brilliant career as an astronomer and scientific administrator behind him. Over the course of those years he had become the world's leading authority on the nature of comets. His pair of papers on the subject in 1950-1951 had become classics, and the model of comets that they propounded was fully confirmed by space probes that were sent to study Halley's comet in 1986.

I first met Fred in 1955 when I arrived at the Harvard College Observatory (HCO) as a postdoctoral fellow. At that time he was busy organizing the Smithsonian Astrophysical Observatory (SAO), so I did not get to know him well. When I returned to Harvard in 1972, we met in his office, surrounded by models of astronomical instruments that he had designed and built. At that time I was a candidate for the directorship of HCO, which is located in the same group of buildings as SAO, and Fred welcomed me graciously.

The Smithsonian Astrophysical Observatory was a big institution, the home of many diverse astronomical projects, almost all of them initiated by Fred since becoming the SAO director in 1955. His quick intelligence was obvious. His dedication to science became apparent as he described his research on comets. I shared his delight in being able to estimate the magnitude of all sorts of things, from costs of building telescopes to the sizes of astronomical objects. Despite his administrative responsibilities, he was directly involved in interpreting observations of comets. He enthusiatically told me of his latest ideas, and how they were faring. This was a style everyone recognized as Fred's. If colleagues or others were ignorant of comets, he would rapidly introduce them to the subject, and get them interested.

My wife, Susan, caught his enthusiasm, and came to admire and love Fred, finding him enormously entertaining. When he and his wife, Babette (also called "Babbie"), first entertained us at their home, we were startled to find a huge kinetic painting on the wall in the dining room, which morphed from one color pallet to another in ever-changing patterns. In their living room a variety of sculptures, surrounded by flowering plants, set the stage for a sweeping view of Boston. Looking down into the yard, we saw a lovely rose garden tended by Fred.

Talk on those evenings involved astronomy, politics, psychology, and of course, our children. Babbie has a Ph.D. in psychology, and her thoughts on the love and care of children were always worthwhile.

Fred was an energetic and optimistic person. He had played tennis at the University of California, Los Angeles, and loved to do so whenever he could. Unfortunately, he was struck by polio when he was younger, leaving him with one leg shorter than the other, and thus unable to compete at his skill level. But he noticed that there was space for a tennis court on the observatory grounds, and was one of the people who helped build it. Were it not for his disability, he might have been the Harvard College Observatory tennis champion every year.

Fred biked to work six days a week throughout the year, rain or shine. The round trip was 5 miles on busy streets, but Fred kept biking into his nineties. When illness overtook him, his doctors suggested that he walk a mile or so every day to keep his leg muscles working. He walked around the block in Cambridge until he could no longer do so, and then began to walk the corridors of the observatory. Then in his middle nineties, he would walk by the seminar room where we were working, and we all thought, "There goes one tough guy."

Fred loved scuba diving at his and Babbie's home on Great Camanoe Island in the British Virgin Islands. He was known among the divers there as an incorrigible explorer. No sooner would a group of divers reach the bottom, than Fred was off on his own, far from any help should he need it (personal communication, J. Giacinto of Dive BVI, Virgin Gorda, British Virgin Islands, May 31, 2006). For those visitors who did not dive, Fred graciously introduced them to snorkeling, a much less strenuous sport, but still a thrilling way of seeing the varieties of fish and coral that are available.

When he addressed the staff of the Smithsonian Astrophysical Observatory, he did so with a twinkle in his eye. Invariably the staff ended up smiling and laughing. On such occasions he would be wearing a tie from his large collection of comet designs. He ate lunch at Armando's Pizza, where he became such a fast friend of Armando that Armando treated all astronomers who came in as friends of his also. No doubt Armando's attitude toward Fred was affected by the fact that when Fred returned from a visit to the Vatican, he thoughtfully gave to Armando a medal that had been blessed by the Pope.

One of Fred's famous habits was to ask that jam be on the table at home at all times, whatever was being served for dinner. Another was his interest in gadgets. There were several hard-to-solve puzzles in the living room, and I recall his pleasure in demonstrating to me a new acquisition, a shaver activated by a wind-up spring. Somewhat surprising was his fascination with occult phenomena, such as clairvoyance and astrology; he concluded that classical astrology has no scientific basis. His own religious beliefs appeared in an unpublished paper titled *My Conversion to Atheism.*[1]

Fred appreciated the many honors bestowed upon him. He bore them lightly but was justly proud of his accomplishments. In particular, I recall how happy he was to have been chosen as the UCLA alumnus of the year in 1976. In his published interview with Ursala Marvin, a senior scientist at the Smithsonian Astrophysical Observatory, he was reminded that in the year 2000 he had been named a "living legend" by the Library of Congress. Fred's comment was, "I don't feel very legendary, but I am pleased to be still living."[2] Although he was intolerant of persiflage and would dismiss it with a wave of the hand, he chatted easily with everybody, earning the respect of the entire staff of SAO.

THE SCIENTIST

From 1924 to 1927 Fred attended UCLA, where he obtained a B.A. degree in mathematics. At UCLA he took an astronomy course with Frederick Leonard, which he found to be "extremely interesting."[3] He decided to pursue astronomy as a career, but because UCLA had no astronomy department at the time, in 1927, he moved up the coast to the University of California, Berkeley, where he obtained a Ph.D. in astronomy in 1931. Fred studied under A. O.

Leuschner, whose work centered on celestial mechanics, including the orbits of comets.

Fred's membership in the Berkeley astronomy department afforded him access to the Lick Observatory on Mount Hamilton in California, one of the leading observatories in the United States. Within a short time Fred became an accomplished observer. When he completed his Ph.D. in 1931, he was offered a research position at Harvard College Observatory (HCO) by its director, Harlow Shapley. The astronomy department was in the HCO building, and Fred became a faculty member there. Fred demonstrated a strong interest in comets by examining 70,000 plates in the HCO collection of astronomical photographic plates for serendipitous images of comets. He was successful in this search, finding six new comets, for each of which he was awarded the Donahoe Medal of the Astronomical Society of the Pacific.

In this period he was aware of the claim of Ernst Öpik, a famous Estonian astronomer whom Fred admired greatly, that contrary to common opinion not all meteors follow closed orbits around the Sun. Öpik believed that some meteors follow a hyperbolic trajectory that comes in from outer space and goes out again. Meteors are objects that often are ejected by comets and are later seen entering Earth's atmosphere when Earth crosses the orbit of a comet. Thus, studies of meteors have implications for the study of comets. To investigate Öpik's claim, Fred set up a network of cameras that could track meteors as they entered the atmosphere. This called for a fast camera with a wide field of view, with a rotating shutter that would interrupt the trail of the meteor as it streaked across the sky, enabling one to calculate its velocity. Viewing it with cameras located at different points enabled its geometric path to be defined. The results were clear: None of the meteors observed follows a hyperbolic orbit. Öpik disagreed with this result for many years, but in 1959 wrote Fred a letter apologizing for his stubborn opposition.[4]

The Harvard Meteor Project succeeded in tracking thousands of meteors. The physics of the entry of a meteor into the atmosphere depends upon both the atmospheric density at the observed altitudes and the physical properties of the meteor. Separating the two effects in the data was finally accomplished. One important result indicated that the density of most meteors is less than that of water, suggesting a spongy ice composition. Because at least some meteors are known to originate from comets, this raised the possibility that comets also contain ice, an idea that Fred developed much further in two famous papers to be considered more fully below. A very useful result of the meteor project was a table of the density of Earth's atmosphere at altitudes above 100 kilometers, data hard to obtain by other methods.

Fred harbored the hope of measuring the track of a meteorite, that is, a solid body large enough to survive entry into the atmosphere and reach the ground intact (known as a "fall"). Such a meteorite could be analyzed in the laboratory to find its age, type, and composition. By observing its track through the atmosphere one could infer where in the Solar System it originated. To accomplish this Fred established another network of telescopes, this time 16 stations spread over the Great Plains, called "The Prairie Net." A number of near misses ensued until January 4, 1970, when a participating scientist drove out to look for a meteorite in the neighborhood where its track indicated it had fallen. There was snow on the ground, perhaps favorable to spotting a meteorite. In Lost City, Oklahoma, there was a rock in the middle of the road that turned out to be a meteorite.[5] Its orbit indicated that it originated in the asteroid belt, well known to astronomers as the home of countless solid bodies believed to be fragments of a long-gone planet. The Lost City meteorite had a high density, not the low density associated with meteors. Fred realized that because of their low densities, meteors are more likely to burn up in the atmosphere, and are therefore unlikely to reach the ground.

In 1946 Fred joined the Rocket and Satellite Research Panel, a group of scientists charged with advising the Naval Research Laboratory on the best ways to acquire information about space, using rockets acquired from Germany after World War II. The panel also considered the future use of artificial satellites, enabling observations of Earth and its surroundings, and the use of orbiting telescopes, pointing away from Earth, allowing astronomical research. This panel played an important role in space research before the establishment of NASA in 1958. In 1952 a series of articles originally printed in *Collier's* magazine were published as a book, *Across the Space Frontier*.[6] With articles by Wernher von Braun and Willy Ley, this book served to inform the public about the possibilities of science in space. Fred authored a chapter titled "The Heavens Open," which described how telescopes in Earth orbit could open the whole electromagnetic spectrum to observation, including ultraviolet radiation, X rays, and gamma rays.

With his experience in tracking meteors, Fred pointed out to the panel that a network of telescopes should be established to track artificial satellites and compute their orbits. When the National Aeronautics and Space Administration was created in 1958, Fred was ready to take on the responsibility of tracking scientific satellites if and when they should be launched. But that is another story to which I will return below.

Without doubt, Fred's most significant contribution to science was the pair of papers he published in 1950 and 1951 in the *Astrophysical Journal*, titled "A Comet Model."[7] In them he used what he had learned about comets from a study of their orbits, in particular, information gleaned from the fact that the orbits often do not conform exactly to the predictions from strictly Newtonian gravitation. Specifically, both Halley's and Encke's comets deviated from their expected arrival times. Fred realized that material that leaves the comet nucleus to form the fuzzy head must, by the law of action and reaction, exert a force on the nucleus, effectively forming a rocket. Fred calculated the magnitude of the force, drawing on the chemistry of the material, the physics of evaporation, the theory of heat transfer into the nucleus, and the properties of frangible material. Comparing his calculations with the observed deviations of orbits from the Newtonian values, he was able to fit the data only if the comet's nucleus were composed of ices of water and other volatile materials, forming a fluffy matrix in which mineral grains were embedded. His theory fits the data, and thus began the modern era of research into the nature of comets. Fred referred to his model as the "Icy Conglomerate Model," but the press quickly coined the term "dirty snowball," and Fred became famous as its originator.

The nucleus of a comet is too small to image from Earth, so for years Fred's model provided the best concept of what a comet really is. In 1986 mankind got its first look at the nucleus of a comet when Halley's comet approached the Sun. The European *Giotto* mission to Halley's comet found that its size, composition, and surface properties agreed with Fred's 1950 model.[8] In paper II of the series Fred compared his model with what he and others had learned about meteor streams associated with the tails of known comets. As indicated above, many meteors required a low density to fit the observations, agreeing with Fred's conclusion that the nucleus of a comet is made primarily of ice.

The Stardust mission to Comet Wild 2 returned a sample of the comet to Earth on January 15, 2006. Crystalline silicates have been found in the particles that have been analyzed.[9] It will take years to completely analyze the data, and unfortunately, Fred did not live to see it. Babbie Whipple writes, "Fred was deeply interested in this project, which would bring back to Earth samples of the material that formed the Solar System, dating from 4.5 billion years ago, material he believed would answer many big questions as to the source of life on Earth. For example, did it originate in interstellar space (a theory supported by Sir Fred Hoyle) or did it originate here?"[10]

Fred was both an observational astronomer and a theorist, a rare combination nowadays. As time went on, he spent less time at the telescope and in the Harvard Collection of Astronomical Photographs, and more time analyzing data and proposing and testing theoretical models. He was always involved in advancing the art of instrumentation, through endeavors such as the Harvard Meteor Project and the Prairie Net.

Fred choose to focus his research on comets and meteors. Other leading astronomers of his generation chose to study stars and galaxies. Although Fred began his career analyzing cometary orbits, his Ph. D. thesis at Lick Observatory, supervised by Donald Menzel, was on variable stars, and when he arrived at Harvard in 1932, he started to work on galaxies. But "I soon learned that [Harlow] Shapley considered galaxies to be his own topic and he did not care to have any competition."[11] As new techniques were developed, the physics of planets and comets posed challenging questions.

Fred anticipated this development by focusing on the physics of comets: Their tails could be studied with current telescopes, and the meteors they left behind could be studied as they fell toward the ground.

Everything changed in 1958 when President Kennedy decided to send a man to the Moon. Up until then few scientists studied the Moon. Suddenly large sums were available from NASA to study its surface and plan experiments that astronauts would carry out. NASA's contractor at Caltech, the Jet Propulsion Laboratory, initiated unmanned missions to the Moon, Venus, and Mars, and later to the outer planets, and still later to comets and asteroids. It does not take a rocket scientist to see that this activity would be a boon to Solar System science. When a spacecraft encountered Halley's comet in 1986, Fred lived to see the object of his 1950 research become the target of a major scientific and engineering effort.

It has become increasingly apparent that there are excellent scientific reasons for studying comets as part of an effort to understand the formation of the Solar System. Moreover, many planetary systems beyond our Solar System have been found recently. From the time that Immanuel Kant formulated the nebular hypothesis, astronomers have conjectured that the planets formed from a disk of gas and dust orbiting the Sun. Self-gravitation acting on an interstellar cloud drew the material of the Sun together, but any material that had large angular momentum was left behind to form a disk. Study of such disks orbiting other stars has proved that this idea is correct.[13]

Comets provide an important test of this picture. Their orbits, unlike those of planets, do not lie close to a single plane, and do not all revolve in the same direction. They travel vastly larger distances than the planets do. Fred showed that they are composed largely of ices, which could not survive if they were formed close to the Sun. Thus, they may have predated the formation of the Sun, and thus, may represent a sample of interstellar matter that escaped incorporation into the Sun or the planets. I believe with Fred that the study of a comet sample in the laboratory is of greatest importance.[14]

THE ADMINISTRATOR

Many astronomers probably knew of Fred as the director of the Smithsonian Astrophysical Observatory. How did it come about that SAO is collocated with Harvard College Observatory? SAO is part of the Smithsonian Institution in Washington, D.C., best known as the quasi-governmental organization that runs museums like the Natural History Museum and the National Air and Space Museum. But it also supports research on many topics, including meteorites, the history of art, tidal estuaries, and at SAO, astrophysics.

It all started when Samuel P. Langley was appointed secretary of the Smithsonian Institution in 1887. A former physics professor, he was interested in measuring the infrared radiation from the Sun—a major contribution to the total solar energy—and in 1890 established a laboratory for that purpose called the Smithsonian Astrophysical Observatory, or SAO, with Charles C. Abbott as the director. Using ground-based instruments, Abbott measured the energy emitted by the Sun, and concluded that it varies over time by as much as 1 percent. Measurements from spacecraft have since shown that the emission does indeed vary, but only by about 0.1 percent. Abbott succeeded Langley as secretary of the Smithsonian, continuing to support SAO as a bureau within the institution, funded under the annual federal appropriation to the Smithsonian.

When Leonard Carmichael became secretary in 1953, he wanted to expand SAO's mission to encompass the major fields of astrophysics and discussed this idea with Donald Menzel, the director of the Harvard College Observatory (HCO).[15] Menzel suggested that SAO be linked with a university in Washington but later concluded that HCO would be a better match. When he proposed this to his HCO colleagues, including Fred Whipple, they responded enthusiastically. In 1955 Harvard University and the Smithsonian Institution signed an agreement to move SAO to Cambridge, and Fred was appointed director of SAO, a federal civil service position, while remaining a Harvard professor.

What happened next was unexpected. As a member of the Rocket and Satellite Research Panel, Fred had proposed a plan to establish a network of stations to track any artificial satellites of Earth that might be launched. Various nations, including the United States and the Soviet Union, proposed to launch such satellites in support of the International Geophysical Year, sponsored by many nations in 1957 to improve our knowledge of Earth. Fred proposed to the U.S. government that SAO establish an optical satellite tracking network. This required a rapid expansion of SAO staff from 5 to 500. Overnight SAO became the largest observatory in the United States.[16]

As luck would have it, the Soviet Union surprised the world by orbiting *Sputnik I* on October 4, 1957. The automated SAO network was not ready to track *Sputnik* until October 17, but thanks to Fred's foresight, SAO had set up a backup network staffed by volunteers with small telescopes and stopwatches who recorded enough information for SAO to find *Sputnik*'s orbit within four days. Fred's photograph appeared on the cover of *Life* magazine when the press learned of this feat. The Satellite Tracking Network was soon up and running, and providing precise information to

NASA. The network continued into the 1970s, providing information on the shape of Earth and the density of its atmosphere.

Fred was very interested in using space technology to put a telescope into space. Under contract with NASA, he organized the first Orbiting Astronomical Observatory (OAO), which successfully reached orbit on December 7, 1968. Unfortunately, its batteries failed, and little data was obtained. However, the two follow-on missions OAO II and III succeeded in gathering large amounts of information about both stars and diffuse matter between the stars.

Fred always aspired to have a ground-based observatory at SAO. Recognizing that the skies of Massachusetts are too brightened by city lights for it to be located nearby, he settled on a site at Mount Hopkins, Arizona. Believing that it was important to have an instrument there as soon as possible, in 1968 he commissioned a telescope of novel design to detect the tracks of incoming high-energy gamma rays by means of the Cerenkov radiation from the particles in their wake. This facility succeeded in detecting astronomical sources of such radiation, one of which turned out to be a massive black hole. Believing that understanding the source of such radiation may require new physics, physicists are now constructing a number of similar telescopes.

In 1970 Fred also built a 1.5 meter optical telescope on Mount Hopkins in collaboration with the University of Arizona. This telescope was used to conduct the first major red shift survey of galaxies in 1983.[17] The observed red shifts of galaxies were used with Hubble's law of the expansion of the universe to find the galaxies' positions in three dimensions. When a slice of the sky was plotted, it was apparent that galaxies form walls and filaments surrounding apparent voids, a great surprise at the time. Theorists have now reproduced these results by using computers to follow small perturbations in the density of matter in the Universe as they grow by mutual gravitation to form galaxies. Thus, one of the great discoveries in cosmology was made with a telescope that Fred initiated.

Fred wanted to build a larger telescope. He concluded that to keep costs down, one should use many smaller mirrors, each focused on the same point, rather than a single large one. Thus was born the MMT, or Multiple Mirror Telescope, a joint SAO-University of Arizona project, completed in 1979. In it, six 1.8 meter mirrors were combined to form the equivalent of a single 4.5 meter telescope, at the time one of the largest in the world. The principle having been established, two 10 meter Keck telescopes on Mauna Kea were later built, each with 36 mirrors. Fred could be proud of his participation in this revolution in telescope design.

BIOGRAPHY

Fred was born on November 5, 1906, in Red Oak, Iowa. In his words, "As an Iowa farm boy, I contracted a case of polio, and it prevented me from becoming a professional tennis player. When I entered UCLA, it was my main ambition to excel at tennis . . . but I never made the tennis team."[18] Before attending UCLA, Fred moved with his family to Long Beach, California in 1922, where he attended the Long Beach High School and worked in his family's grocery store. In 1923-1924 he attended Occidental College and from 1924 to 1927 he attended UCLA, where he received a B.A. degree in mathematics. When he decided to pursue astronomy as a career, he enrolled in the graduate program at UC, Berkeley, obtaining a Ph.D. in astronomy in 1931. His thesis on variable stars was supervised by Donald Menzel, who at the time was an astrophysicist on the staff of the Lick Observatory of the University of California, and who later became director of the HCO. Fred was invited by Harlow Shapley to join the staff of HCO and to take charge of its Oak Ridge Station; appointed instructor in the astronomy department, he moved up through the ranks, attaining the rank of professor in 1950 and becoming department chair in 1949. He was appointed Phillips Professor of Astronomy in 1968, serving in that position until his retirement from the faculty in 1977. His colleagues at Harvard included Shapley, Bart Bok, Cecilia Payne-Gaposhkin, and Donald Menzel. His first marriage, to Dorothy Woods, in 1928, ended in divorce in 1935; they had one son, Earle Raymond Whipple. Fred married Babette Frances Samelson in 1946, and they had two daughters, Dorothy Sandra ("Sandy") Whipple and Laura Whipple.

Fred served on advisory committees to the House Committee on Science and Astronautics of the U.S. Congress, to NASA, the International Geophysical Year, the National Research Council, the U.S. Air Force, the National Science Foundation, the National Advisory Committee on Aeronautics, the Office of Naval Research, and the University Corporation for Atmospheric Research. He was a member of the International Astronomical Union, the International Scientific Radio Union, the Committee on Space Research, the International Astronautical Federation, and the International Academy of Astronautics.

Fred was a member of many honorary societies, including, of course, the National Academy of Sciences, to which he was elected in 1959. Others included the Royal Society of Arts (London), the American Academy of Arts and Sciences, and the American Philosophical Society. His professional societies included the American Astronomical Society, of which he served as vice-president from 1948 to 1950, the American Astronautical Society, the American Geophysical Union, the American Institute of Aeronautics and Astronautics, the American Rocket Society, and the American Standards Association.

His many honors included Donohoe medals (in 1933, 1934, 1937, 1941, 1942, and 1943), a Presidential Certificate of Merit (for "Window," a radar countermeasure used by the Air Force in World War II), the J. Lawrence Smith Medal of the National Academy of Sciences, the Space Flight Award of the American Astronautical Society, the Distinguished Federal Civil Service Award by President John F. Kennedy, the NASA Public Service Award, the Leonard Medal of the Meteoritical Society, the Kepler Medal of the American Association for the Advancement of Science, the Joseph Henry Medal of the Smithsonian Institution, the Gold Medal of the Royal Astronomical Society, the Kuiper Award of the American Astronomical Society, the Bruce Medal of the Astronomical Society of the Pacific, the Henry Norris Russell Lectureship of the American Astronomical Society, and finally, the establishment of the Fred L. Whipple Lectureship of the Planetary Division of the American Geophysical Union.

From simple beginnings Fred became a world authority on the nature of comets. When he started his career, many astronomers were ignorant of the subject. Now, largely as a result of Fred's work, we realize that comets carry unique information about the formation of the Solar System. Fred was an observer, an analyst, and a theorist, scientifically active in spite of heavy administrative commitments. He carried his physical disability without complaint, and he continued his commitment to rational thinking into every sphere he encountered. In short, he was a person that every scientist can admire.

IT IS A PLEASURE TO acknowledge conversations with Ursula Marvin, my colleague at the Smithsonian Astrophysical Observatory. Brian

Marsden at SAO graciously helped me to select Whipple's most significant works, reviewed a draft of the memoir, and made many helpful comments. I am indebted to Babbie Whipple for her comments on the manuscript.

NOTES

1.F. L. Whipple. My conversion to atheism. Personal copy of G. Field.
2.U. B. Marvin. Oral histories in meteoritics and planetary science. XIII. Fred L. Whipple. *Meteorit. Planet. Sci.* 39(suppl.)(2004):A199-A213.
3.Ibid.
4.Ibid.
5.Ibid.
6.C. Ryan, ed. *Across the Space Frontier.* New York: Viking Press, 1952.
7.See "Selected Bibliography" (1950, 1951).
8.U. B. Marvin. Oral histories in meteoritics and planetary science. XIII. Fred L. Whipple. *Meteorit. Planet. Sci.* 39(suppl.)(2004):A199-A213.
9.D. S. Burnett. NASA returns rocks from a comet. Science 314 (2006):1709-1710. (See also other papers in this issue.)
10.E-mail message dated Jan. 11, 2006, from Babette Whipple.
11.U. B. Marvin. Oral histories in meteoritics and planetary science. XIII. Fred L. Whipple. *Meteorit. Planet. Sci.* 39(suppl.)(2004):A199-A213.
12.G. Marcy, R. P. Butler, D. Fischer, S. Vogt, J. T. Wright, C. J. Tinney, and H. R. A. Jones. Observed properties of exoplanets: Masses, orbits, and metallicities. *Prog. Theor. Phys.* 158(suppl.)(2005):24-42.
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14.Personal communication from Fred L. Whipple; as indicated in Note 9, this wish has already been granted, although Fred did not live to see the sample returned from Comet Wild 2 by NASA's Stardust mission. The crystalline nature of Stardust particles suggests to some that these particles cannot be unprocessed interstellar dust, as might be expected if Comet Wild 2 formed in the cold outer reaches of the solar system. However, lively debates will ensue before this issue

is settled. If Fred were now alive, he would surely be an active participant in the discussions.

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